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ROUND TABLE DISCUSSION
SOME THOUGHTS ON CALIFORNIA ARCHAEOLOGY:
AN HISTORICAL PERSPECTIVE
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AN HISTORICAL PERSPECTIVE

FRANCIS A. (FRITZ) RIDDELL, SESSION CHAIRMAN
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Cover Illustration: Jeremiah B. Lillard, President of Sacramento Junior College, during excavations at Lompoc, Easter Vacation, 1935
INTRODUCTION
FRANCIS A. RIDDLE, SESSION CHAIRMAN

This final session of the 16th Annual Meeting of the Society for California Archaeology [Sacramento, March 31 - April 3, 1982] has multiple parentage, and is, in fact, an extension and broader elucidation of Arlean Towne’s thesis "A History of Central California Archaeology (1880-1940)." It was Norman Wilson, however, who came up with the idea of hosting a group of "old fossils," although he did not use those words. The idea sounded good to the membership of the Central California Archaeological Foundation (all ten of us), so my being recognized as the official California State Fossil, I was asked to put this fish-fry together. I was able to generate quite a bit of enthusiasm and personal commitment from a sufficient number of senior archaeologists to be able to put together this present panel.

My "research design" for this session is predictably simple:

a) to allow the younger archaeologists in California to have an opportunity to see, hear, and interact with the "founding fathers," so to speak;

b) to allow the "founding fathers" to reminisce and/or make substantive comments and observations on the state of the art; that is, where it’s been, where it is now, and where it may (or should) be going; and

c) to allow the next younger generation, or "semi-fossils" to respond to the comments of the older, or "fully fossilized" founding fathers.

As a baseline or take-off point for these discussions, the participants have been provided with Heizer’s "Some Thoughts on California Archaeology at the Moment," a public lecture given at UCLA on April 10, 1975, and published in Vol. 1, No. 1 of the Journal of New World Archaeology. This public lecture presented Heizer’s thoughts in an interesting and provocative way. Though it, we may experience his thoughts on the subject despite the obvious fact he cannot be here.

Although I am not here to be Bob Heizer’s spokesman, there is no doubt about his both early and late contributions to California archaeology. Because he cannot be here, however, I would like to read several paragraphs from his article in Gordon Willey’s Archaeological Researches in Retrospect (1974) titled "Studying the Windmiller Culture:

When I began college in 1932 there were no archaeology lecture courses taught, and no instruction in how to excavate. Archaeology, as we understand it today, was a non-subject. At Sacramento Junior College where I had to spend two years making up courses which were not taught in the seventy-student Nevada high school I had attended, I learned something about scientific excavation from the president, J.B. Lillard, and a very good archaeologist, Richard van Valkenburgh, whose support was being managed with a part-time job in the college library. The Depression was on, and van Valkenburgh felt himself lucky to have this much employment and at the same time to be able to practice some archaeology. Having removed my (academic) deficiencies, I went off in 1934 to Berkeley as a junior and there learned more about excavation, through digging in local shellmounds, with Waldo Wedel and Philip Drucker, where were already graduate students. Alfred Kroeber, head of the Berkeley Department, was not
exactly against his students doing archaeology, but you could not at the same time say that he was wildly enthusiastic that they should engage in this pursuit. There were still old Indians to talk to and there remained some big gaps in the record of tribal ethnographies for California, and Kroeber was clearly more interested in training his graduate students by having them spend a summer talking to the few surviving Tolowa or Shasta or Atsugewi than he was in having them indulge themselves by digging up skeletons and arrow-points.

Of the four sites we have been talking about, Sac-107 has been nearly destroyed by the ranchowner to secure fill and provide a level storage area for farm machinery which seems to have been bought only to abandon and turn to rust; SJo-142 was ripped out years ago to make a rice field when that crop was bringing good prices. SJo-56 has been mostly dug out, but something probably remains to be excavated, and the same is true for SJo-68, which is threatened with final destruction by a large canal which will help move surplus Mokelumne River water to thirsty Southern California. In the latter case, it is "One man's drink is the other man's archaeology". Two additional Windmiller culture sites (Sac-168, SJo-112) have been discovered and excavated since 1950, and they add a bit to our total knowledge.

However, it seems probable that the most obvious and best of the Windmiller culture sites have been already found and excavated. Most of these are now so completely dug over (SJo-56), destroyed (SJo-142, Sac-107) or threatened with imminent destruction (SJo-68) that we probably have in hand nearly everything we will ever know about them. The prehistory of the valley floor of Central California has been essentially destroyed by cultivation and urban development. I believe that the most useful activity I have engaged in in this area from 1932 to 1947 was to dig these sites. When I say that it is not with any sense of pride which comes from a job well done, since much of what I did is quite inadequate by modern standards, but because we saved at least that much from the bulldozer. What we found is safely stored in the Lowie Museum of Anthropology, and our notes and maps are deposited for the use of students still to come. And they will come, because for much of California the archaeology for future study lies not in the ground but in museums and archives.

Surely there are at least a few more Windmiller culture sites, and when these are found they should be either protected from disturbance, or if that is not possible to guarantee, they should be excavated with the greatest care and attention due 'an endangered species.' Anyone in a position to carry out such a project will have read the spate of recent literature on 'processual archaeology,' 'research design,' 'archaeological strategy,' and the like. I believe that there is enough detail now known about the nature of Windmiller sites and their contents already excavated to allow anyone with proper training to do the right kind of job by today's standards when another Windmiller culture site comes to light. This future work, which I confidently expect will come, will be differently prosecuted than the work we did in the latter half of the 1930's and early 1940's. Our investigations were done with practically no financial support at all, and we were essentially untrained. We used our own cars (mine was a 1926 Dodge sedan; Alex Krieger had a later and better car—a 1930 Lincoln touring model) and we lived on very little cash plus large contributions of food supplies (at night) by neighboring farmers whose acquaintance we avoided in the daytime. While I say this in a kind of Proustian sense of recollection, I leave you with the hope that practicing archaeology will be as much fun and be
accompanied by as much feeling of accomplishment for today's reader as it was for me at a young age.

With these brief introductory statements, let us begin this session with open minds and good will. Let us recognize the need for temporal perspective and understanding in California archaeology. Let us recognize the value of differences of opinion, but let us also recognize that California archaeology has a very great need for both the oldtimers and the newcomers, as well as for the professionals and the avocationalists. Let us all work together to heal those breaches which have plagued our society now for a number of years. We must move forward with renewed vigor and with high academic aims. We must set aside politics and do a better job of attending to the professional archaeological needs of California.
Excavation Crew at CA-SAC-66 (Deterding Site), June 1938. "The Fateful House" structure burned destroying field records, some collections, and one pet dog. Left to right: Franklin Fenenga, Sacramento Junior College; W.C. Massey, Joe Ben Wheat, and Gordon W. Hewes, all from U.C. Berkeley. (Photograph courtesy of Franklin Fenenga.)
When Fritz Riddell was searching for his living fossils, he certainly found one, because my experience as an archaeologist was very brief and goes back roughly two generations to the early thirties and before.

When I was in high school, a friend and I became first interested in the subject and sought out some of the local weekend archaeologists or diggers to see their collections and talk to them about what they'd found. Also, we visited the beginning of the State Museum which was Hathaway's private collection hidden away on the second floor of the old State Capitol. To the uninitiated, this was a very interesting group of material, especially I recall one individual squatting in its glass case literally sprouting arrowpoints from every bone. And I think we were told that there was a little manipulation there, and some of these weren't originally found in that manner. But, anyway, it made a very exciting exhibit for us, and there was quite an accumulation of skeletal material which apparently disappeared over the years. I hope this has been retained or kept somewhere. But, of course, now for various reasons an exhibit of this sort is not available to the general public.

We also sought out Henry Gibbs, who had quite a collection, and Schultz Martine and Elmer Dawson down in Lodi, in this area. Some of this material was published, but the others were basically collectors and in the business of finding artifacts and selling them, which, in those days, apparently was acceptable. In fact, as Heizer remarks, archaeologists were then considered a little strange and the weekend foray business was certainly looked down upon no more than weekend golf, for example. It was just another form of recreation. And we also visited a young man in Folsom. I have a clipping here from the Sacramento Union some time ago. It says, "Folsom youth, now college student, started a collection at the age of three. He has a museum all of his own. Valuable Indian relic collected by Louis Payen." I don't know if Dr. Payen is here, but this is a clipping from — I don't think we've met for 50 years. And if he would like to have this clipping, he certainly would be welcome to it.

And we finally entered Junior College, so-called, at its inception of the archaeological program. I have another clipping here which Mr. Riddell might like. It says, "J.C. class reconstructs archaeological history." And a photograph of van Valkenburgh and someone else from the Sacramento Union about 50 years ago. To illustrate a point here, apparently the person who wrote the article wasn't too familiar about archaeology. They talk about "Art Effects." "Art Effects recovered by a painstaking process." It was an interesting experience. Of course, it was just getting off the ground then. And, as you know, Heizer is one of the students; van Valkenburgh and Walker, from what I've learned today, were employed as librarians, apparently, in order to obtain their services. We worked first on the Augustine Mound out near Sloughhouse, later on the Hicks Mound down the river, and the Hertzog Mound during that first season.

I was rather surprised to see that Mr. Zallio was rather brushed aside at this time. There must have been some feeling between Zallio and Lillard who took over the field work, even though he was president of the college and probably might have had something else to do, but apparently that was his bag at the time. And Zallio, of course, had been one of our weekend archaeologists or diggers on his own prior to this period and had his own collection. Apparently because of his activities, it was felt that someone else should handle this particular program. So he was left on the outside. I felt rather sorry for him at the time. He was brought into the program a bit more in later years from what I saw on
the panel [of photographs] outside. But, at the time, in '32 and '33, he had very little to do with the program and the fieldwork.

After graduating, or going on from Junior College, we went down to Berkeley where Heizer and I were roommates. And, of course, we had a fieldwork program there. The most interesting experience was possibly the trip to Alaska in the summer of '35. Heizer had been with Hrdlicka the previous summer, working on the shellmound on Kodiak Island, and they had started excavation there. During the fall of '34, Hrdlicka came out to Stanford to give a talk. We went down there to meet him, and, of course, volunteered or were enrolled as students for the next season — the summer of '35 — digging in the shellmound on Kodiak.

We went up in the spring on an Alaska packer boat, a real old tub which was later sunk in the South Pacific during World War II — it was called the "Curacao" — and worked part of the way up as longshoremen or unloading supplies and loading canned salmon back onto the boat at the different canneries on the way to Kodiak. Apparently, they hadn't heard of Harry Bridges at the time, because we were allowed to do so, along with regular seamen. The shellmound was on the north coast of Kodiak on Larson's Bay, and was quite extensive; went right down to the water's edge. In the previous season, they'd made a cut working back from the water's edge into the face of this mound. We continued working on this face the season we were there, the summer of '35. Hrdlicka was apparently primarily after skeletal material. And the procedure would probably shock most of you today, because we worked the face of this cut, with pick and shovel just like coal miners more than archaeologists. We would drop this material down off this face, including all the assortment of brush on the top, shovel it into mine cars, and run them down to the beach on rails and dump this material into the ocean.

We were provided with three valuable archaeological tools, A black pencil — anything you saw fall out of the upper layer, which was Aleut supposedly, we would mark with a black pencil. Anything coming out of the middle layer we'd found or guessed had come from the middle layer as we shoveled things in the mining car, we would mark with a red pencil. Anything out of the lower layer, lower level, roughly, we would mark with a blue pencil. That was the extent of the recording on this site.

Of course, again, Hrdlicka was primarily a physical anthropologist and the skeletal material was what he was after. In fact, we had heard while we were living at the cannery — the Alaska Packer's cannery — that there was a cemetery across the bay where Oriental employees had been buried. And one day, Hrdlicka took a few of the fellows — I didn't care to go along — over to the cemetery and disinterred an Oriental ex-employee, cleaned him up and brought him back, and boxed him in with all the other skeletal material. Well, we didn't advertise this around the cannery, because there were a great many Orientals employed there and they might not have been too enthused about this. We finally completely leveled the site. Working, again, against the face, and dumping it into mine cars and tracking it down the beach. These mine cars were quite heavy, and we raced them down to the end of the dump where we had a log nailed across the end of the track. Sometimes in our enthusiasm, we would shoot these things down so fast they'd hit the log and bounce off out on the beach. Of course, this would take the whole crew to get them back on the track again. This wasn't always accidental. Sometimes it brought a little relief from the shovel work. And we didn't have a fourth pencil, but we should have had one because we needed it when the tide was out. We'd walk the beach and pick up things which we overlooked in the shoveling. So, we weren't able to determine exactly where these had come from.

And, finally, of course, the entire site was leveled and the skeletal material was packed up and sent back. I assume that the artifacts, as such, were also boxed and
But we had to leave before Hrdlicka finished his work there because our last year at Cal was taking in.

Well, that was just about it as far as my archaeological experience goes. The following year, in '36, I graduated. We had a family business which had been going on for many years, and I had a decision to make. I felt an obligation to go with the business, even though I've regretted it many times. And you might say that I've done a little bit to preserve some early California history, because the business is now 132 years old. So, that was the end of my archaeological career. And I've spent it otherwise during the last few years, but without a shovel.

RIDDELL: Well, that shows that we can make a lot of progress in California archaeology with a beginning like that. And I think we have. I want to thank Brad for the clippings. I assume you want us to archive those clippings. And he also has some of the other material and the photographs; he has a photograph album. I might say one thing about photographs. We put these photographs together out in the lobby rather hurriedly, but we canvassed various oldtimers for pictures of people. And maybe Frank or some of the other people will respond to that. They didn't take pictures of people because of the cost of film and processing; the camera was almost kept in a velvet box, you might say, under lock and key to take one picture at a time. Now, we don't think anything about taking photographs — miles and miles — most of 'em, 90 percent probably, useless, but we sure do burn the film, color and everything else. But it's hard to believe that there was no color film and film was so expensive. Well, it wasn't so expensive, you didn't have the money anyway to buy it. But these are the kinds of things that a lot of you who hadn't thought about it can see why we don't have some of these photographs.

Thank you very much, Brad. Next we'll have Alex Krieger give us a presentation and go on from there. Alex...
Robert E. Heizer, CA-SAC-126 (Augustine Mound), ca. 1934. (Photograph from the McKee Collection.)
ALEX KRIEGER

My instructions were to read this lecture that Heizer gave at UCLA in 1975 and to make comments on it. I'd rather do this other thing. I don't think I can add much to what Bob said at that lecture. He and I were classmates at Cal in 1935 and 36, and we graduated together. Kroeber was the only person around who was interested in archaeology. He gave it every encouragement, but no funds, no tools, or anything. We worked on weekends, summer vacations, lived off the country. Stole chickens and turkeys, sometimes, Fished in the Sacramento River. Eels, rabbits — and I had two uncles up in Glenn County who had farms and orchards. And they gave us things — fruits and eggs. But I think it was good for us. It made us realistic about the difficulties, and I wouldn't trade those days for anything.

As for Heizer's lecture, he makes a number of very interesting points. I won't repeat them because they are in print. Now, for a long time now, many years, I have asked myself this question: whether archaeology is a science? And it might sound laughable to you, but I don't think it is. I think it has a long ways to go. What I'm talking about is the lack of a common terminology for the things that we've found, for the cultural units that we try to define. I'm going to call that a tradition. I don't care what you call it. What difference does it make? So you get people from two or three or four universities in the same city or the same state, using different sets of terms. I talked to a lot of Europeans and Latin Americans about this, and they are profoundly puzzled. Why don't you people speak the same language? You can compare that with the biological sciences, the concept of a species or a family or an order, something like that. There are arguments in those professions. But at least they have a basic realistic language and they understand what they mean. Archaeology, no. You can take exactly the same objects from ten different states and get ten different names. This is what I call it; I don't care what you call it. So you wade through the literature, and you try to decide, what are they talking about? Is it a scraper, a knife, or a fist axe? Even grinding tools. You have to realize that there was no terminology a long time ago and none is applied to any kind of grinding implement. Now the mortar and pestle is one thing. The metate or two-handed mano or something else, connected with agricultural people for the grinding, primarily, of corn. If you travel to Mexico or some other country, they say what do you mean by these words? So I try to explain and scribble on the blackboard. That's not what so-and-so said last time, either.

Now, one suggestion I have to make is that workshops be established everywhere in this country for the purposes of language and typology. In the late forties and early fifties, I managed to get a few grants of money for typology workshops in Austin, Texas, and Norman, Oklahoma; Lincoln, Nebraska; Denver and Boulder, Colorado. So a few people set our for a week, two weeks, whatever time they could spare. And the grant was for their living expenses and travel, and they were to bring sample specimens from their excavations. Tables were laid out, rows and rows of artifacts all labeled with the type names on them. So we would try to reconcile the differences. The last time this happened was in Pocatello in 1961, and after that I gave up, temporarily I hope. Why? Because there was a lot of enthusiasm, a lot of exchange of ideas. This has been really worthwhile. Everybody went back home and used the same names that they'd always been using in conversations, in publications. I don't see much solution to that problem unless a number of people are ready to get serious about it.

Oh, yes, once in Pocatello, that turned out to be half of the workshop and half of the users under a program of papers read to one another. There were only a dozen people there,
but they read papers to one another because that's the way that we do things in this country.

I say that we are not yet a science for a number of reasons. And the lack of a common language is one of them. Terminology for artifact types, fossils, anything you find, is not uniform, and I don't think it will be for a long time. Now, I would say that anybody can count. That doesn't take much sense. You can count the number of objects on each level of your excavation — that doesn't make good scientists. Does being careful make you a scientist? Not necessarily. So I leave that question open to the present. I'll not spend too much time on it. Sincerity, dedication, yes, that applies to all of us. It still doesn't make us scientists.

Now, Heizer spends about half of his lecture on the subject of salvage archaeology. It was very interesting, what he writes there, what he spoke about at UCLA. And one thing I noted, page after page, he deals with public archaeology and public agencies — state agencies, federal agencies — but it's all public. Salvage archaeology. That reminded me that sometime in Washington, D.C., and other places, I was on a committee to write laws for a certain state legislature or for the federal government. And we would spend days, days, and days getting the wording just right with legal advice. It should be against the law to do such and such, to touch any kind of property on public lands. And that means the National Forests, the National Parks and Monuments, lands owned by BLM, any federal land you could think of — highway rights-of-way. Now, these millions of dollars that come to us — and I would ask this question around the table — who is going to enforce these laws? Supposing they do become legislation? Who then enforces them? Who can stop the looters from getting into those places and helping themselves? I don't know. Maybe we'd better talk about that for a little while. Well, who?

And I made a note late last night saying that I think, perhaps, the state and the federal monuments are the best off places because they are patrolled and guarded, and you can be arrested for littering, fined for dumping garbage, or camping in the wrong place. And they'll get you for that. But at least they have the personnel to watch over that piece of land, sometimes around the clock. Three sets of people just go everywhere checking up on the campers. Any you have people that enforce the game and fishing laws, too. That's one possibility. And we took that up in several states. We don't have the personnel, we don't have the time to do that sort of thing. And even if we made an arrest now and then. Then what? It would be taken up in court and the judges would probably toss it out as of no consequence whatever.

You can take it from there with your own imagination what would happen. The national parks are pretty well patrolled, but there are vast regions of wilderness that are not patrolled by anybody. Fortunately, there are not very many archaeological sites in national parks, either. Then we talked about state patrol. Any you won't catch one of them 50 feet away from his automobile on the pavement. Now, use your own imagination about how any kind of legislation about looting our antiquities can be enforced. And I mean with fines, or jail terms, or both. It will be a long time before we get around to it.

Now, on the other hand, we have public situations; I mean to say private situations. Archaeological sites on private land. Once, a long time ago in New Mexico, I was called a communist because I advocated the state law of New Mexico against looting on private property as well as public property. So, I was a communist and wrong in the head.

Now, after I went to the University of Texas in 1941, and it took some years to get used to those people, especially ranchers, farmers — way out in the cattle country. And they are kind of strange people in some ways. And they have their customs and traditions, and they can spot in five seconds that you don't know your way around. And I can remember helping to set up local archaeological groups, societies, in the larger towns. And during this 15-year period, when I was able to, I would get out to 25 or 30 different towns,
This was a good experience. And it cost nothing, because they insisted that I stay in their homes, and sleep in their homes, eat with them. And we got to be close friends. Now, I think, this is extremely important for the sake of salvage of archaeological remains. That is, to have local groups who are intelligent and dedicated and they watch every square mile around it — 50 miles, 60 miles — around town. They know where the sites are. And if you teach them properly, they know how to draw maps, how to measure the place, estimate the number of man hours it would take to explore the site — an open site, or a cave, or a rockshelter, or whatever it is.

Last night I remembered one thing that happened in San Angelo, if you know where that is. It happened in brush country, more or less on the western side of central Texas. And there was a very rich cave deposit there and it belonged to a rancher. And I was concerned about it. I spoke to this group in San Angelo about sort of patrolling that place and keeping the looters out of it. In a few minutes, one of these enormous Texans — 6 feet 6 and 300 pounds — just banged me on the back and said, "Don't worry about a thing, Doc. If anybody touches that place, we'll just beat the shit out of 'em?" I think they mean things like that, too!

Very, very interesting all around. Now, what does that have to do with you? I don't know what sort of organization you have throughout California, whether you've tried to train amateurs at all, but I...

Riddell: That's a very broad question, of course. And there are a number of local societies who are doing an extraordinarily good job. And I agree with you, that more of it needs to be done. And, so, it's not being neglected, but more should be done, and can be done and, I am sure, will be done. Of course, the Society for California Archaeology has that as one of its aims... to involve avocationalists. And this is one of the new -- the president of the society [Gary S. Breschini] -- this is one of his platforms, and his program is to be more progressive in that respect.

Krieger: You have to weed out some people, there are always some people that are nuisances, and you have to...

Riddell: That's right!

Krieger: Now, Heizer says many things about salvage archaeology, and I quote one sentence, and he says, "Every archaeological site is a salvage operation." Not very good, because he meant not only residue highway salvage and urban expansion salvage, but privately owned land, as well, which is going to be looted, or had been looted, or will be next week or next year. In a sense, that proves every site we know of is a salvage operation.

Now the next thing would be how to speed up the fieldwork. And I'll tell you frankly, I've gone insane sometimes just watching people picking over every pebble in the ground, and every chip has to be examined. And this is not recent, is it? Because in the late thirties, the early forties, I went to several of the Pecos Conferences in the Southwest. At that time, whole flocks of easterners would come out of their universities, cities, and have a nice summer vacation in the Southwest, and get credit there. And I saw a place where thirty students would form a field crew and in eight weeks, more or less, they would clear one house floor, or maybe just a part of a house floor, because they were being so careful. Now, if you're going to do salvage, you have to hurry up. We don't have the rest of the century to save such-and-such anyway. How are you going to do that? First of all, you have to have the time. And Bob Heizer, in the beginning, worked a little too fast, to me,
He just ripped into things — not destroying them — but just, just lightning fast. And I was a little more careful, I think. And right now, I would say there are certain ways of going about this business.

Supposing you just had one of these mounds on the bank of the Sacramento River [going to the blackboard]. Now this was Howells Point Mound near Knights Landing. This was in the summer of 1936. Three students — one graduate in the Department of Anthropology and two undergraduates — three of them cut through Howells Point like this. Five feet wide, 120 feet long — and we did that in one week. And, like this, Stratigraphic trench to the bottom. Then, another one, this way, just wide enough to move in. That left four quadrants. And all the time, we were hoping to see remains of human skeletons. Well, somewhere or other we hoped to see some human bones in this wall. And you'd take a place like this and cut it down from the surface to the cemetery. And after that, 62 graves, all loaded with grave offerings. And we were busy. And that's all we could do with Howells Point at the time, because we intended to come back the next summer.

Then we went to Redding on the north bank of the Sacramento River. It's all urban now, suburb I mean, on the north side of the river. But then it was just farmland. We dug a trench 50 feet long, 6 feet wide, and 14 feet deep in one week. Now, that's shoveling, and that's moving fast. And we recovered a good sampling of artifacts and, as I remember now, maybe 5 graves altogether. Then, as I told you, just for a change we decided we'd go up to Mount Shasta and have a look at the country. When we got back to Berkeley, Kroeber said, "What the hell did you want to do that for, anyway?" We could see the whole geography of northern California, southern Oregon. All right? Okay?

Now, in a cave deposit — suppose this is a rockshelter. Here's the type for all of the rockshelters. This is the lip which is lower down usually. Now you want to try a trench right through the middle. From here to here to the back part and down to bedrock, wherever that is. It might be three or four feet, it might be ten feet — you never know. But at least you can see the stratigraphy in the center of the cave. In the side wall, the strata will either be horizontal or they might slant this way, or they might be very erratic. Then, we would take a trench of the dig to this point. And the stratigraphy over here might not match this one up. It might or it might not. Under here, like this. Then we have the quadrant. So, the University of Oregon students in 1938, we totally excavated four caves in one summer using this type approach. And we knew all the time that the people around there, so-called pothunters (I call them looters myself), were just waiting for us to break camp and go loot, And then they'd get what was left. So we were dedicated to the idea of cleaning out the whole deposit. We realized all the time that we might lose something, some sort of record, or the exact position of a few artifacts might be lost. But just think what would happen if we left the place unfinished? They'd be in there next week and finish it for us. Now there are other situations like that which I could explain if there were time enough, but I think you've got the general idea. You must speed up. You don't have forever.

Then, in the Lower Rio Grande Valley, shared by Texas and Mexico, we found all kinds of occupation sites — mostly things left on the surface of the ground — sometimes in the river bank — but there were dozens of places on the open ground and they would average maybe half a foot in thickness. We found there that there was no stratigraphy in the first place. So we worked these sites with ordinary garden rakes. Just get out there and rake it — square by square, or course — we had it laid out and we kept records of what we'd done. But the garden rake is as good as anything.

Riddell: I think you've just about done it, Alex, so we'll cut it off and we'll catch you at the other end here. Bill Wallace is going to get mad if we don't let him get up.
Now, normally I follow the advice of my favorite Chinese philosopher, Charlie Chan, and don't look back for fear I'll see all my mistakes piled up behind me. But, our chairman has asked us to reminisce and perhaps boast a little, so here goes.

My career in archaeology began after Alex's, after Charles McKee's, and after Heizer's — although only shortly after Heizer's. My academic background is similar to Heizer's. I went to Berkeley. I was exposed to the teachings of the same professors, Alfred Kroeber, Robert Lowie, Edward Gifford, and Ronald Olson. Perhaps I was a little more influenced by Robert Lowie than Heizer was. I recall Heizer assiduously avoided signing up for Lowie's seminars. I never quite knew why, but this was one of his idiosyncrasies.

At that time, the Department was strongly oriented toward ethnography. Kroeber was interested in archaeology, and Olson, of course, did do some archaeology. He went out to the Channel Islands and did some digging there. But, essentially, our training and orientation was in ethnography. There were very few courses in archaeology offered. Olson gave a course now and again in either Mexican or Peruvian archaeology. Kroeber gave a course in world civilizations which was a wonderful course, but it was basically historical rather than archaeological. I do recall some excellent seminars that Kroeber gave that did deal with archaeological topics, particularly one that dealt with the development of archaeological research in the southwestern United States. It was an extremely stimulating kind of thing. Kroeber was a great man in seminar teaching. He was very good at undergraduate teaching, also. There was no methods course. In fact, none was instituted until Heizer joined the staff some years later. We learned archaeology from one another. You may say we pooled our ignorance. We also learned by doing. From time to time, and we had it much better than when Alex and McKee were students, the Department would dig up $200 in research funds. We would take a crew out for a summer. We did live a little bit off the land. And our late lamented colleague, Adan Treganza, was particularly skillful at this. In fact, the local farmers always locked up their goats and daughters when Treganza was in the vicinity.

My first extended digging was on the Marin coast. We worked at Tomales Bay. We worked at Drakes Bay, digging the mounds on Drakes Bay that contained the porcelain from the Spanish Galleon. Subsequent to... this was in 1941; you see, I'm not quite as ancient a fossil as the two gentlemen here on my right, but certainly as ancient as some of the others. But, going on, I did my first work on the Marin County coast. And then, it was there we spent two summers. And it was there I learned how to do archaeology. Subsequently, I did learn a few niceties of technique from the colleagues at Indiana University where I taught for some years. At that time, the Midwest was far in advance of California in archaeological techniques. And I learned to use a trowel instead of a shovel. I also learned, instead of trenching as Alex has shown us here, to excavate a large shell mound by cutting in a steep vertical face clear through the mound. I did this when I directed the dig at West Berkeley [CA-ALA-307]. Now, since that time I have done archaeology in a number of parts of the state — from the Oregon border, almost the Oregon border, to the Mexican border, from the coast to the desert. And a great deal of this has been public service archaeology for the National Park Service, for State Parks and Recreation. And I have not felt this lacks ethics in doing it, as Heizer suggested in his article, or at least I think he suggests, nor have I felt that it wasn't possible to do what Heizer refers to as anthropological archaeology. I was given — most of the work I have done was in connection with Paul Schumacher for the National Park Service, or Fritz here for California Parks and Recreation — and, aside from sort of broad guidelines, they didn't tell me...
what to do. I worked out my own research strategy. So if I didn't do anthropological archaeology, it was my fault and not the public agency.

I have also participated a good deal in emergency or salvage archaeology with volunteer groups where sites have been threatened in the local area, and for the National Park Service, for the Southern California Edison Company. And, again, I have not felt this need to avoid anthropological archaeology. I don't particularly like that term, but I think you know what is meant by it. I have also done historic site archaeology. This, again, was an interest of Heizer, although he doesn't go into it in this particular article. But he did participate in a number of historic site excavations, conducted a number of other kinds of investigations along these lines.

I have also — I don't like to keep using the word "I" but I don't know quite how to get around it — I have also done ethnography. In fact, I started as an ethnographer, not as an archaeologist. I worked extensively with the Hoopa, somewhat less extensively with the Mojave. Now, here I would like to take great exception to something Heizer said in this lecture, this article that has been published. I worked, like my predecessors, in the so-called ethnographic present, attempting to reconstruct the native culture as it existed before extensive White contact. Heizer suggests that in doing this, the California ethnographers — to use his phrase — missed the boat. I don't think they did. Now, historic changes had taken place in the cultures. And Heizer cites a number of instances of this. But I think all the field workers were aware of these, and they took notice of them. The fact that they did not make historical or acculturative studies was because they were not interested in this. The California Indian cultures were rapidly going, and Kroeber felt a special need for getting the information while we could. And, for that reason, it was the ethnographic present that was emphasized in the studies.

Particularly, I would like to come to the defense of Ann Gayton. I think Heizer sort of singled her out as one who failed to take into consideration the effects of, say, runaway mission Indians on Valley Yokuts culture. Gayton was certainly well aware that historical changes had taken place. And her paper on the 1870 Ghost Dance in south central California is, of course, oriented toward history. And, also, she published the translation of Estudillo's 1819 visit to the Yokuts. So, to single her out, I think, was a mistake.

And, also, I would like to comment on what Heizer said about C. Hart Merriam collecting six tribal basketries from one group in Yosemite Valley. And suggesting that, perhaps, our collections are mislabeled, that we have instead of a basketry collection from a single tribe is a group of basketries that came together through lines of trading. I don't think this is the case. The local Indians, of course, were always well aware of their own basketry. If you asked a Hoopa whether a particular basket was a Hoopa basket or not, she could tell you — a basket weaver — she could tell you if it was a Karok, or Yurok, as the case might be, and not Hoopa. And, if pressed, she could actually tell you who had woven that basket. So I think, in most instances, in the case of the collections, this kind of information was obtained. So, our collections are not that mixed. Even if they are, we probably could call upon Larry Dawson to put us right because he can recognize the subtle differences.

My interests have remained, aside from my brief foray into Hawaiian archaeology, have remained centered on the California Indian cultures and their development. If I have a theoretical bias, it's toward historical reconstruction — which we got a good deal of in our Berkeley days, because this was a particular interest of Kroeber's. The new archaeology has not touched me very deeply. And I would like to make just a few observations on present-day California archaeology. First of all, obviously much more and better technical work is being done. But I have the feeling that large sums of money are being spent without much tangible result in the way of published monographs and published articles on California archaeology. I don't think we've had an increase in published material
equal to the amount of money that has been spent. I also have a feeling that there is a tendency toward regimentation that is coming from the public agencies — that prospectuses for jobs for work to be done spell out what you are supposed to do and leave much less to the discretion and imagination of the field investigator. I think we are too tightly bound in by some of these requirements that appear in present-day contracts.

And, finally, I would like to take exception to Alex and say I hope that California archaeology will not become a science, that it will remain essentially a cultural and historical study. Thank you.

Riddell: Thank you very much, Bill. We will have one more speaker, and then we will get up and fluff up our pillows and have a ten minute break, and then we'll come back. Paul Ezell, we'd like to have you come up and carry on.

Aerial Photograph of CA-SAC-126 (Augustine Mound), ca 1933. (Photograph from the McKee Collection.)
Harry Riddell, Jr. (l) and Francis "Fritz" Riddell (r) recording burials unearthed during land leveling at CA-SAC-45 (Tyler Island), ca 1939.
I would like to begin by saying how impressed I am at seeing so many people show up on a rainy Saturday morning at what I regard as an early hour. Fritz Riddell tried to get me out here at breakfast at seven thirty, for God's sake. But no fooling — and the second thing — at how many out there look so much younger than I expected. I expected to see a lot of my age mates out there...

Delbert True [from the floor]: That's because there aren't many of your age mates left, Paul.

Ezell: Thank you, Del. I regard every one of 'em as a campaign ribbon. Well, back to our knitting.

Nobody, so far, has spoken about how did they ever get into archaeology in the first place. And I get the impression that everybody that I have listened to — not just this morning, but elsewhere — such as Harry Crosby last night, as soon as they could crawl started collecting or photographing rock paintings or something of the sort. I don't believe it. I suspect a great many more of 'em got into archaeology the way I did — just by accident.

I was a student at Sacramento Junior College in 1934, and in the spring semester — that was in 1935 — I wanted one more unit to round out a 16 unit course. And, at the time, I was heading in the direction of becoming a second Knute Rockne, so I was in the physical education program which was the same as the pre-med program, so I had heavy doses of anatomy and osteology, and you name it. And somebody suggested, why don't you take that course in archaeology? Now I knew, in theory, what archaeology was. Because Professor, I believe his name was Card, in geography, had talked a little bit about archaeology and I thought, well that sounds kind of like fun, but it's not really a serious occupation — I want to be a football coach. So, I signed up for Professor Jeremiah Beverly Lillard's course in archaeology. And at the end of my first day in the field, my world was turned around. I knew what I had been born for.

And, so, let me describe a little bit of what that course consisted of, because, apparently, I got a rather different introduction to it from the people who talked before me. We had a field manual, and we were required to read that field manual and know it. The field manual was written by a man by the name of Bade, who had been an archaeologist for the Pacific School of Religion and done all his excavation in Egypt. The only thing I can remember out of Dr. Bade's manual was that, above all, you must not permit the laborers to tell ribald jokes while they're on the job.

Even then, I was smart enough to wonder how in the world do you stop somebody from telling a ribald joke in Arabic when you don't know a word of the language! Well, so much for the field manual.

But, we also had something else we had to read. We were working on the Windmiller site. And, so, we had to sit there in Professor Lillard's office and read Gifford's *Miwok Ethnography*. And read it, and read it, and read it. And, at the time, I just didn't see any connection at all. But, as we worked at the Windmiller site, and I worked on excavating one burial, and I found a shell bead. And as Dr. Lillard had instructed, you noted the location of that shell bead — you noted the microlocation of that shell bead. What bone was it next to? Where was that shell bead in relation to the entire burial? And as time
passed and I worked on that burial, I began to see that what I was doing was recovering a
rope of shell beads that had been wrapped around that burial just as it was described in
the ethnography. And I could tell you, it was a wonderful feeling.

Well, another point I want to touch on is this matter of research design. You can see
what kind of research design I had when I went into archaeology as a career. By the end —
no, by the middle of the semester, I — either Dr. Lillard was darn hard up for help, or I'd
impressed him favorably — because I was made the excavation foreman. And I haven't
seen any mention out on the bulletin board or I haven't heard of a man by the name of
William K. Purves. Because Bill Purves was around when Dr. Lillard wasn't around. So we
had a fair amount of on-site instruction. And at the time I didn't realize how good that
on-site instruction was. So I am going to jump way ahead in time, now, for another
anecdote.

In the early 1970s, I was attending a Pecos Conference at the University of Arizona.
And I listened to three graduate students present a kind of panel, make a panel presenta-
tion, of how you should excavate. You should, first, lay out your site in grids. And you
picked a datum, you set a datum point. And thereafter, everything you found you should
locate vertically from that datum point and triangulate it from at least two corners of the
grid. Remember, this is the early 1970s. And I don't know that goes on here, because that's
what I learned back on the Windmiller site. That is what you did — with Dr. Lillard and
Bill Purves. So, that evening I got a chance to talk with a couple of graduate students at
the University of Arizona whom I knew slightly and had some respect for — maybe because
they were both women. And I asked, what about this — how come? And she said, because
don't learn that now. They learn processual archaeology at Grasshopper Ruin, but
nobody told them that was the way they should do their digging. They should locate
activity areas and on and on and on.

So, the point of that is that I think I got a pretty good introduction to archaeology.
So we developed a research design, Dr. Lillard and I, of a kind. He called me into the office
one day and said, you seem to like this very well — do you like it enough to try to make a
career out of it? And, sure, but how? And he said, well, to be honest about it, you don't
get the kind of archaeological instruction at Berkeley that I think you ought to have. So,
how about you're trying at the University of Arizona? Okay, fine. To encapsulate all
this, he interceded with Dean Byron Cummings at the University of Arizona who promised
to use his good efforts to try to get me a tuition scholarship if I could figure out a way to
support myself. And so I graduated from Sacramento Junior College. I got a job driving a
freight truck to Hollywood, hitchhiked down to San Diego, hopped a freight train out of
Jucumba, and got off the freight train in Tucson with about $2.50 in my pocket, and went
out to the University and eventually ended up with a $20 a month job in the museum and a
tuition scholarship — and I was on my way. Now, the end of this research design was that
I was to get my Bachelor's Degree at Arizona and then come back to Sacramento Junior
College for a teaching position in anthropology. See, I had my life all laid out.

Only, when I graduated and came back to Sacramento, Dr. Lillard had fallen ill and
was forcibly retired. His successor said, enough of this nonsense — we're not going to fool
around with any of this. He packed up the nice museum on central California archae-
ology there — they shipped it, presumably, down to Berkeley, eliminated the courses in
anthropology. I don't know what happened to the people over there. So, there was nothing
else to do. I went back to Arizona to go back to school. Not quite true. My roommate and
I went down to Berkeley and enrolled in a course at Berkeley. And I got a job sacking coal
in one of the local coalyards. And I came home one evening, and my roommate was
packing. I said, what the hell are you doing? He said, I just heard that Haury has been
appointed chairman of the department at Tucson, and I am going back. I said, wait a
minute — wait a minute. And so, that evening, that fast, we withdrew from Berkeley, went
back to the University of Arizona where we could continue getting some instruction on excavation.

I think you're entitled -- I'm not so much afraid of looking behind me at my mistakes. I don't mean that you were, Bill, it just never occurred to me. I have used this anecdote a lot in trying to get my students to pay attention to where they find things. But the archaeological field school, the summer of 1936, I was working on one of these rooms and digging along with a trowel and so on. And I came up with a deer jaw. Oh, that's interesting, a deer jaw. I laid it up on the wall. After a while I found another. Eventually, I think I had found about four. And that evening, it rained. The next morning the rain had washed the dirt and stuff off the deer jaws, and they were all painted. I showed them to Dean Cummings and he said, Mr. Ezell, that's very interesting — where did you find them? And I said, over in this room here. And he said, no, I mean where did you find them a little bit more precise than that? Well, they were all about in this area here. And then he said, well, it's possible, Mr. Ezell, that what you have recovered is a shaman's cache, but we'll never know because you can't tell me whether the deer bones were in association with each other or not. Somebody has said you learn more from your mistakes than you learn from your successes. But I never forgot that one, I can tell you!

Now, back to Sacramento Junior College and another person who has been mentioned briefly here. I am going to mention him a little bit more, because he was, in his own way, a kind of a legend. And this was Mr. Zallio. I was always taught that he was Dr. Zallio. Dr. Zallio was a legend for a number of things, I expect, but a couple of them in particular. One was his habit of walking up and down the halls carrying a skull with one finger — I think there's something symbolic about it — stuck in the foramen magnum of the skull and pulling his goatee with the other hand — which had an index finger lopped off. And, somehow, we always — we knew that had been lopped off in a duel back in Italy. Pulling his beard, like this. And, of course, that kept both hands busy so he couldn't then do what he did to the girls all the time. So, then, he'd go, and — I can tell you, if they saw Zallio coming down the hall with both hands free, they found some other way to go. He was impressive.

The other — the story that I have no way of testing it, but it was certainly widely told, that Dr. Zallio's excavation technique was to find a mound and put two or three sticks of dynamite in the center and set 'em off and then collect whatever landed on the surface. As they say, I don't know whether it's true or not, but it makes a lovely story, doesn't it? Thank you.

Riddell: Thank you very much, Paul. Now, everybody leap up and fluff up your pillows and go potty, or whatever, you have but ten minutes. Come back as quickly as you can.

[After the break.]

Riddell: One thing that I did neglect to point out is that this session is sponsored by the Central California Archaeological Foundation. A nonprofit archaeological foundation in California established in 1959 by a number of us here.

There was recent concern about the direction in which the Society for California Archaeology has been going. I wouldn't say drifting, but there seems to be some problem. Now, there's one thing that could be done and that is simple say, "The hell with it." "I'm taking my marbles and I'm gonna go home and I won't play marbles with you guys again," or whatever. There has been a certain amount of that, and I have often thought of doing that myself. I'm sure it has crossed all of our minds for one reason or another, but I don't think that's really the constructive way of bringing the Society for California Archaeology together. We ought to stay in it and we ought to make it grow. We ought to develop
it within its own framework and if we need to make changes, well, then we will do it in an academic and democratic way. So, rather than just getting off in a huff, and saying the hell with you all, the members of the Central California Archaeological Foundation wanted to do something productive. Now, whether this session is or not, you never can tell. This happens to be my proclivity, and if you don't like what we are doing here, then you can get your own session next year and do it your way. If you'll notice, I managed to get as many pictures of myself as I could, but then, in any event, you do it your way. The basic intent was that we would serve as a healing factor and we're not asking you to agree with what Heizer has presented in his lecture of 1975, or what is being said here, but there are some of his expressions here that are of value. I think that we are very concerned, and the Foundation itself is concerned, that we do pull together and that we do make this society into what we want it to be made into. There has been some discussion now and through the years, about just what that is going to be. We do have new officers and maybe there's a new thrust, but nonetheless, we do not want to take a stance and say well, now we're in the saddle, and the hell with the rest of you. On the contrary, we want to find all the ways that we can all be accommodated within the framework of professional and avocational archaeology, where the standards are high. We do have considerations, and there are some very vexing and serious problems that we face in our society and people in California for the understanding, the integration, of the needs of the California Indians. These are problems that are growth pains, and these are problems that we are grappling with, and there are peaks and valleys. There are a lot of ruffled feathers now, but this will pass. We have to be cognizant of Native American needs, but also we cannot let our academic needs or our archaeological needs be sidetracked. I don't want to dwell on this, I don't want to get too deeply into it, but we do need to have a broad understanding. This is the Society for California Archaeology and we have a certain framework in which we operate. With sensitivity to others and working together, it is our firm hope that we will grow and prosper. This was the aim of the Central California Archaeological Foundation, which has sponsored this particular session.

I will proceed with the next speaker, Frank Fenenga.
FRANKLIN FENENGA

I want to talk about the particular circumstances that made, for the decade of the 1930s, from 1930 to 1942, Sacramento the center of archaeological activity in this state and the community college of the city being the academic center. Now the experience that I had in this connection was shared by Heizer and by McKee, by Paul Ezell and by Fritz. Alex, Gerow, and Wallace were not part of this same system. But let me talk a little bit about the background for it.

In the first place, in 1932, there was no academic archaeology offered anyplace in the state nor on the coast, at any institution whatsoever. Not only have people spoken of the fact that they had never had a course in archaeology, but my entire career, I never had a course in archaeology — it just simply was not offered. There were archaeologists working in the state at the time. The archaeologists who were working were employed by museums, Mark Harrington and Amsden and a community of avocationalists that they had gathered around them worked at the Southwest Museum. Malcolm Rogers, down at the San Diego Museum of Man, again a community whom he had gathered around him. David Banks Rogers at the Santa Barbara Museum of Natural History, and he was not the kind to assemble a community in many ways. He was very much of a loner, and his successor, Phil Orr, was even more lonesome. At Berkeley, Loud, who was a custodian in the museum, carried out the archaeological objectives that came up occasionally as a matter of immediate need or salvage of some particular lot of materials. A sometime associate of the department, W. Edgar Schenk, who was never employed because there wasn't any money for it, and in part, because he didn't need it, had a title in the department and also carried out some archaeological research. But, there were no real academic positions whatsoever. The first ones that did develop in the state generated around an exceptional personality, Jeremiah Beverley Lillard, who has been mentioned in several connections here who became the president with the foundation of the City College, Sacramento Junior College, here in Sacramento. He very early got turned on hobby-wise, to archaeology. He employed a succession of people, teaching in a Department of Anthropology; that included Dr. Richard Reeve, of the University of Washington, and Bill Purves, who you just heard about, who had had some archaeological experience in Canada. They became brief associates; there was the enthusiasm of a man who taught Italian in the school, Emilio Zallio, who Paul has said a few things about. All my life, I've borne an extraordinary illness. People say to me, "Frank, why are you so indiscriminate? How can you persist in saying something nice about every son-of-a-bitch in the world?" One of the three or four people I can't think of anything nice to say about was Zallio. But the dynamite story is not true.

Anyway, Lillard had gathered something of a community at that college and he did an out-and-out conscious job of recruiting. Heizer was the subject of a newspaper story in the Winnemucca or Lovelock newspaper, that somehow or other, had landed on Lillard's desk. Lillard wrote him a letter. It was a newspaper story about the precocious young man who was interested in Indian relic collecting. Lillard wrote him a letter and invited him to come to school. Why it was Sacramento Junior College, rather than any of a number of other institutions that might have been Bob's first alma mater. Fritz and Paul Ezell were walk-ins. My archaeological experience began in the Middle-west, and it began very precociously, in spite of the things that Paul says about such precocity, and I had had a couple of years experience in field work as a high school student. In a meeting of the National Educational Association, I spoke about archaeology as a career, as one of a series of high school students who were recruited to talk about potential career opportunities. I would hate to have anything I said at that meeting quoted. But one of the men sitting in the audience was Lillard, then being treated as the father of the Junior College system,
which was an exaggeration. Lillard began a correspondence with me, actually my summer
of 1934 was spent excavating at Woodcliff, Kentucky with a Chicago field party and they
in turn — Faye Cooper-Cole — offered me a scholarship. I was recruited in that instance
which led me to spend my first year at the University of Chicago. But the moment that
scholarship was over, it was real clear that my family and I could not afford that kind of
an institution, and Lillard had offered a job. I was the special assistant to the Librarian,
and this was the same job that some of my predecessors had held. Not Heizer, he wasn't as
desperately in need of the money. This was absolutely the case with Dick van Valkenburgh,
and it was this recruitment practice that had a good deal to do with what went on.

But again, I want to come back to how little there was going on archaeologically,
academically, any place in the state. The first individual who we identify as an archae­
ologist to earn a Ph.D. at Berkeley was Bill McKern. He was a native of the city of
Sacramento, and when I first came here, his father was a druggist here in this town.
McKern's doctoral dissertation deals with the Patwin and his Functional Families of the
Patwin is an abstract of that doctoral dissertation. The second individual who we would
identify unquestionably as an archaeologist who went to school at Berkeley was Duncan
Strong. His doctoral dissertation was Social and Political Systems of Aboriginal Southern
California. Julian Steward worked as an archaeologist early in his career. Ultimately, we
think of him in different terms. But his doctoral dissertation has to do with social
organization in the Great Basin and Plateau. Heizer's proposals for a Ph.D. subject, which
included the report of a cave that he had excavated in Nevada and Kroeber rejected that,
and a review of the archaeology of the central California area, and Kroeber rejected that.
They said the subjects were too narrow — to pick a bigger subject. I'm serious, Heizer
thought that as a joke he'd suggest that he take whaling as a subject. 'Kroeber said great!
So, Bob's thesis is the distribution of whaling techniques throughout the world. There was
very, very little encouragement for archaeology.

The first archaeological dissertation ever accepted at Berkeley was Dick Beardsley's,
When Beardsley brought his thesis in to Kroeber, Kroeber called a quick meeting to talk over
whether this whole composition was conceivable. That quick meeting included Heizer and
myself, and we sat on poor Beardsley's thesis on the question of whether or not the
taxonomic system was an acceptable one in any way whatsoever. It was really, in a sense,
brand new. The Middle-western archaeology was Gordon Willey, not McKern or Cole, or
Duel, as far as Kroeber was concerned, for reasons that I'm not really sure about today. My
own enthusiasms are the other way around, in terms of cycle of time.

This is a but of an aside, and it's a little more personal than I intend, but in 1940-
1941, Kroeber had ceased driving for reasons of health. I had an appointment as an
assistant in the University Museum, and what that meant was I was Kroeber's chauffeur.
I am still the world's poorest driver, and how he stood this, I don't know. But this was an
extraordinary experience and I think almost no, nearly no other student, ever had that
same kind of close association with him. Time after time we would talk about pitstops
and their necessity, and other such details. Kroeber was relatively aloof and not an
immediately accessible individual until his very last years. The experience was an extra­
ordinary one, but he told me repeatedly in the course of these drives, that he had, early in
the century, assessed the threat of the loss of information about the Native Californians.
It seemed to him that the prehistoric remains would last for hundreds of years before they
would have to have any attention directed to them; that the neglect of archaeology came
not from any disinterest. Obviously he made substantial contributions to archaeology in
Peru and Mexico and the Southwest. It came not out of any disinterest, but out of the
imperative, we now speak of, salvage operation of recovering the information from people
who remember what the native cultures had been like prior to the intimate and total
confrontation with western culture.
Outside of the fact that there was nothing else going on, there were opportunities for archaeology to go on. They were formulated as a consequence of the desperate economic situation of the United States in that particular time, the Depression. It resulted in great unemployment levels and the solution that was visualized by the Secretary of the Interior, and by the President, Franklin Roosevelt, from 1932 on, was that of finding a series of labor-intensive jobs that would not compete with private industry. Now can you think of a more labor-intensive job that has less economic value but ours? This was it, and there were funds available that could be tapped. CWA was the first, PWA (Public Works Administration), NYA (the National Youth Administration), and this was the dominant one as far as colleges and universities were concerned. This provided employment. Employment, again, the wages were 35 cents an hour. There was a limitation on how much you could earn per month. That limitation was $17.50 by the system. To make that not so hard to swallow, board and room in Sacramento or anyplace, cost $17 a month at the time, and that gave you 50 cents for other kinds of things. We employed crews of limited work time to carry out excavations on Saturdays and Sundays in the mounds of the Sacramento Valley and the lower San Joaquin Valley.

I want to switch for a moment to the fact that I think there were some very positive results from that cycle of excavation, in spite of the fact it is real easy to tell some horror stories. First, in the last five years, four Californians have written books, an Introduction to Archaeology. David Hurst Thomas, Jason Smith, Sally Knudson, Brian Fagan. All four of them agree on one single thing at least. The objectives of archaeology are very easily described. The first of these is to discover local chronologies. The second is to report prehistoric social systems. The third is to describe the causes of cultural change. Now a product of the work that was carried on in this system initiated by Lillard and centered in this community college before the became interested. Lillard recommended Berkeley to me very, very strongly, and suggested that Arizona was where the easterners spent their summer vacations. There's no question but what the results of that work represented in the two bulletins that were published by the Sacramento Junior College, and in particular by the second one, and by the subject of Beardsley's thesis, is an effective local chronology. There's also no question but what people did address themselves to was the description of prehistoric social systems, prehistoric cultural systems. Heizer's summary, the Early Horizon [The Archaeology of Central California I: The Early Horizon], which is in effect from sites of the Windmiller complex, constitutes his style. There were many addresses to the explanations for cultural change that were a product of that cycle of time. Heizer's contribution is the recognition that the fired clay objects of the lower Sacramento Valley are a particular adaptation to an alluvial plain, where no stone resources are available, and that there are analogs elsewhere in the world, as in the Archaic, the Lower Mississippi Valley. It represents just such an explanation — a model one in many senses. Another cultural change product of Bob and, in this case, Gordon Hewes, is the account of ritual burials of animals by simple Californian peoples, with the rich use of ethnographic explanation and analogy. In all modesty, my count of weights and projectile points as they are clued to their functions, constitutes a model and a testable hypothesis in terms of explanation of cultural change.

Academic achievement was very substantial. In terms of technique, there are some other states that are part of that same scheme. In the first place, the outline forms for the recording of archaeological data that we employ included what is infinitely the most popular site recording form used anyplace. It was developed in that community and is used not only by a large proportion of us. I recognize that everyone of us feels, for his particular data there is a way to improve it. But it became adopted by the River Basin Surveys of the Smithsonian Institution in consequence of which the system became the system for many other states in the Union, and as a product in part of those forms, and the site record form in particular.
The first site survey is the one that initially had S, the initials; and the Howells Point for which Alex Krieger had some of his early crew boss experiences, and that fast-dig through the site is S1, the first site in that system and hence, in many senses, in the present day California archaeological site recording, is No. 1. An ultimate product and several of the second stages of fossilization. People here in the front row had an important part of that, was a generation of a succession of additions of a handbook of field methods in archaeology. Once again, for some length of time, this was certainly the most sophisticated one available. No longer would it be the model because their archaeology is more complex that what we expect. There were very positive products of this cycle of time.

I want to change the subject a bit, and talk about the personality of Lillard. He had a very real and motivating effect on Paul Ezell as he has expressed it, and on me, who was the third of these field chiefs that Lillard found. On Fritz, who was my successor, in this particular experience, Heizer outgrew the association and never felt quite as warmly as the rest of us do toward Lillard, for what had to be personal reasons. You react toward older men like you act to your father. Some of my students react to me just as they would react to their fathers. From my point of view, I learned enormously from Lillard. In the first place, one of the questions that he asked — and I had a good deal more archaeological experience than he when I came here and more sophisticated circumstances — but one of the things he constantly pushed on me was not to call things by some jargon name for the type, Type XI, or something. Make what you say about it explain what it's used for or what its importance is, or how it works. Anything we defined in geometric terms or algebraic terms or some combination of these, instead of terms of functional identification at any stage of an attempt to communicate our information was a no-no, as far as he was concerned. This has served me well to be able to communicate better what I'm involved with and some of my peers. Or else I'm not bright enough to read all their writings.

Well, I obviously have spoken upbeat with respect to the past. My own attitudes about the expectable future are also very positive, this with respect both to the society and with respect to the successes in California archaeology. I share Wallace's recognition of the fact that we have increased the number of people involved and the amount of money involved more rapidly than we have increased the reporting and so forth of our data. I can think of something that everybody ought to do, everybody else ought to do. I'm sure that State Parks and Recreation should long ago have established a museum in every Indian Center in California, and if the Indians are extinct in the particular area, then there ought to be a museum that deals with that extinction. But, there has been surprisingly less support financially for archaeology in California than in some states that are nowhere as rich as we. But I think sometimes that comes from the fact that we have not done an effective job of representing ourselves and lobbying to those agencies which support us. One man at Missouri and one man in Arkansas could have done an infinitely better job, or one man in the state of Washington, or one man in the state of Georgia. I mean McGimsey, and Carl Chapman, Art Kelley, and Dick Daugherty have done a better job than 700 people involved in California archaeology have done, in terms of convincing our public support base that what we are doing is worthwhile. As I say, I don't know why; if Fritz is next to me, I can think of right offhand one of the things the State Park system ought long ago to have done, but I expect I can think of something that all of you ought to do. Thank you very much.
BERT GEROW

I have one comment to make with respect to Frank Fenenga's reminiscing, and that I am impressed by the parochialism of Berkeley. I was a student at UCLA in 1935 when Ralph Beals offered a field course in archaeological field methods, and we went down to Wilmington and dug there on Saturdays, developed blisters, got sunburned, and so forth. That was one of my first experiences in formal archaeology, and UCLA apparently antedated Berkeley in this respect.

Unlike other speakers this morning, I have arbitrarily assumed that you would be more interested in ideas than in personalities, and, right or wrong, that's what I am going to talk about. In reading the article by Heizer, naturally, what I looked for were ideas. One statement struck me as particularly useful, if applied:

The real problem, as always, is how to determine what is solid and progressive in new ideas, and what will turn out to be a blind alley, a false lead, or an incorrect and misleading approach to problems. The answer lies in repeated testing, effective criticism, and correction, as well as the creation of further new ideas.

Heizer made this comment in connection with his appraisal of the new archaeology. I would apply this rule to diachronic archaeology as well.

One of the events that was particularly important in my career was the discovery and excavation of University Village [CA-SMA-77] on the southern shores of San Francisco Bay. I approached this excavation from the point of view: here was new data, and the Central California Taxonomic System should be tested in the light of these new data. At the same time, I was interested in placing University Village in a spatial and temporal context. When the excavation took place between 1951 and 1953, there were virtually no radiocarbon dates. There was one from the Blossom site, there was another from the Patterson or Newark site. By 1954, I had received two dates for University Village, one of 750 B.C. and one of 1200 B.C., but these had to be evaluated in the light of the very small number of California dates that had been determined by that time. These dates were subsequently corrected in 1965 to 1000 B.C. and 1450 B.C., respectively. Unfortunately, many students still quote Heizer's published dates for University Village. At present, 13 radiocarbon determinations on charcoal, oyster shell, and bone collagen range from 790 B.C. to 1450 B.C. and yield an average of 1118 B.C.

By the time of the 1954 winter meetings of the AAAS, I had reached the conclusion that the University Village Complex was contemporaneous with one or more of the Windmiller facies settlements, but was not a development of that culture. What most influenced me was E.W. Gifford's Californian Shell Artifacts, on which I had worked as a graduate student during the early 1940s, Californian Bone Artifacts and Californian Anthropometry. I was struck by the essential similarity of shell artifacts from the Windmiller substratum and those of University Village and Scrl-3 on Santa Cruz Island, excavated by R.L. Olson in 1927. Particularly convincing was Gifford's unusual remark with respect to a specific artifact type (Scrl) shared by the Scrl-3 and the Windmiller substratum, namely "DR specimens appear so similar to SC specimens as to suggest importation from the SC or vice versa."
1. Dating Windmiller Settlements and the Termination of that Tradition

General acceptance of the contemporaneity of Early Bay and Windmiller facies settlements has been impeded by the belief that the SJ0-68, the only component with 14C dates, initially was on typological grounds the most recent of the original four, and, therefore, a Central California Middle Horizon culture commenced as early as 2000 B.C.

I argued against this proposition in March and April of 1967 before the SCA and the Kroeber Society. The following abstract was included in the first issue of the SCA Newsletter:

1. The lack of archaeological support for the generally accepted assumptions that the Blossom Site is the most recent of the Windmiller facies components and that it, therefore, provides a minimal date for the termination of the Windmiller culture and the commencement of a Middle Horizon culture type or types.

2. 14C dates determined for the Blossom Site, even if correct, are not immediately referable to published archaeological data since the former were determined on charcoal or calcined bone which came from a probable average depth of 42 or 50 inches, but the latter were obtained, according to Schenck and Dawson (1929) mainly from the upper 18 inches of the deposit.

Using published typological data, I endeavored to show that SJ0-68 was not the most recent, but the earliest of the four original Windmiller facies settlements. In three companion tables, SJ0-142 (incorrectly designated CCO-142) was indicated to be the most recent. A comparison of obsidian hydration measurements from SJ0-68 and from University Village suggested that the periods of occupation of the two overlapped. I believed that what I referred to as the Middle Horizon proper (evidence of *Olivella* 3b, 3c, and *Haliotis* 3 beads) did not commence prior to 500 to 1000 B.C. in the Bay region and no earlier in the interior valley. The total argument was incorporated into the work *An Analysis of the University Village Complex with a Reappraisal of Central California Archaeology* which was submitted to the Stanford University Press in June of 1968.

Sonia Ragir, utilizing additional 14C dates, a statistical analysis of flakestone points and a seriation by depth of SJ0-68 charmstones, apparently arrived at the same conclusions during 1968 and place the commencement of her Cosumnes or Middle culture at 1000 B.C. While the relatively greater antiquity of the Blossom site seems to have become generally accepted, a suspicion voiced by Ragir linger that all the bone collagen data are much too recent.

To determine once and for all whether or not this suspicion had any substance, several years ago I submitted samples of charcoal, shell, and bone from one grave at ALA-329. The bone and collagen dates were in close agreement. The shell, which consisted of *Olivella* beads, yielded a date of 550 years earlier. Subsequently, it has been possible to attribute this difference to a phenomenon, which marine geologists refer to as "upwelling." For the coast near Monterey, Stephen Robinson of the USGS in Menlo Park has proposed a corrective factor of approximately 700 years for marine shell dates. More recently, samples of charcoal, oyster shell, and bone for three University Village graves with significant association were submitted to the Riverside Radiocarbon Laboratory. Erv Taylor's determinations have yielded the following average results:
1. charcoal — 1020 B.C.
2. oyster shell — 885 B.C.
3. bone collagen — 978 B.C.

Up to the present time, 13 14C dates have been run on features from University Village, ranging from 790 B.C. to 1450 B.C. and averaging 1118 B.C. Hopefully, these tests and future determinations by additional laboratories will eliminate all doubts.

In the mid-seventies, a large series of bone collagen dates were determined by UCR for many of the Interior Valley sites. These included dates for SAC-60 and SAC-99, both of which were important in establishing the Central California Taxonomic System. An examination of this series of dates suggests that my estimate of 500-1000 B.C. and Ragir's 1000 B.C. are only applicable to some of the diagnostic bead types and not to the termination of the Windmiller Tradition of extended burials in a ventral position. These burial traits seem to have continued in the Stockton area to at least 600 A.D.

2. Relationship of Early Bay to Windmiller Facies Settlements and Olson's Early Island Cemetery SCR-3 on Santa Cruz Island

In 1968, I wrote:

University Village shares some Windmiller traits, however it is our view that the basic pattern does not appear to be an outgrowth of a Windmiller culture type. The technologically simple culture exhibited by the record clearly contrasts with the Windmiller culture with its emphasis on drilled and elaborated forms of shell, stone, and bone. As presently known, the Blossom site seems to show the least emphasis on elaboration of this order if we consider the frequency of whole Olivella shell beads relative to drilled shell fractions, leaf-shaped points relative to stemmed points, simple oval-shaped points relative to those exhibiting secondary curvature (piled and phallic), and undrilled relative to drilled canid teeth.

Flexed burial posture, powdered red ochre in the grave and/or on the skeleton, an emphasis on whole Olivella shell beads rather than drilled shell fractions, on crude flake-core scraper-knives and choppers rather than flaked stone points, on crude edge-notched stone weights rather than plummet-shaped weights (perforated and non-perforated), and on unelaborate forms of shell, stone, and bone rather than their counterparts, characterizes Early San Francisco Bay culture.

[Outside of San Francisco Bay] the closest analogue known at present seems to be Olson's Early Island Cemetery C-3 on Santa Cruz Island, which shares not only specific posture (Orr 1956:221), a relatively high incidence of red ochre, an emphasis on whole Olivella shell beads relative to drill shell fractions, limpet shell ornaments, stone mortars and pestles, bone gorge fishhooks, and punctate geometric design on bone. Both are characterized by a short-statured, mesocephalic population. Ornamental shell types suggest that they belong to the same general time level. Unfortunately, published data do not permit a closer comparison. Orr's Redhead phase of his Dune Dweller culture on Santa Rosa Island, with its emphasis on red ochre and seated burials, may represent a much earlier stage of the same basic pattern, but, again a detailed comparison is not possible at this time (Gerow with Force 1968:122-123).
In a review of *The University Village Complex* published in *American Antiquity* [Vol. 34, No. 3] in 1969, Wallace wrote:

Less well grounded is the alleged separateness of the Early Bay and Delta cultures. The two hold in common a plurality of diagnostic artifacts which suggests that they had a not very remote ancestor and represent regional subtypes of the same tradition. The Bay manifestation can be fitted satisfactorily into the existing taxonomic scheme as a new facies of the Early horizon (Wallace 1969:341).

Partly in answer to this review and one by Claude Warren, published in the *American Anthropologist* in 1969, I wrote the two parts of *Co-Traditions and Convergent Trends* in 1970. I reported my ideas at the SCA meetings at Asilomar and then at the SAA meetings in Mexico City. At the SCA meetings, I met Robert Hoover for the first time. He was then completing his dissertation at Berkeley. As a result of our meeting, Hoover kindly made available to me data from Olson's field notes on burial depth, posture, position, and orientation, but information on specific gravelots, except for red ochre, did not become available until the latter part of 1971 after he completed his dissertation. Consequently, *Co-Traditions and Convergent Trends* should be read as of 1970.

I had hoped that Robert Hoover or someone else would reexamine the conclusions in *The University Village Complex* on relationships between Early Bay, Early Delta, and Early Island assemblages in the light of the more complete data available after mid-1971. Unfortunately, this has not come to pass, so I would like to do so now.

Robert Hoover, in his analysis of R.I., Olson's gravelot data from SCRI-3 arbitrarily separated them into an earlier Christy Beach phase and an intermediate Frazer's Point phase. He refers to three burial styles, namely extended (erroneously labeled prone), sitting, and flexed. Burials occurred between 18 and 63 inches. Information on burial posture is recorded for 77 individuals; 12 or 15.6% extended; they occurred at depths of 30-63 inches. Six flexed burials in a sitting position occurred at depths of 18-39 inches. Clearly the majority of burials were flexed (49%) or loosely flexed (35%). Flexure occurred at depths of 18-55 inches; loose flexure between 18-60 inches. While the deepest burial at 63 inches was extended, it occurred pretty much isolated with eight other extended burials in pit C. In pit P, the other three extended burials were encountered. All of the extended burials lay in a supine or dorsal position; 42% of the flexed burials were also supine or sitting; 30% were equally on the right or left side. Orientation was non-westerly or variable. Only 21% were oriented to the west; none of the extended burials were so oriented.

83% of 102 graves had associated goods. Burials shared Windmiller and SC regional shell bead and shell ornament types irrespective of whether they are extended, sitting, or flexed or loosely flexed. Central California Middle Horizon shell bead and shell ornament types were lacking among some 13,000 specimens, associated with 70.6% of the 102 graves. Extended burials appear to have possessed the greatest quantities of grave goods and may have been wealthier individuals of higher ascribed status. Considering the evidence, one can find little or no justification for separating the graves into two phases.

Wallace and Elsasser both have incorporated summary data from Hoover's dissertation into their respective articles in the California volume of the *Handbook of North American Indians*. Both have accepted Hoover's separation of gravelots into earlier and later phases. Wallace follows Hoover in relating the Christy Beach phase to Orr's Early Dune Dweller culture on Santa Rosa Island and places it prior to 3000 B.C., even though Early Dune Dweller graves share few or no specific Windmiller facies beads and ornament types as SCRI-3 graves surely do. Without apparently checking Hoover's characterization of the Christy Beach phase culture against the actual gravelot data, Wallace repeats much
that is patently incorrect. There were no "prone" burials among the extended, and only 13% of the flex burials were so placed. Mortars and pestles were not rare: 14% of the graves, including extended burials, were accompanied by mortars or pestles, or both.

Elsasser also seems to accept Hoover's statements at face value and proceeds to discuss the flex burials as a Frazer's Point phase component in the context of a generalized Central California Middle Horizon commencing after 1000 B.C. without seeming to be aware that these burials were associated with only Windmiller facies shell beads and shell ornament types.

Both Wallace and Elsasser in their comparisons of West Berkeley and Windmiller facies single out shell beads and shell ornaments, curved bone fishhooks, perforated charmstones, and flakestone projectile points. Wallace adds asbestos rods, and Elsasser asserts that obsidian was little used for implements. Both recognize a difference in burial posture and in the relative frequency of stone mortars and pestles. While Elsasser concedes that the idea of an Early Bay culture may have some substance, he, as well as Wallace, prefers to view their Berkeley facies as not fundamentally different from Windmiller facies. Wallace flatly states that cultural assemblages that agree closely with West Berkeley or Windmiller have not been certainly identified beyond San Francisco Bay. Neither seems to be firmly in control of the typological data. The same Windmiller types of shell beads and shell ornaments are shared by SCR-3, which in turn shares with SJO-56, a Windmiller facies component, a unique turtle carapace pendant with Haliotis la type beads affixed to the surface with asphaltum. Similarly, single occurrences of unique curved bone fishhooks at West Berkeley and SJO-58, and two asbestos rods from West Berkeley and unworked splinters of the same material and possible function from from two of Dawson's graves at SJO-68, two graves at SAC-107, and one grave at SJO-112 provide temporal cross-ties but do not establish a generic relationship. Elsasser's assertion that obsidian was little used by either for implements is patently incorrect. Two flaked stone points with concave bases and eight leaf-shaped blades with squared bases which suggest a Windmiller connection must be balanced against eight expanding stemmed or side-notched points from greater average depths which suggest a southern coast connection. Perforated charmstones provide the strongest case for linking West Berkeley and Windmiller rather than with SCR-3. However, against perforated charmstones must be balanced a whole series of parallels between Early Bay and Early Island. These may be summarized. Higher incidences of the following: flexed burial posture, variable position, variable orientation including sitting position, dual graves, red ochre in graves, whole and naturally tubular Olivella shell beads, bone and antler tools (aws, daggers, bipoints or gorge hooks, antler wedges), globular and oval stone bowl/mortars, short cylindrical and conical stone pestles, flake scraper knives, and Monterey chert for tools. Lower incidences of the following: ventral position of the body, westerly orientation, projectile points, concave-base points, obsidian for implements, and quartz crystals. To these may be added cetacean vertebra bowls or mortars, steatite pendants, limpet shell ornaments, geometric punctate design on bone, and donut stones.

The more complete record of R.L. Olson's field notes afforded by Hoover's dissertation supports what I had previously inferred from the literature available in 1970. Namely, one can detect on Santa Cruz Island from the period of approximately 1000 B.C. a gradual weakening through time of the pattern fundamentally distinct from Windmiller, and absolutely and relative increases in such characteristic Windmiller features as ventral position of the body, westerly orientation, stone projectile points and blades, concave-based points, obsidian and quartz crystals.
Excavation Crew from Sacramento Junior College, Lompoc, ca 1935. Left to right: Anthony Zallio, Professor of Italian, Anthropology, and Fencing, Sacramento Junior College (private collector); Henry Gibbs, private collector; J.B. Lillard, President, Sacramento Junior College; Clarence Ruth, Superintendent of Schools, Lompoc; Schultz Martine, private collector; Richard H. Reeve, Professor of English and Anthropology (later to become the Chairman of the Department of Anthropology, Sacramento State University); Jerry Hurley, student at Sacramento Junior College (a Cherokee Indian). (Photograph courtesy of the California Department of Parks and Recreation.)
Just a couple of comments. Alex mentioned that Heizer dug too fast for him. He didn't quit that, at least not in my experience; he has now, of course. In 1957, when we were digging at a Nevada site, Eastgate, it took me two days to slow him down enough so that we could dig in levels. That fine grain kind of digging. I think he never liked to wait. He wanted to see what was down there.

Since we're looking to the past a little bit, I'll mention another thing not having to do with Heizer, but with his close friend, Philip Drucker. I saw the other day a report by Drucker to Frank Roberts dated 1948, in which Drucker reported on the survey of three proposed reservoir areas. One of them was called the Dry Creek area, which is now the Warm Springs area. Another one was Indian Valley; another one was Sly Park. There are reservoirs now in all three of those. Drucker reported to Roberts that evidently these were undesirable areas and little occupied by Indians, for there were no Indian remains there. In an area called Indian Valley! Although we can castigate a good deal of what goes on in government agencies, it doesn't go on like that anymore.

One of the things that Heizer says about public archaeology is that they apparently are spending too much money on stuff that's not worthwhile. In fact, he singles out Warm Springs in this regard. He says that the last year, fiscal 1973-1974, not less than $150,000 was paid by the Corps of Engineers for archaeological survey and ethnographic and historical background surveys, etc. I was in charge of the Warm Springs at that time. This was one of the various periods when Heizer and I weren't speaking. In any case, he goes on to say that its likely to cost a lot of money and wonders if it is worthwhile to spend all that money there. Well, in fact, we have spent that money and what he says is well taken. If we wanted to find out about North Coast Range archaeology, probably we wouldn't be digging there. Rather, we would be digging along the Russian River or in some other areas. Nevertheless, that's where the dam goes and that's what is going to be ruined. So, that's where the money gets spent. I doubt if he would be happy at the amount of money that has been spent there in the last three years, and he probably wouldn't be happy at a contract that we're now negotiating. But, he would be happy in one respect; he says "Further, I would, if I were Czar of Public Archaeology, require that every salvage excavation and contract stipulate that a complete written report be prepared by a designated individual, approved by an external review board, and filed before the certified archaeologist was permitted to be employed on another project." And that's exactly what this current, or I hope it's current, contract will be about. And that he would be, I think, pleased with that.

Incidentally, with respect to Warm Springs, Heizer himself, by way of a bequest, published the Warm Springs test report. I had a manuscript of the test report, he wanted to know if I wanted it published. He took it to Berkeley and then died, but left with the facility, the manuscript with the recommendation that it be published, and they did publish it. So, I'm grateful to him for that. That's all I want to say.
Recording a Bedrock Mortar Site, Folsom Area, ca. 1932. Charles "Brad" McKee (l), Louis Payen (c), and John Butler (r). (Photograph from the McKee Collection.)
I found the discussion this morning very interesting on a number of counts. It gave me insight into my own training, since I worked as a student with many of the people here who were then a generation above me. I was somewhat like Paul Ezell, except that, unlike Paul, I didn't even have a goal in college. I enrolled at the University of California because my parents expected me to do so. I had no concept of academic "major" or of inherent differences in elementary curriculum between engineering and liberal arts. I eventually completed the equivalent of two college years, including two semesters spent in a Navy training program, and began my search for a liberal arts major in which I could graduate. At that time, the late 1940s, there was a major (actually referred to as a "non-major") called "general curriculum," which consisted of 36 upper division units distributed through no more than three departments. I looked through the college catalogue to determine which upper division courses were open to me. There were only two fields for which I had the lower division prerequisites, German and psychology. Further searching of the catalogue revealed that anthropology had a number of upper division courses that did not have prerequisites. Thus, anthropology became the third field for my major.

I had never had an anthropology course and, as a matter of fact, I had no idea what anthropology was about. Nonetheless, in my junior year I enrolled in three anthropology courses (as well as one in German and another in psychology). The anthro courses were "Chapters in Culture History," taught by Robert Lowie; "Primitive Religion," taught by David Mandelbaum; and "Archaeology of North America," taught by Robert Heizer. My grades in these courses are of little use in predicting that eventually I would become a professional anthropologist. I received grades of "B" in Heizer's course, "C" in Lowie's, and "D" in Mandelbaum's. Despite my poor grades, I decided that the broad scope of the discipline meant that it had room enough for me. I declared anthropology my major.

Heizer's class was particularly interesting and novel to me. In his review of North American archaeology, he constantly drew pictures of artifacts on the blackboard, as well as numerous sequences of named phases, earliest on the bottom of the board and latest at the top. I dutifully transcribed into my notes the lists of sequences and amateurish renditions of the artifacts, often missing diagnostic features, but with little understanding of what it all meant. The most important thing that happened during the semester was Heizer's announcement in class that volunteers would be accepted for upcoming summer digging which the department was sponsoring — the first summer dig following World War II. Subsistence (food, not funds) would be provided by the university. Those who were interested were asked to drop by his office. I dropped by, indicated my interest (since I had no plans for the summer other than to seek a paying job), and was accepted.

The field work was done during the summer of 1947. I was on a crew directed by Russell Newman, working on the Richards mound north of Sacramento, and eventually moving to the Johnson mound south of that city. I was invited to end the summer at the Tank site in Topanga Canyon working under Treganza's direction. I'm certain that my success, if it could be called that, was based upon the facts that I dug very straight side-walls and could meet the expected production goal of digging a unit five feet square, five feet deep, in a day.

My students have repeatedly heard me tell stories of my early experiences. One of my favorites is about the time in 1948 that Al Mohr and I — Al is now sitting in the front row here — were assigned the task of conducting the survey for the New Melones Reservoir. We picked up a university car on a Friday morning, drove up to New Melones, spent the
weekend walking up and down about 12 miles of Stanislaus River, and turned in the automobile on Monday night after completing the work. We recorded four prehistoric sites and made the observations that the survey area contained much evidence of past mining activity and that the village of Robinson, which contained old mining refinery buildings and equipment, would be flooded by the proposed reservoir. Our recommendation was that no further archaeological work be conducted since the survey results did not warrant it!

I never had a formal field class and learned more about survey from Al Mohr than I learned anywhere in a strictly academic context. Learning from my peers was a very important part of my early experience. I cannot over-emphasize that fact, and I note the very supportive nature of peer relationships at that time. I worked with Jim Bennyhoff, Fritz Riddell, Clem Meighan, Marty Baumhoff, and quite a few other students of that time period. I also consider myself fortunate to have had opportunities to work in the field with Frank Fenenga, Bill Wallace, and Adan Treganza, as well as Heizer.

Hearing the stories and vignettes this morning helps me realize that I am part of a larger tradition and was offered exceptional opportunities. For example, despite the fact that I did not have a "B" average in my undergraduate career, Heizer invited me to apply to graduate school at Berkeley. I did apply and was accepted. After several semesters in the Department of Anthropology at Berkeley, I dropped out. For about ten years, I did things other than archaeology and anthropology. In the late 1950s, my wife decided to enter graduate school in anthropology; she was accepted at Berkeley, and I became involved in archaeological work once again and eventually completed my graduate training in anthropology at the University of California, Davis. I joined the faculty at Sonoma State College in 1967.

Listening to the talks this morning, I gained some understanding why I emphasize certain concepts and procedures to my students. Why, for example, I emphasize methodology and a concern for certain kinds of detail; why I am concerned with the development of productive excavation methods. I was struck by the emphasis which panel members placed upon "moving dirt." Although this emphasis could be viewed as a poor approach to conservation, without moving dirt those relatively rare materials which provide so much of the distinctive cultural context are seldom recovered in sufficient quantity to allow meaningful analysis. Without such materials to provide context, the detail recovered by contemporary excavation methods often seems meaningless. I ask myself, "Is this conservation?" Perhaps we can strike a balance between these two approaches, that of meticulous attention to fine detail and that of moving dirt.

It was also very entertaining to hear Alex Krieger's comments on workshops. As those of you know who have worked with me over the years, I am convinced that the workshop format is productive and necessary to achieve satisfactory synthetic results. I see now where the roots of this conviction lie.

So, I find the talks this morning interesting and enlightening. Although I don't know whether I will change my behavior in any way as a result of what I heard, I can say that I found the experience worthwhile. Thank you.
Fritz wanted me to say something about D.B. Rogers, whom I once visited. Maybe this courtesy is something that is not remembered anymore, but in those days, it seemed rather nice to visit somebody who had excavated the site before you, and see whether they had any further interest in the site. So, when Bennyhoff and I were going to dig at Carpenteria, I visited D.B. Rogers and asked him about further interest. Obviously, he couldn't have very much interest, he was an old man like this sat the time, but he was very kind and helped us as much as he could.

But, really what I would rather say is something about two other people. Now, we've heard about Treganza and his slight of hand in grocery stores and his cat eyes at night for ferreting out goats, or various other things. I was raised in a rather straight environment and I always shuddered at these activities, though I benefitted by them, of course, when it came to mealtime. I look on Treganza quite aside from these things as one of the senior students, graduate students, who did the most to help new students. Of course he benefitted by our labor sometimes, but he also went out of his way to help everybody. I would place him along with Frank Fenenga, as the core of this group. And while Treganza's name has come up several times in connection type problems, it is sort of disappointing to see how few references are made to someone who was so active and important in the past.

Now, the other person that I've been thinking about is E.W. Gifford, as a person who, in his way, was exceedingly important during this time period. I'm talking about from my experience, about 1946 to 1950. He has been mentioned in connection with some of his published work, but I was thinking about Gifford as a person who maintained, if not a unique research facility, certainly a very unusual one, in the UCMA [University of California Museum of Anthropology] at the time. Now, since that time, I visited museums in various parts of North America and other foreign countries attempting to get useful information. One quickly discovers that having large collections in an orderly type of storage where there are accessible records that, at least numerical records that one can put one's hands on, to find out where they come from. This is not something one finds every place. And, when I talk to people who were students in other universities in this period, even where there were large museums, one finds that the access to such resources was not common, whereas at the UCMA .... Well, actually the first time I went there I was a high school student, in about 1942 or 1943, and I knocked on the door and a little Japanese man came to the door, whom many of you have known, Kanmo Imamura, and I was conducted up to Gifford's office and Gifford arranged for me to see what I came to inquire about. There was no problem. While Gifford had his peculiarities about access to the museum, by and large students at Berkeley had a resource to work from and they could get in to use it in the fashion that was not common. It's been mentioned that perhaps there was a field school given at UCLA at an early date, much later at Berkeley, but there was a .... Besides a student network, a really functioning student graduate student network at Berkeley, an access to resources that was really unusual, and that was Gifford's doing, I think.
Sacramento Junior College Excavation Program at CA-SAC-126 (Augustine Mound), 1932-1933. Top: J.B. Lillard (l); Charles Hughes (r), Superintendent of Schools for Sacramento. Bottom: Charles "Brad" McKee (l); Richard van Valkenburgh (c), Sacramento Junior College; Robert F. Heizer (r). (Photograph from the McKee Collection.)
A few months ago, I was in a Safeway store and I saw this fellow in front of me checking out through the counter. He was buying an item or two, and I noticed that in the back of his shirt there were about three packages of cheese and various other things, so I was tempted to ask him where the archaeological excavations were.

I am really relatively a newcomer to California archaeology. I started 24 years ago and, if you stop to think about it, 20 years before that, Fritz was already started. There are also some other good, old-time stories that I would like to share with you because we didn't. . . . In southern California, we did some things differently. We didn't always have to go out and live off the countryside. This is partly due to historical accident. Before I came to California, I was a graduate student at the University of Washington, and I met this fellow graduate student, a young woman, who became a very close friend of my wife. Her name happened to be Louise Scripps. She was married to Sam Scripps, who was the grandson of E.W. Scripps of Scripps Howard Newspapers. They lived in La Jolla on the Scripps Ranch.

I remember one time when I had $20 in the archaeological survey office to do a survey of a freeway right of way in San Diego. I got a crew together, and we went down to San Diego and spent a day walking the right of way. I don't remember much about the archaeology. But I remember that Mrs. Hawkins, who was Sam Scripps' mother, invited us to come to the ranch afterwards. So we went to the ranch after surveying in the hot spring sun, and swam in their Olympic-size swimming pool. After that, we sat around and drank scotch and water in his living room which was about the size of this room. Finally, we sat down to dinner and had dinner served to us by the butler. So, those are what I remember as the good old days.

I have another story to tell. When I first came to UCLA, I didn't know anything about California archaeology and Clem Meighan for some reason hired me in the Archaeological Survey office there, and he decided that I needed a project to work on. So he said, go down to San Diego and find out what in the hell San Dieguito is. Well, by the time I got to San Diego, I wasn't sure whether I was supposed to be finding out about San Dieguito or something called "Diegeno." But he told me to look up a person whom I had noticed when I had received this appointment — when I was still at Washington — I went and looked at a most recent American Antiquity, and there was an article on San Diego County. He told me to look up this person named D.L. True. Now I thought D.L. True was a graduate student, because he had published this article in American Antiquity. And, so, I went down and found D.L. True hiding out among his avocado trees in Pala Valley. And, over the next four years, I learned two things about D.L. True. One was that he was one of San Diego's finest archaeologists, and he was one of California's worst avocado farmers.

The other thing that I am struck by is the amount of money that was spent then and now. When we excavated the C.W. Harris site the first time in 1959, we had a budget — a total budget — for transportation and everything, of $500. And out of that $500, we supported a crew in the field. As Fitzwater always says, he got paid a six pack of beer and a package of cigarettes. And about halfway through the season he struck — he demanded a six pack of ale.

Today, I am working on a project where we are spending more like $500,000. I find that it is not nearly as much fun. Another thing that struck me about what other people have said up here was that — talking about Heizer digging rapidly. Clem Meighan was one of Heizer's students, and Heizer apparently passed this on to Clem Meighan. And he came
down to the C.W. Harris site, and I couldn't keep him out of the pits. And he — we were
digging in this gravel that was really terribly hard to dig in — and he picked up a rail-
road pick and started going at it. And I was trying to figure out how to get him to slow
down. And I finally succeeded by sending Fitzwater to the nearest store to bring back a
case of beer.

I think there is one serious comment I want to make. In Heizer's article, he noted
that he hoped that the universities would train archaeologists — continue training
archaeologists — to be archaeologists and not cultural resource management people. At
least, that is the way I interpreted it. And, today, I wish that we had listened to him a
little more. I find working on the project that I'm working on at Fort Irwin — I find it
very frustrating to try to meet all the requirements of cultural resources management and
to do research. And I think that any department that is starting a program or is running
a program in cultural resource management must, above all else, teach their students to do
research. I think if you teach them to do research, they can then do, then can then learn,
even after they are out in the field, by research how to meet those requirements of cultural
resource management. And I think that we have gotten too much bureaucracy into this
whole program and that we are losing things that we should not be losing. And, on that
kind of sour note, I'll stop.

Riddell: Thank you, Claude. We are getting pretty close. Obviously, we are not going to get
all the way through the first row, let alone get into the second and third rows. I don't
know how long we have this room for. But, in any event, we do have time enough for a
couple of more people to come up here. And I think that it's only fair that "Red" True get a
crack back at Claude. Well, you ought to give him a couple of zingers, "Red" — what the hell!

True: No, I don't think so. Heizer was already sick before I ever got to know him. I really
don't fit into this old fogey category.

Riddell: Well, now you've heard it all. He's right on both scores — both old and fogey.

Well, anyhow, we've had one turndown. That's a heck of a note. We'll catch him on
the rebound. I'd like to have Jim Bennyhoff make a few comments. He's with Sonoma
State University, and he's got to promise not to pick on Bert, though.

Gerow: I welcome it.
Well, I'll keep this brief. I came into archaeology as a high school student through the National Geographic. I, in my high school days in Quincy, I decorated my notebooks — instead of writing the notes down — I decorated them with my Maya glyphs.

After the war, I knew I wanted to be an archaeologist. I had no idea what anthropology was. The nearest university was Berkeley, and, so, I entered as an undergraduate student and had serious difficulty with Dr. Mendelbaum and Dr. Lowie that I kept saying I wanted to be an archaeologist; I don't want to be an anthropologist. And, fortunately, they prevailed, I hope. I was thoroughly indoctrinated into the weekend sessions that I'll cherish — as long as I live — primarily under Frank Fenenga and Fritz Riddell.

In my undergraduate days, I was still — I wanted to be a Mesoamericanist. I had been raised in Quincy where the "Digger Indians" were insignificant. One didn't — who'd want to study them? I was fortunate that the first field school, summer field school, that Dr. Heizer directed was SJO-68. To most excavators, an Early Horizon site. And we did use shovels. I, myself, was knee-deep in water when I removed one of the most important charmstone caches that I'm still trying to unravel. Nevertheless, these early days produced the volume of material that gave us the sequence. And our units here and our units there that we dig today are not going to give us the sequence that we developed in the Delta with large crews. The emphasis was on grave associations. Nevertheless, we did locate a significant number of artifacts in the midden with the shovel, and I think that statistical analysis would demonstrate that we did get a significant sample with shovel archaeology.

There are — incidentally, we supported ourselves in the Delta by raiding asparagus fields at night.

I think that will be all...

Riddell: Thank you very much, Benny. And I would like to have one of the people who has made this ... a lot of this activity of moving dirt possible because of the financial aspects that were involved. And, of course, the National Park Service in the early days did provide certain kinds of funding through contract to the various institutions and all, and Paul Schumacher, early on, with the National Park Service, knew us all and provided us with certain kinds of funding to make some of the early latter-day activities possible through federal grants, and so forth. Paul, would you come on up and make a few observations? Try to be polite.
PAUL SCHUMACHER

Well, I wasn't expecting this. And I think that we ought to do this again at the SCA meetings in San Diego, maybe just have a symposium on the culinary delights of archaeological field schools. I have had the privilege of — I've never excavated in California — but I've had the privilege of working with most of you here, except for this gentleman here [McKeel]. But, I have visited not only your field camps, but also your homes, and enjoyed everything from Edith and Bill's peanut butter and jelly delights to Claude Warren's taking us down to Tijuana, Mexico, and things like that. But I think one of the ....

Riddell: Can you enlarge upon that a little?

Schumacher: Well, there is a good story behind that one! I caught hell from Edith afterwards. I think Claude did, too. But I think Edith knew me better than she did Claude, and we did lead Charlie Rozaire astray, I think, by taking him down with us.

But what I want to really express — I think this has been a great meeting. And due to Fritz organizing it and to the Central California Archaeological Foundation, and I think we really ought to have more of these at future SCA meetings. Thank you.

Riddell: I apologize for some of the oldtimers that weren't able to hobble up here and get in the front line. There was no intent to leave anybody out. I see Bill Evans back there rather disconsolately sucking on his pipe. He ought to be with us, of course; we could catch him on the next round. And Mark Kowta, and all — any number — Bill Pritchard.

But we maybe skimmed some of the cream off, but we — if you want, we can organize another one. Maybe it served a purpose. And, of course, on behalf of the Central California Archaeological Foundation and the Society for California Archaeology, I want to thank you all very much for the tremendous attendance and enjoying this ....