

# CASUAL PAPERS

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## E R R A T A

Page 148: The following paragraph should follow at the bottom of the page after the last paragraph.

More radical chronological claims were advanced by Drover (1971, 1975, 1978; Drover *et. al.* 1979) for ceramic finds from Ora-64 in Orange County and from SCAI-17 on Santa Catalina Island. Ceramic material from these sites was proposed as dating to at least 1500 B.C. and up to several millenia earlier, making it among the earliest in the New World. Dating was both by stratigraphic radiocarbon samples and by thermoluminescence of the ceramic items themselves. The majority of the objects were interpretable as figurines or figurine fragments, but a few vessel fragments were present as well. Evidently, the last word has not yet been said on the validity of the dating of these specimens, which involves quite technical issues. The presence of an early baked-clay figurine tradition would be fairly easily accepted, but the presence of early pottery vessel manufacturing and its apparent subsequent disappearance would raise significant and difficult issues in the interpretation of the cultural processes involved.

Page 179: Omit.

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HOW WE DUG OUR OWN HOLE: A PERSPECTIVE ON ARCHAEOLOGY'S  
POLITICAL DILEMMA AND WHAT TO DO ABOUT IT\*

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It is an honor to have been asked to speak with you tonight. When I was a practicing California archeologist, we used to look with disdain on those who had run off to work in more glamorous places--Mesoamerica, for example. Such people, we felt, were mere thrill seekers, blind to the obvious fact that the prehistory of California is the key to understanding humankind's deepest concerns. Although I don't quite equate Washington with Mesoamerica, despite certain similarities in social organization, it embarasses me slightly to have joined the ranks of the deserters, and I am relieved that you see some merit in having me back for an evening. At the same time, since leaving I've become aware of a California tendency--which when I was one of you I doubtless shared--to see the world of archaeology, if not the world, period--ending somewhere on the eastern slopes of the Sierra Nevada. In the face of this perception, I'm grateful that you know I exist.

This insular tendency among Californians has several results. One is that Californians tend not to be aware of, or take much heed of, useful models being developed, and problems being grappled with, elsewhere. Another is that California becomes a sort of independent laboratory for the study of problems in archeological management. If something is going to happen, one can pretty much expect that it will happen first in California. Because of your relative isolation, such events can be observed under somewhat controlled conditions, and the rest of us can figure out strategies for dealing with them. On the other hand, we remain in a state of constant nervousness about what "those California wackos" may do next.

I am going to be somewhat critical tonight. I assure you that my criticisms are not directed solely at California archeology. What has happened and is happening in California archeology is a micocosm of what is going on across the nation. Nor do I exclude myself from blame for what has happened; in a variety of contexts I have had influence over archeological policy, and I don't think I've always made the right or best decisions. I don't intend often to point a finger of blame, but to try to isolate some factors that, in my opinion, have placed archeology in dangerous circumstances, and to suggest some solutions to our problems.

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\*Keynote address, Society for California Archeology, March 25, 1983, San Diego.

I should note that I am going to be addressing that segment of archeology with which I am familiar--archeology performed in response to the environmental and historic preservation statutes. There remains no satisfactory term for this activity; some of you will read me as meaning "cultural resource management" or, worse yet, "CRM" (cf. King 1982) others call it "public archeology." Others recognize that we do as part of the national historic preservation movement--though other preservationists do not necessarily share this recognition. For simplicity, I will just call it "archeology."

Archeology is in trouble today. We are widely perceived, and not without cause, to be profiteers. Our profit orientation is alleged not only to cause us to rip off clients, but to hold up projects needed for economic development and to meet social requirements, and to violate the graves of Native Americans. We are perceived as doing these things behind the smokescreen of "protecting the archeological resource base," which apparently means leaving on the landscape every stick, stone, and tin can ever thrown there before last week--or paying us big money to carefully, ever-so-carefully, pick it up. In this land of AB 952, I don't think I need to elaborate.

A few years ago I wrote an article outlining some of these perceptions (King 1979). The response I received was interesting. A few people involved in fieldwork-for-hire excoriated me for even suggesting that problems could exist; several employees of state and federal agencies said I'd hit the nail on the head, and a considerable number of senior colleagues wrote or called to say that I was right, but that in no way should I say my piece in print. The overall message was, "Get your head back in the sand, dammit, with the rest of us." Well, I don't think that's wise, and I think that our collective ostrich behavior has gotten us where we are today. Whether or not we are what we are perceived to be, we who live and practice by the leave of the public can hardly afford to ignore public perceptions; we must confront them and deal with them. More importantly, perhaps, even if we are not as bad as we are accused of being, I think most would agree that we could be much better; we could be doing better archeology, doing more with our archeology, learning more, being more useful, than we are. If we were more demonstrably useful, more productive, we would not be perceived as ripping anyone off, as a profession in need of legislated limitations.

My subject tonight is opportunities missed, wrong roads taken, over the last ten years--roads that have led us to our present dead end--and ways that we might escape the impasse in which we find ourselves.

At my advanced age I think I'm entitled to some reminiscence, so I'd like to begin with a couple of quotes from this Society's early days.

First, I recall a time, in about the third year of the SCA's life, when I was its President and we were involved in trying to get legislation passed to establish a "California Archaeological Survey." Bob McGimsey was a hot property in those days, and the "CAS" was modeled directly on the Arkansas Archeological Survey. We organized pretty well to push the legislation; we had an operative in virtually every legislative district, and we lobbied like crazy, got the bill through the legislature only to have it die under a gubernatorial veto. In the midst of this effort I recall being taken aside by Fritz Riddell, who said in effect, "Look, this survey bill is all very fine, but there's this thing called the California Environmental Quality Act that's going to transform the way projects are planned, and maybe we really ought to be spending our time organizing to make the most of it."

Well, as far as I was concerned, Fritz was just trying to protect his turf, because the CAS bill would take away from him what pitiful vestiges of archeological power the Department of Parks and Recreation still had. Besides, everybody knew that the CEQA just applied to state-funded projects, right, and the major problem with site destruction was elsewhere. So, we put all our organizational eggs in the basket of the California Archeological Survey, which bombed, and when the Friends of Mammoth decision came along, applying CEQA to virtually everything, we were scarcely better prepared for it than the land developers were. Whatever his motivations at the time, I think Fritz's approach would have been wiser and more productive than mine. It makes little difference to say it now, but Fritz, you were right; I was wrong, and the result of my failure of vision--certainly shared with others, but mine nonetheless--was that we were not prepared to take CEQA and make it work for archeology. Instead we let archeology be twisted to accommodate the systems designed by state and local government, the development community, and a plethora of consulting firms to implement CEQA.

Stepping back even further into the past, I want to quote from a paper delivered by Eric Barnes at the Southwestern Anthropological Association Annual meeting in 1967, within a year of the SCA's birth. Barnes was, and still is as far as I know, a planner by profession, a student planner in those days, who had become involved in archeology as an amateur and done good work in the Sierra foothills by himself and with Jay Von Werlhof. He was at San Francisco State when I met him; we both were students there, along with Rob Edwards and other notables and he had a profound effect on my thinking. In his 1967 SWAA paper, Eric said:

Forseeable public policy directions associated with governmental adaptations to changing needs--particularly those policy directions concerning natural resources, urban development, transportation, and related matters--would appear to have high combined potential for stimulating and facilitating

further change in the pattern of archeological research activities.... Generous preservation, coupled with a climate of positive planning, would seem to offer encouragement for problem-oriented archeological programs designed to incorporate regions corresponding to natural or cultural areas.... Perhaps the provisions of the Historic Properties Preservation Act of 1966 (sic) will apply to such ventures in archeological planning. It certainly would be unfortunate to lack integrated plans at a time when shifts in public policy favorable to their implementation were to become effective. It is not at all too early, therefore, to begin to think seriously of ways in which archeology might relate creatively to the emerging planning climate. A policy-and-program gap can best be precluded by advance preparation (Barnes 1977).

The ink was hardly dry on the National Historic Preservation Act--it was so new that even Barnes, who used to come to class carrying things like the "Congressional Record" in large brown envelopes franked by exotic federal agencies, didn't get its name right. And yet here was Barnes, telling us what it could do for us, and suggesting ways to make it work.

Well, as I recall, the Society for American Archeology formed a committee to interact with the National Park Service in implementing the Act, and the main concern voiced by the discipline, through that committee, was about the possibility that institutions might be asked to turn over their site records to the National Register. We haven't lacked opportunities to make the laws, federal and state, work for archeology; we have lacked vision. As a result we have drifted, accepted models of organization and behavior designed to serve other purposes, failed to control our own worst impulses, or to rise above the needs of the moment, and we now have a practice of archeology that is often little short of nonsensical.

There are, I believe, six big areas in which we have failed, and I'd like to discuss each in turn. The order of priority means nothing; they are all more or less equally important, and they are also interrelated. I separate them into six categories merely for convenience.

First, the failure to plan. Suppose we had taken heed of Barnes' remarks in 1967, and begun defining regional plans, regional research goals, and perhaps most important, regional organizations to implement the plans and pursue the goals. Suppose, further, that we had taken heed of Fritz Riddell's suggestion a few years later, and figured out ways to make such organizations relate to CEQA. Perhaps we would be in a position today in which the work required by CEQA, NEPA, Section 106, and the other preservation-forcing authorities would be organized rather than atomistic, directed



toward advancing research rather than toward whatever the consultant of the moment dreams up to sell to his client. We would be doing useful research, and the public would know it and support us.

Now--we did try. A variety of organizations arose in the early 70s to promote regional cooperation and, to some extent at least, planning; I'm thinking of such entities as the Bay Area Archeological Cooperative and the California Desert Archeological Committee, now forgotten except by us ancients. These failed for two reasons, I think. First, we failed to find an organizational context in which they could establish and implement plans and research designs. Second, we failed to deal with the atomistic tendencies that the Mammoth decision engendered in the profession. It may be that with the rise of the independent archeological consultant, the idea of regional planning was doomed.

I am not convinced that there must be archeological consultants; they are the creatures of our procurement systems, to which I'll return in a moment. But even with independent consultants, I am not sure that regional planning had to be doomed. If we had had a context in which to legitimize the nascent regional cooperative organizations, and in which to construct plans and research priorities and make them available to the agencies that could use them, we might have had a chance. By the early 70s the context existed, in theory, in the State Historic Preservation Office, and there was money for Preservation Planning through the National Historic Preservation Act grants program. In fairness to California though, the National Park Service didn't much encourage planning in those days--it was still wrapped up in promoting the National Register. Still, I think that if we had the will and the vision, we could have made state preservation planning work for archeology, and thus avoided many of the ills that now beset us.

A second area in which we have failed is in our relationship to the systems by which archeological services are procured. I was told recently, and our Denver Office checked it out and advises me that it really is true, that an academic institution in this state actually sought to take CALTRANS to court for failing to take the low bid on an archeological contract project. Now, I have no basis for assuming that the higher bid was any better as science, as research, as a preservation project proposal, than the low bid, but be that as it may, I think that when we accept, defend and vigorously insist through the courts that the low bid must be accepted, we have abandoned all pretension to scholarship. To quote what must by now almost be a cliché--at least, it's a cliché in less exotic places than California--procuring good archeology is different from procuring good bridges, or solid hunks of steel, or even silicon microchips meeting rigid production standards. Archeology is not done on a production line. We want the best work we can get for a reasonable price, not necessarily the cheapest.

Few of us accept low-bid as the way to get good archeological services-- certainly neither the Department of the Interior nor the Advisory Council has approved it. Most of us have promoted competitive procurement, however, and in the long run, I'm not sure the effect has been much better.

The idea of competitive procurement, of course, is that you put out a request for proposals and then rate the responses according to some objective scale, without knowledge of the prices attached. THEN you open the bids, and if your best proposal is attached to an out-of-sight price tag, you try to negotiate the offerer down before you give up and move to the next best. This all seems very clean, very professional, but it has fatal flaws. First, it is project specific. I have become pretty well convinced that as long as we work on a project-by-project basis, selecting Sam Spade to do this project and Marcia Marshaltown to do this other, we will never establish or even permit the continuity for archeological research. Second, the real difference between competitive procurement and low-bid can be almost vanishingly small, because the people writing the requests for proposals, and subsequently evaluating proposals are often just not very good scholars. This is not the National Science Foundation, with all its admitted old-boy flaws but at least a tendency to select as project reviewers people with education and experience; this is the State of Grace Transportation Department, or the Bureau of Forest Management, or the Interbureau Archeological Services Division, staffed mostly by people who couldn't or wouldn't make it in academia and, perhaps more importantly, by people who could and would make it in bureaucracies where you get along by not making waves.

They are not likely to be able, even if they so desire, to innovate or to welcome innovation. They use a procurement system that is fine for obtaining state-of-the-art engineering design studies or descriptions of local flora and fauna for the production of an EIS, but that is deadening to the creative processes that are central to advances in archeological research.

What is the alternative? I have some difficulty here, because we have all accepted the idea that "contract archeology" is the way to get the public's archeological work done. The only alternative I'm aware of is the "sweetheart deal," where an agency and an archeological entity enter into a cozy relationship over a period of years, the institution providing all the agency's archeological services on a non-competitive basis. The dangers of this approach are manifest, and most of us have frowned on it, but I am beginning to wonder whether perhaps, even with its warts, it is not a more productive approach than the one we have. Controls can be placed on sweetheart arrangements, using peer review, monitoring by third parties, and competition every few years for the overall sweetheart contract. The advantages of sweetheart arrangements are that they provide security for both parties, and that they provide a context in which the institution involved

can make thoughtful decisions about how to allocate its resources, rather than simply doing piecework on demand. I think it's no accident that whenever my good friend Bennie Keel starts extolling the virtuous results of Interagency of Archeological Services' contract program, the examples he points to are things like Richard B. Russell Reservoir, the Tennessee-Tombigbee Project, and American Bottoms--all huge, multi-year projects where services were procured competitively at the outset but which since their inception have been sweetheart deals between IAS and the contractors. Unfortunately, the great bulk of projects in this country are not the Tenn-Tom or American Bottoms, they are little sewer projects or highway widenings in which contractors have very little flexibility to innovate, or to even think seriously about research needs. If one were doing all sewer projects or highway widenings in a given area over a period of time, one could think about research applications, but when a host of contractors is scrabbling for each and every job, and the person who wins job X has no guarantee that he or she will have job Y down road the next month, it is awfully hard to establish and pursue research goals.

I don't have a real answer to the procurement problem, but I think we need to seek one, and I think we were wrong to accept without question that archeology had to be procured in the same way bridge designs and descriptive biology are procured. This acquiescence on our part, even without acquiescence in the low-bid syndrome, has fostered a system that selects for the lowest common denominator among us.

This leads me to the next failure. We have allowed the contract system to give selective advantage to the simple-minded, both in the agencies and among contractors. Part of the problem here springs from the fact that the federal archeological system--despite our best efforts remains mechanistic. One first goes out and looks all around a project area to find all the sites; one then tests the devil out of them to find out if they're eligible for the National Register, and one then, perhaps, thinks about what sort of research questions one can ask of them, and how. Depending on the nature of the project, one may be able to terminate work after just identifying the sites, by altering the project to avoid them all regardless of importance. Or, if one is into digging but not much into thinking, one can sink a lot of money and time into testing, quite likely deciding at the end of the testing phase that you know enough about the site, and don't need to salvage it. This saves a good deal of thinking about research design. It's a quick jump from this to the assumption that testing, "evaluating," describing the site is all that one needs to do in any case, and one can then have jolly arguments with one's intellectual peers about "how big a sample is sufficient" without ever having to ask oneself the question: "sufficient for what?"

One practitioner in this state a few years ago assured me that the law required 10 percent excavation of every site eligible for the National Register. I was certainly glad to have federal law so elucidated. It is obvious that one could train chimpanzees to dig 10 percent samples; you could probably even get them to argue about whether 1x1 meter units are better than 1x2s. It might take a bit more effort to get them to fill out forms, like the reporting form that I understand is to become the backbone of San Diego County's new archeological system. This is not the way to do productive archeology.

We have recognized the role that the federal system plays in driving this sort of simple-minded archeology, and we are changing it. The suspension of portions of the Council's regulations (ACHP 1982a), elements of Interior's draft preservation standards (DOI 1982), and our programmatic agreement on surface coal mining (DOI 1983) are all designed, in part, to move away from simplistic, property-by-property, context-free evaluation and treatment, and toward planning in context, which could by its nature select for a higher level of intelligence among its practitioners than that required to find and test sites. I believe that some similar actions may be needed by those of you responsible for managing archeology under state and local authorities.

It must be obvious from what I've said so far that I assume that what archeologists properly do is research. We usually fail to make our research questions explicit, however, and when we do get explicit, we often are silly. Here I must point a finger, in order to be clear, at Mike Rondeau of the SHPO's office. Mike has recently published a paper concerning research design on salvage projects (Rondeau 1982). Mike seeks to cope with a real problem; he points out that archeologists turn in research plans that articulate lofty goals and then fail to connect what they do in the field to the pursuit of those goals. This results in fieldwork that serves no purpose. Mike's solution, however, is to abandon the goals. Mike would have us focus, in each case, on what we can learn from the particular site in question. We have a site with lots of lithics? OK, don't study political organization, study flaking technology and edge wear.

I can appreciate the frustration that must drive Mike's proposal, but I don't believe that lithic technology studies, or midden development studies, or studies of what people ate and drank in a Gold Rush miner's camp, or studies of prehistoric settlement and subsistence systems, are worth one public penny in and of themselves. They are worth expenditures in the public interest only if they are reasonably related to topics of concern to the public--be that public be the people of the nation, or of a community. I think that as a nation, indeed as a species, we need to know more than we do about what makes social organization tick, about how the environment has

changed, about the structure of the human mind, about the effects of culture contact, about the causes of cultural change and continuity. I know that there are communities that want a clear, understandable picture of their roots, and that there are immediate practical questions of importance to planners and others that archeology can address. I think that to be justified, a piece of archeological research must be connected somehow to a "big" research question defensible on its intellectual merits, to the interests of a community, to current or future planning needs, or ideally to all three. We are public servants, and the public is under no obligation whatever to fund our work just because we happen to be curious about something. So we need to articulate large-scale, lofty if you will, research questions. Then when we confront a particular site, we should figure out a la Rondeau just what that site has to offer that is relevant to such questions. We have not only failed to do this, we have, on the whole, actively resisted it. I have been accused by one scholar in this state of trampling on his academic freedom because I argued that the Federal government should not fund his research without more evidence that this research is in the public interest. Those of you who read EARLY MAN MAGAZINE know how my proposal to establish National Archeological Research Topics was received (King 1982; Adams et al. 1982). I assure you that the last shot has not been fired in this skirmish, however. I suggest to you that archeology really has only two options. AB 952 has shown us how much the traffic will bear: the current proposition--that every piece of salvage research should be funded that a given contractor thinks is nice--cannot persist, nor should it, because it commits us to the trivialities conceivable by the simpletons who are the lowest common denominator among archeological consultants. Our options are to define our own limits, based on our own research priorities, or to have those limits defined for us. In California, I fear, you have gone far toward the second option.

Our failure to make our research goals defensible in the public interest is akin to another failure--the failure to involve the public in our work. Public interest in archeology remains high, and there are impressive examples of public involvement programs scattered around the country. Stuart Struever's is the largest and most obvious example. The Alexandria, Virginia, Urban Archeology program is another, involving hundreds of people every year in the process of archeology. Here in California there are--used to be, anyway--a plethora of good avocational societies; I wonder how many of you who do contract work systematically involve such groups. How many of you subcontract with them? How many of you have integrated your studies with their interests? As a rule across the nation, the record is bad. Contractors insist that archeology is a "professional" activity--meaning, apparently, that each project should be supervised by someone marginally meeting the minimal Park Service "professional qualifications standards" and be carried out by shovel-bums. Involving avocational groups and the public generally is held to be too complicated--lots of supervision problems,

problems of liability, and all that. I think that's nonsense; I think that on the whole we're just too lazy to make the effort. I don't think the complications are insurmountable, and I think the payoffs could be great.

Finally--and I mention this last in order to emphasize it--is our failure, those of us who deal with the leavings of Native American societies, to relate positively to the public that's descended from those we study. We should have common cause with Native Americans, not because our goals are identical--I think that both parties should by now be sophisticated enough to know that allies need not have identical goals--but because we have a common problem in seeking accommodation between the things in and on the ground that we treasure and the needs and processes of the modern world. California has been sad to watch, these last few years, because however legitimate or illegitimate the Indian position may have been, the archeological response has typically been naive, hostile, or both. On the one hand, there are among you, people who apparently think you can't be a proper practicing archeologist unless you adopt blindly the most radical available point of view that passes for that of a Native American group. Conversely, there are those who refuse to believe that the 1950s have passed, and insist that as scholars we can ram down the throats of Native Americans a set of assumptions that is manifestly unpalatable to them. There is a middle road. One can, I believe, frankly acknowledge that one is not a Native American, and reject the proposition that defect must carry with it a load of guilt, but extend to Native American groups the hand of respect and cooperation. I do not believe that the archeological community should accept the idea that we need a Native American observer looking over the shoulder of each fieldworker. On the other hand, I do not believe that we can or should cling to the notion that the bones and grave goods of a Native American community belong in perpetuity to us and not to the descendants of that community. I think we should stand on the principle that we will not allow significant data to be destroyed without study simply because a Native American group doesn't believe in western science but at the same time, we must accept the rights of Native American communities to control the disposition of their ancestors' remains. We must excavate and study cemeteries that are threatened with destruction, but, we must do so with respect for the dead and their descendants, and we must, if those descendants so desire, give up to them the bones and the grave goods.

The failures that I have discussed this evening are by no means exclusive to California. If you have read the General Accounting Office's study of archeology's national failures (Comptroller General 1981), you will know that your problems are not isolated. I want to end my comments on an optimistic note, however. I think you have an opportunity, right now, to remedy many of the failures of the past, and to show the rest of the nation how to do it.

Your legislature has passed AB 952, a law that in my opinion has many defects. Blemishes notwithstanding, I think AB 952 addresses a real problem, and presents a real opportunity. Section 21083.2 of the Public Resource Code, added by the Act, limits data recovery to "unique" sites and defines "uniqueness." This is an effort, however arbitrary, to put a cap on what is perceived to be the ability of archeologists to rob developers blind. There is no use rallying against this perception; it is a fact of life. I suggest your attention, however, to Section 21083.2(g)(1), which defines a "unique" site as one which: "Contains information needed to answer important scientific research questions." Who is going to decide what kinds of sites fall into this category? The County Board of Supervisors? The Mayor? Every practicing consultant regardless of interests, qualifications, and integrity? If any of those things happen, you will have missed a tremendous opportunity. The legislature is saying to you, get your act together, archeologists, tell us what's important to you, and justify your judgement, and the law will support you in addressing it.

You have, in short, in Section 21083.2(g)(1) an opportunity, an invitation, to establish research priorities. This inevitably is also an invitation to undertake regional planning, because it is in the context of regional plans that priorities can best be set and the contributions of given site-classes to given research goals recognized. In Section 21083.2(c), moreover, the section that prescribes that only half of the cost of data recovery must be put up by the developer, you have an invitation to organize and make common cause between avocational and professional. It seems to me you have two ways to cope with this section. First, all the consultants can double their basic rates and then "contribute" half their time to each project; if you allow this to happen you will have failed sadly, and the legislature is not so stupid that it will fail to notice. Second, you can organize local and regional groups that will donate legitimate services, such as the hard work of avocationalists, to help provide the "match." That would be a positive solution. It would, I believe, meet the intent of the legislation while providing the grass-roots support and involvement you need to keep worse solutions from being imposed upon you.

You also have, in the so-called "burial bill," SB/297, an invitation to detente with the Native Americans. I notice that Willy Pink, in a recent issue of San Diego State's "Casual Papers," has once again held out an olive branch to you on this matter (Pink 1983), and I hope you will take it. The bill provides for cooperative agreements with the Native American Heritage Commission to handle burial discoveries. In the context of statewide and regional planning, in the context of local and regional organization to address the mandates of AB 952, you surely could work out a set of standing cooperative agreements with the Commission, to ensure that burials are handled in a sensible, balanced, systematic manner that recognizes both their scientific value and their cultural, emotional importance to Native Americans.

Before leaving Washington last week, I made two phone calls. The first was to Chuck Redman, the National Science Foundation's current resident archeologist. I read him Section 21083.2(g)(1), of the Public Resources Code, and asked him if NSF would receive with interest a proposal to address it by establishing state and regional research priorities. He said, of course, though he hedged a little by saying such a proposal would have to be carefully phrased. The second call was to Larry Aten, Chief of the National Park Service's Interagency Resource Management Division, which sets the standards for and reviews State Historic Preservation Officers functions., I told Larry about Section 21083.2(g)(1), and asked him if he would welcome a proposal from your SHPO to undertake planning to address that Section's requirements. He said, as I knew he would, that of course his Division would welcome such a proposal.

I made these calls, and mention them tonight, to demonstrate to you that if you can organize to do planning, to set priorities, there is Federal support available to you. In my own capacity, since the Advisory Council has no money to provide, I can only say that if you can come up with responsible statewide and regional plans and priorities, we will do everything we can to tailor our implementation of Section 106 to them, that we will encourage Federal agencies to help you, and that our staff in Denver and I will do whatever we can to advise and assist. Knowing the directions being taken at the National level by agencies like the Forest Service and the Corps of Engineers, I think you can count on their support as well. It is up to you, however, to organize for the purpose.

In 1967, Eric Barnes concluded his remarks to the SWAA like this:

...the archeologists of California are challenged to an exercise of practical imaginations in the preparation of workable plans for regional research. By working together and with others they can meld their own efforts with maturing social opportunities in a pilot program proposal that can have national significance for the future form and social purpose of public archeology (Barnes 1977).

Those words are as true today as they were 16 years ago. We did not effectively heed them in 1967--perhaps, under the circumstances of the time, we could not have--and we have suffered the consequences. Today you--and all of us who remain Californians at heart or who look to California for models of innovation and creativity--have another opportunity, and I urge you to seize it.



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NEW RESEARCH TRENDS IN SAN DIEGO PREHISTORY  
SYMPOSIUM INTRODUCTION

The following collection of papers includes a number of the presentations made in a symposium entitled "New Research Trends in San Diego Prehistory" held at the Society for California Archaeology 1983 Annual Meeting. I chaired this symposium because it was my impression that a great deal of new and interesting work was being conducted in the area that was not being made generally available. These papers (Bull, Minshall, Hanna, Ezell, May, Borst & Olmo, and Hector) fulfill a part of that need by either presenting new data and research or rethinking already existing concepts and information. Several are controversial and will stimulate responses. Others should prove to be thought-provoking. I hope that all are interesting and informative.

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## SHAKING THE FOUNDATIONS

### The Evidence for San Diego Prehistory

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#### INTRODUCTION

Research into the prehistory of San Diego County began in the early 1900s with the work of Malcolm J. Rogers. Mr. Rogers' pioneering work in the archaeology and prehistory of the area has served as the primary focus for the great majority of subsequent analyses performed in the area. In 1929, his article in American Anthropologist, "The Stone Art of the San Dieguito Plateau," presented three archaeological patterns which, with modification, are still employed today. For lack of a better term, this construct will be referred to as the Rogers Triad and will serve as the unifying thread for the present discussion.

Recent archaeological investigations and reports on San Diego County, particularly those produced in conjunction with the environmental assessment process, have presented past archaeological research in a "culture history." It has been the tendency of these documents to provide a framework of prehistory to which specific data is applied. In a sense, the model for the cultural development of San Diego County has become the foundation with which data is to be evaluated, rather than the reverse.

The following discussion is an attempt to modify the procedure of discussing local prehistory and is an approach which is philosophically oriented toward evaluating the construct with the data rather than vice versa. To accomplish this, it is necessary to discuss, in some detail, the actual evidential basis for the prehistoric framework presently employed. In this way, it will be possible to provide a critical evaluation of both the existing data base and the generally accepted creation.

While information presented to date as "culture histories" has been organized around temporal and spatial development within the San Diego region, the present discussion will approach local archaeology as a history of the research which has gone into the prehistory synthesis, not as a history of the people who created the materials. In this manner, both the validity and reliability of the final product can be evaluated. In addition, suggestions for specific research direction can thus be formulated.

The following presentation of archaeological research will be divided into four basic periods. The first is pre-1950 and is arbitrarily termed the Rogers Era. The second, the Expansion Era, encompasses those works from 1950 through 1958. From 1959 on, a large influx of archaeology appeared in publication, but for this presentation, this period will be split into a past- and pre-California Environmental Quality Act (CEQA) era, resulting in a 1960 to 1972 pre-CEQA period and a 1972 to present post-CEQA period. The 1972 date is the year of the "Friends of Mammoth" decision and marks the actual proliferation of archaeology resulting from CEQA.

Obviously, it is not possible or even desirable to include a discussion of all archaeological publications and reports. The emphasis in this discussion is on the significant contributions to prehistory of Southern California in general and coastal San Diego County specifically. This review will deal only with prehistory. A great deal of work has been done on other archaeological and anthropological questions, ranging from functional and technological questions to systems modeling. These research interests are very important to local archaeology; however, they are not addressed here.

#### MODIFICATION OF THE ROGERS TRIAD

##### The Rogers Era: Pre-1950

Prior to 1950, the vast majority of the archaeology appearing in the published record was done by Malcolm Rogers. As noted above, the first of his pioneering works was his 1929 article, "The Stone Art of the San Dieguito Plateau." With the identification of the Mission Indians, shell midden people, and scraper-maker association, this work served as the guiding publication for subsequent prehistoric investigations. In this article, Rogers identifies two groups within the Mission Indians classification: the Yuman and the Shoshonean. The scraper-maker and shell midden classifications are used to describe "two distinct and older cultures throughout the coastal belt" (1929:456), and it is these two archaeological associations that the article addresses in detail. The 1929 publication is basically a summary of the work done by Rogers and does not present specific data. The assemblage for each category is discussed in general terms and is subjectively evaluated.

The shell midden people pattern is described from surface evidence and includes:

. . . metates, manos, hammerstones, teshoa flakes, and a great amount of split stone, but no chip stone artifacts which may be recognized as finished implements, unless it be the teshoa flakes. Potsherds are entirely absent, but occur sparingly in the vicinity. Many of the middens produce nothing that would associate them with man except split stones (Rogers 1929:457).

The scraper-maker association includes artifacts that Rogers groups into three generic types: scrapers, knives, and ceremonial stones (1929:458).

At the time of this 1929 presentation, Rogers felt that the shell midden people predated the scraper-maker people. He felt that:

[the] scraper-maker culture can be connected through its chip stone industry with still another culture, viz., that of the shell midden people; in fact, there is some evidence that the former is an outgrowth of the later (1929:456).

He also indicated that in some areas scraper-maker sites blend into shell midden sites without a break. It was his concluding suggestion that:

If on the exhaustion of seafood on the adjacent coast and in the contiguous lagoons, a shell midden people were compelled to move inland in search of food, it seems likely that in their economic readjustment they would become a hunting group, specialize the teshoa flake into a more perfect fleshing tool such as the scraper plane, and later develop the knife (1929:467).

After a nine-year hiatus, Rogers produced a brief article indicating he was conducting excavation in the San Dieguito River Channel at what was to become known as the Harris site (Rogers 1938). The following year, the San Diego Museum of Man published "Early Lithic Industries of the Lower Basin of the Colorado River and Adjacent Desert Areas" (1939). In this publication, Rogers reterms the scraper-maker, referenced in the 1929 article, as the San Dieguito industry and includes it as a part of the Playa industry. He indicates that the San Dieguito and Playa industries were identical in most every respect, except that stemmed blades were not present in the western area (Rogers 1939:28).

Six years later, in 1945, Rogers presented an outline of the original development of the Yuman culture complex. Yuman is a language family of the Hokan stock spoken by people located in extreme Southern California and portions of Arizona. It was the goal of the 1945 Rogers article to help establish an explanation of Yuman prehistory. As with his 1929 article, this discussion is primarily a summary of experience gained through his substantial fieldwork. There is a limited amount of data presented concerning the development of ceramics within the Yuman pattern, which, for the Colorado River basin area, was divided into Yuman I, Yuman II, and Yuman III. The evidential foundation for the presentation is not provided.

The summary of preceramic origins for the Yuman pattern relabels the shell midden people described in 1929 as the La Jolla culture pattern and divides it into two phases, La Jolla I and La Jolla II (Rogers 1945:171).

He states that the La Jolla II phase of the La Jolla pattern was a "local Yuman group" (1945:173). As proposed in the 1945 publication, Rogers indicates that at the time of its inception, "the La Jolla material pattern as known consisted of: a basined metate, unshaped mano, a few primary flakes of stone, and an even lesser number of crude, beach cobble choppers" (1945:172). Furthermore, unsegregated interment without mortuary offerings was the referenced method of disposing of the dead.

La Jolla I blends with La Jolla II and is marked by the improvement of flaking techniques, the increased use of metates, and the introduction of some new tools. It is noted that:

. . . the La Jolla II pattern was evolved gradually through the agencies of technology and culture enrichment, with the result that there is no abrupt change in the nature of the middens and their contents, except in instances wherein middens of the first and second periods are separated by natural formations (1945:172).

Rogers does not indicate which sites had the referenced natural interruption of the pattern. He continues in describing the development of La Jolla II and indicated that burials became more segregated and true cemeteries were formed. There is also an indication that trait connections were:

. . . affected either with the Channel Islands natives or an intermediate littoral people to the north such as the coastal Shoshoneans, with a result that typical Channel Island shell beads and stones digging weights were included with burials, which were marked with one or more inverted metates (1945:172).

It is obvious that Rogers had completed an in-depth evaluation of a great amount of archaeological evidence to arrive at these conclusions. What that evidence was and how, in fact, he felt it reflected the cultural development described cannot be determined from this publication. This material was presented as an outline and was only considered by Rogers as "a condensed version of a future and more detailed report on the southwestern archaeological field of considerable geographic extent and cultural complexity" (1945:167).

One additional point needs to be made at this time. It is here that the first reference to the archaeological effects of the "Shoshonean wedge" appears. This is the explanation for the appearance of Uto-Aztecan speakers in Southern California and is called a "wedge" because it separates two Hokan language families, the Yuman and the Chumash. Rogers suggests that:

. . . this movement seems to have split the Yumans off from the Chumash and was possibly contributory to the momentum which carried the Yumans across the California desert to the valley of the Colorado (1945:170).

This is mentioned here because it plays a significant role in the formation of future discussion on coastal San Diego County archaeology.

In summary, Rogers' 1945 article renames the shell midden people the La Jolla, indicates that the San Dieguito complex occurred prior to the appearance of the La Jolla, and suggests that the La Jolla pattern was in fact the preceramic Yuman pattern. In addition, it separates the La Jolla pattern into two phases, La Jolla I and La Jolla II, and the Yuman pattern into three phases, referenced I, II, and III. It is important to remember that while this is the result of great effort on the part of Malcolm Rogers, the evidential foundation for the conclusions is not provided. It is therefore difficult to assess the accuracy of his conclusions.

Soon after its appearance, Rogers' work was referenced in other literature. Perhaps most important at this time is the discussion of the San Dieguito presented by Heizer and Lemert (1947) and Treganza (1947). Discussing the investigations at Topanga Canyon, Heizer and Lemert propose strong ties between the Topanga Canyon materials and Rogers' San Dieguito pattern: "We believe that Topanga Canyon may now be counted as the most northerly known occurrence of the San Dieguito pattern" (Heizer and Lemert 1947:250).

In the same year, Treganza provided an expanded range for coastal San Dieguito materials:

It now becomes obvious that the San Dieguito lithic industry in Southern California was widespread. By virtue of Heizer's finds in the Santa Monica Mountains and Sauer's and Massey's discoveries well into lower California, the area has been considerably extended (Treganza 1947:254).

The above points are stressed because they are both made with information from the Topanga Canyon region. As will be discussed below, the Topanga Canyon pattern is described by a milling-bearing site, a trait of extreme importance to future understandings of local prehistory.

Although Rogers produced another article in 1948, the 1929, 1938, and 1945 articles represent the foundation for the future development of archaeology in the San Diego region. With the onset of the 1950s came a proliferation of archaeological publications and further expansion of understanding of prehistory in Southern California. Malcolm Rogers' death left unpublished important knowledge relating to the ancient settlement of this region.

Expansion Era: 1950-1958

In 1950 the first archaeological publication of San Diego County material by an individual other than Malcolm Rogers was produced--George Carter published a brief article in late 1949 on work in La Jolla, but it was the 1950 article which really marked the onset of the expansion period of San Diego prehistory. Carter's work appeared at various times throughout the 1950s and culminated in the 1957 publication Pleistocene Man at San Diego.

In 1954 Clement Meighan published the results of his work at SDi-132. It was his purpose to provide a definition of a late archaeological horizon as seen through the investigation of this site (Meighan 1954:215).

In this publication, Meighan presents the summaries and materials recovered through excavation of SDi-132. Two basic points are developed as a result of the qualitative evaluation of this site as compared to other Southern California resources. First, the evaluation is seen as eliminating the problem in the introduction of ceramics into the region, and second, it works with the definition of two phases of late occupation.

The assessment of the arrival of pottery is made in relation to two assumptions. The first is that "the midden itself is definitely pre-pottery in age. . ." (Meighan 1954:220) and the second is that the site is apparently ". . . extremely late in time" (1954:221). The contention that the site is preceramic is based on the absence of ceramics in the midden, a correlation which is not necessarily supportable. The late date for the site is based on two considerations: one, that "the midden is black sooty and dusty showing no alteration which might be attributed to age" (1954:221), and two, that the artifact assemblage is quite similar to sites with known late dates. The association of SDi-132 with other assemblages is done on a qualitative basis and lists a series of shared artifact types between many resources.

Little confidence can be placed in the assumption that the site is preceramic. The absence of ceramics, particularly at a small site, may well be the result of the type of site use. Because of this, the assessment of the introduction of ceramics into the region using the limited published results of this report is nonconclusive.

The second concern addressed in this publication is the definition of two complexes: San Luis Rey I and San Luis Rey II. The assemblage described in the report is that of San Luis Rey I and differs from San Luis Rey II assemblages solely in the appearance of ceramics during the San Luis Rey II period. Meighan differentiates these patterns from Rogers' Yuman III and lists characteristic differences.



As with some of Rogers' early works, Meighan's article provides little analytical foundation between observed data and preliminary constructs. It must be viewed as a subjective assessment based on extensive personal experience. The presentation of data from SDi-132 in a more complete form could provide a foundation for a quantitative discussion of this site in the Southern California milieu.

The next publication dealing specifically with San Diego County materials appears in 1958 with the work of D. L. True. True summarizes the results of investigation of approximately 25 sites, which he considers, "because of their distinctiveness," to be older than Meighan's San Luis Rey I or II. This "early" pattern is labeled the "Pauma complex" and is "characterized by crude chipped stone implements and grinding tools which reveal similarities to the San Dieguito complex (M. J. Rogers 1929), as well as to Topanga, Oak Grove, and other early remains from areas to the north" (True 1958:255).

True presents some data on surface artifacts recovered from Pauma complex sites. The collections are described in some detail and are qualitatively compared with materials of other patterns and variance.

The discussion of the Pauma complex is presented primarily in comparison with the San Luis Rey I and San Luis Rey II complexes described by Meighan in 1954. The general assumption for the evaluation of this complex is best summarized as follows: "At present, a rather abrupt break and considerable separation in time between the Pauma complex and San Luis Rey I is indicated by obvious dissimilarities in artifact types and site locations" (True 1958:257). The "obvious dissimilarities" appear to be related primarily to the absence of the mortar and pestle and small pressure-flaked projectile points from Pauma sites with their presence on San Luis I resources.

That there are differences between the materials described by True in 1958 and those described by Meighan in 1954 is evident. The temporal and cultural significance of these differences, however, cannot be gleaned from the two reports themselves. The fact that the sites identified as Pauma complex resources occur in ecologically different settings than do the San Luis Rey resources (True 1958:257) could conceivably indicate an environmental rather than temporal distinction. This is not to say that the temporal arguments made by True are in error but simply that the foundations for the conclusions drawn are not adequately presented. However, the inability of the reader to use the summaries presented both by Meighan and True in assessing the conclusions drawn in the latter article is probably as much a result of the scope of the articles as the omission of in-depth analysis evaluation between assemblages. As is indicated in True's acknowledgment, much of his understanding of the San Luis Rey and its application to the Pauma complex comes through personal communication with Meighan.

One additional point needs to be made concerning True's 1958 article. In comparing the results of his investigation with other Southern California patterns, he concludes that:

Nearly all sites in the complex seem to fall generally into Wallace's (1955) Milling Stone Horizon II. However, Wallace (1955: 219-221) has separated the San Dieguito Complex from his Horizon II and placed them in Horizon I early period. For some inland manifestations of San Dieguito, at least (for example, Rogers Escondido Locus 4), this may create some questions since nearly all elements described by Wallace as being typical of the Milling Stone complexes in Horizon II are found on nearly all San Dieguito sites examined by this writer. This is not to suggest that Wallace's dating of San Dieguito is in error, but more to emphasize the milling stone elements apparent in at least some San Dieguito sites (True 1958:262).

This is brought up at this time because it makes apparent the confusion surrounding the definition of San Dieguito sites in San Diego County. This confusion is further illustrated by the fact that True reported a ground-stone perforated disk at one of the Pauma sites and although "no similar specimens have been found within the complex, . . . an artifact similar in size and description was found on a San Dieguito site near Escondido" (True 1958:259).

North of San Diego County, the the 1950s also saw an increase in the productivity of archaeological research. In 1951 Edwin Francis Walker published a report of five archaeological sites investigated between 1936 and 1945. These sites are located in the San Fernando Valley, at Malaga Cove, near the Sheldon Reservoir in Pasadena, at Chatworth, California, and at the Big Tujunga Wash. The publication is primarily a description of the results of the investigations. It therefore adds little to the understanding of prehistoric development. The information presented, however, supplements some of the later investigations.

In 1955, the in-depth report of an excavation of a site at Zuma Creek was prepared by Peck. The purpose of this investigation was to "salvage as much information as possible from sites at Zuma Creek which were threatened with obliteration by a real estate development project" (Peck 1955:XIII). Although some comparison is done, it is the ultimate conclusion of this report that "in the main, this treatment of the material found at the Zuma Creek site has been descriptive. This was necessary because of the lack of published data on which to base adequate comparisons" (Peck 1955:83). The report does provide in-depth descriptions of the results of the investigations and has been used by later investigators for comparative discussions. It has become an important contribution.

One of the latest presentations of data in the 1950s for the coastal area of California north of San Diego County is the report on the final excavations of the Tank site (Treganza and Bierman 1958). This report describes a site which does not completely conform to other sites previously known for the general environment and describes what is referred to as "the Topanga Culture." It is a summary of excavations done in the late 1940s and was presented following other papers and articles on the Topanga culture (Treganza 1950; Treganza and Malamud 1950; Heizer and Lemert 1947).

As with some of the earlier works, this final Topanga report ". . . remains primarily descriptive in order to make the data more usable for comparative purposes" (Treganza and Bierman 1958:72). However, it does propose some internal structuring which has significance for general Southern California prehistory.

The results of the Tank site investigations are summarized as reflecting two phases of the Topanga culture: "The two phases of the Topanga Culture are derived primarily through differences in projectile points and burial customs" (Treganza and Bierman 1958:72). These characteristics are summarized as follows:

Certain elements in the Topanga Culture might be viewed as "index artifacts" when they occur as associates. To have comparative value it is the combination of traits which create the culture pattern and not the isolates. Phase I is characterized by combination of extended burial with the head south, reburial of long bones under metate, fractional burial, percussion flake projectile points and blades, predominance of flake and core tools, dominance of milling stones with wide variation in the handstone (mano), crescentic stones, stone cogs, and stone discoids. The latter two may occur late in this first phase. Phase II has flexed burials with no specific orientation, an occasional rock cairn in association, pressure-flaked projectile points constituting several types, and dominance of the cobble mortar and pestle as milling implements, though the latter occur toward the end of Phase I. (Treganza and Bierman 1958:72).

While not the primary purpose of the Topanga culture report, the authors make some attempt at associating the Tank site remains with other excavations, variants, and patterns. Of particular interest at this time is the suggested correlation between Phase I of Topanga and the San Dieguito of San Diego County (Treganza and Bierman 1958:75). This correlation reflects the fact that the San Dieguito complex defined by Rogers (1929, 1938, and 1939) is not yet clarified in the literature.

The first effort to integrate prehistoric explanations for the Southern California area as a whole was done by Wallace in 1955. In this article, "A Suggested Chronology for Southern California Coastal Archaeology," Wallace proposes the existence of four horizons: Horizon I, Early Man; Horizon II, Milling Stone; Horizon III, Intermediate; Horizon IV, Late Prehistoric. This clearly presented outline goes a long way in relating like patterns throughout Southern California. Being based on information in the literature prior to 1955, however, it is not firmly based and must be seen as a format for future evaluation.

A brief summary might be useful at this point to help clarify the many positions taken by the varied authors to Southern California prehistory. Triads were initially proposed by both Malcolm Rogers and David Bank Rogers for San Diego and Santa Barbara (Rogers 1929; D. Rogers 1929). These suggestions summarized much personal effort, but the publications provide little in the way of specific foundations to support the suggestions.

In addition, Dr. Ronald Olsen (1930) provides an excellent foundation for the definition of three mainland periods and two Channel Island periods in Southern California. The degree to which these periods can be extended to other archaeological sites has not been addressed.

When reviewing the articles produced after 1950 and prior to 1960, it is easy to see that there is still much uncertainty as to the archaeological constructs being employed. True (1958), Wallace (1954), Treganza and Bierman (1958), and Peck (1955) all consider certain milling tradition sites as potentially related to the San Dieguito complex defined by Rogers.

Of six primary contributions between 1950 and 1958, three propose new complexes: San Luis Rey I and II, Pauma, and Topanga. All indicate the difficulty in assessing new information because of the lack of well-presented previous data. For example:

There are four cultural assemblages in the coastal district of Southern California which bear considerable resemblance to the Little Sycamore material. A trait-by-trait comparison is not possible because, of the four, only the Topanga culture is described in any detail (Wallace 1954:2).

In the main, this treatment of the material found at the Zuma Creek site has been descriptive. This was necessary because of the lack of published data on which to base adequate use comparisons (Peck 1955:83).

Table 1 illustrates the differing patterns and variants proposed prior to 1960. It can be seen that with ten primary contributors, nine have suggested different constructs from mild to extreme variation. This reflects the fact that little is known and even less is agreed upon. The

TABLE I  
PATTERNS AND VARIANTS PRIOR TO 1960

M. J. Rogers (1939, 1945)	D. B. Rogers (1929)	C. Meighan (1954)	D. L. True (1958)	Treganza and Bierman (1958)	Walker (1951)	Olson (1930)	Wallace (1955)
San Dieguito			Pauma	Topanga I Topanga II	Malaga Cove I Malaga Cove II		Early Man
La Jolla I	Oak Grove					Early Mainland Early Island	Milling Stone
La Jolla II	Hunting					Intermediate Mainland	Intermediate
Yuman III	Canalino	San Luis Rey I San Luis Rey II			Malaga Cove III Malaga Cove IV(?)	Late Mainland Late Island	Late Prehistoric
Sample: Unstated	Sample: Unstated	Sample: 1 site subsurface	Sample: 25 surface sites	Sample: 2 sub-sur- face; 28 briefly reviewed	Sample: 1 site subsurface	Sample: 14 sites subsurface (5 sites intensive)	Sample: Unstated

data presented defining these elements is occasionally internally consistent and well presented, but rarely has the work of the different authors been adequately integrated.

Besides the lack of agreement concerning pattern definition, one point needs to be stressed at this time. It has been a tendency to base the definition of "complexes" and "cultures" on a limited sample--often on only one site. Temporarily ignoring those discussions which do not specify a particular sample, three of the five remaining constructs result from a sample of one, the fourth from surface sites only, and only the work of Olsen (1930) involves a detailed investigation of more than one subsurface deposit. While a discussion of sampling procedures is not within the scope of this presentation, it should be remembered that there are inherent difficulties with the "type site concept" often employed. Also, the results of these important investigations should not necessarily be considered representative of the area at large.

#### Pre-CEQA/NEPA Archaeology: 1959-1971

During the decade following the 1950s, there was a substantial increase in the number of archaeological publications. By far, the great majority of these were produced in the results of the Archaeological Survey of the University of California, Los Angeles (UCLA). While it is impossible to discuss each of these articles independently, several are of particular interest for the prehistory of San Diego County.

In 1960, Dr. William J. Wallace published the results of an archaeological survey around the Buena Vista Lagoon in San Diego County. The survey was conducted in order to gain a picture of the aboriginal occupation of the Buena Vista watershed. In all, 37 sites were examined, of which five were tested with one or two units each. The artifacts recovered are described in some detail in the report and are summarized as being "remarkably homogeneous throughout the Buena Vista Watershed" (Wallace 1960:286). Wallace uses the results of his surface investigation to place Buena Vista watershed sites in relation to other previously defined patterns in Southern California. As such, he indicates that "artifacts from 3 Buena Vista Watershed sites (BVW 5, 15, 19) fall well within the range of this late artifact assemblage [San Luis Rey complex]" (1960:287). Eight sites are ascribed to the Pauma complex (1960:287). Wallace concludes:

Thus, there appears to have existed in the watershed 2 distinct occupations: one represented by a late San Luis Rey (Phase II) assemblage and the other by the more ancient Pauma complex. The 2 seem to be widely separated in time and no assemblage falling between them has yet been definitely isolated in the area. It is quite possible that some Buena

Vista sites do represent an intermediate complex such as the La Jolla of coastal San Diego County (Rogers 1945:172; Harding 1951). M. J. Rogers indicates the presence of La Jolla sites (designated as shell midden) in the Buena Vista Creek area (Rogers 1929:455) which is dominated by handstones, milling stones, and cobble choppers . . . (Wallace 1960:287).

In 1961 Warren, True, and Eudey:

. . . undertook a survey of a portion of western San Diego County in an attempt to locate sites and to obtain a sample of the artifacts from certain areas [in order to] increase our knowledge of the early Milling Stone complex of the San Diego coast (1961:1).

The results of their investigation are divided into five areas: the lower San Dieguito River valley, Batiquitos Lagoon, the Valley Center Plateau, Escondido-San Marcos area, and Green Valley area. Descriptions are provided of both the environmental and archaeological conditions of the regions. No discussion is provided concerning the nature of the fieldwork undertaken, and thus the reliability and validity of the results are difficult to ascertain. Artifacts are described in some detail and are presented by type and site. These results are, in discussion form, presented by ecological area.

This article goes one step further in confusing application of a prehistoric framework with evaluation. It is apparent that, although some effort is made at assessment (1961:27-28), the general thrust of the article fits the recovered data to the existing framework. This is apparent in the introduction to the article when the authors state:

. . . in addition to the sites belonging to the early Milling Stone Complex a number of late "Yuman" or "San Luis Rey" sites containing pottery and/or small projectile points were located . . .

The Pauma complex defined by True (1958) from the Pauma Valley area, while containing some San Dieguito traits appears to have much closer affiliation with the coastal La Jolla material, which suggests it is a related inland complex of these peoples. Both La Jolla Complex and the Pauma Complex fall within the early Milling Stone Horizon as described by Wallace (1955) (Warren, True, and Eudey 1961:1).

Using the La Jolla and Pauma definitions, the authors proceed to evaluate the relationship between the two. This discussion is valid only inasmuch as the constructs themselves are valid.

In explaining a potential relationship between Pauma and La Jolla, Warren, True, and Eudey propose a somewhat complex explanation of the relationship of early coastal occupants and their ecological setting. Using dates from Scripps Estates Site 1, the Sorrento Valley site, and the Del Mar site, as well as sites around Batiquitos Lagoon, they propose that:

It appears from all available evidence that the La Jolla Complex reached its population and cultural climax between 7000 and 400 years ago when there was a plentiful supply of shellfish in the lagoons along the coast. Approximately 4000 years ago lagoons were silted in enough to reduce the number of shellfish available to prehistoric populations of the area.

The dating of the silting in of the lagoons is tentative and undoubtedly varied from lagoon to lagoon depending upon the immediate topography and the amount of silt deposited by the streams feeding into them . . . .

From about 2500 to 3000 years ago it appears that Batiquitos Lagoon could no longer support a supply of shellfish adequate to maintain any sizeable aboriginal population. Not only do the carbon dates suggest this depletion of aboriginal population, but only very scanty remains of the recent San Luis Complex have been located on the margins of this lagoon (Warren, True, and Eudey 1961:25).

Temporarily ignoring the problems with pattern definition, this explanation is in itself weakly established. The lagoon siltation concept was discussed the same year in much greater detail by Shumway, Hubbs, and Moriarty (1961). In their report on the Scripps Estate site, they presented detailed explanation of sea level, siltation, and molluscan biology and related these to cultural materials found on the coast of San Diego County.

The basic purpose of the Scripps Estate report is to provide a description and interpretation of an early burial site (Moriarty, Shumway, and Warren 1959:189). The final report (Shumway, Hubbs, and Moriarty 1961) describes in detail activities undertaken and materials recovered. As with the Moriarty, Shumway, and Warren article (1959) and the Warren, True, and Eudey article (1961), this report also accepts the concept of the La Jolla pattern and uses the results at the Scripps Estates site to refine relationships and understandings.

The La Jolla pattern has only been tentatively identified until this time by Malcolm Rogers (1929, 1945), Harding (1951), and Carter (1957). Far from being well defined, evidence for the La Jolla pattern is, at best, incomplete. The problem with its acceptance is illustrated in 1959 through



the "placement" of the Scripps Estates site into a cultural chronology. The concept of placing a site in a framework necessarily requires the acceptance of the framework.

Warren and True, also in 1961, provided discussion of work done at the Harris site on the San Dieguito River in San Diego County. The purpose of this investigation was to "obtain a sample of artifacts and to increase our knowledge of the controversial San Dieguito complex, which has been renamed and reclassified several times without being adequately defined" (Warren and True 1961:246). To investigate this site, the authors used a bulldozer to clear a trench down to the artifact-bearing gravels beneath an upper midden and sterile overlay. The resulting data is presented in accordance with three components: the San Dieguito component, the La Jolla component, and the Yuman component. As with those articles discussed above, this use of terminology can be questioned. However, it is recognized that the three components are based on physical stratigraphy and that the names applied are those used by Rogers (Warren and True 1961:259). There is a tendency, however, to apply the data to the construct, rather than evaluate the construct with the data.

The difficulty with fitting the data to the construct is well illustrated in the author's discussion of the radiocarbon dates obtained from the Harris site. The date of  $4720 \pm 160$  B.P., referenced for the San Dieguito component of the Harris site (Hubbs, Bien, and Suess 1960:220; Warren and True 1961:260), is rejected for a series of reasons. First, it was assayed 21 years after it was collected. Second, and most important, it falls after a series of dates obtained on "La Jolla sites" identified on the San Diego County coast. Given these points, the date is rejected rather than the construct. Of course, the 4720 B.P. date may in fact be invalid, but the reliability of this assumption is marginal at best. As is illustrated in this 1961 report, it is quite possible that the date is a result of sampling error, particularly since a date of  $6300 \pm 200$  B.P. is presented for the "La Jolla II Stratum 2-C" (Warren and True 1961:260). Which date is in error, if either, is left to conjecture. The authors suggest that since the "La Jolla Stratum" has a date which corresponds to coastal resources, the latter date is in fact the correct one. The conclusion that "... on the basis of carbon dates the La Jolla complex can be given an initial date of about 7,500 years ago and the San Dieguito dates from at least 8,000 years ago and maybe as old as 11,000 years" (Warren and True 1961:261) has little documented foundation.

Several interesting conclusions are drawn concerning the relationship of the San Dieguito pattern, as illustrated at the Harris site, and the other patterns and variants of Southern California. In this discussion, Warren and True (1961) relate the Harris site finds to the materials from Malaga Cove, La Jolla, Oak Grove, and Topanga Canyon.

The most substantial of these comparisons is between the San Dieguito and the Topanga Canyon culture. As discussed above, the Topanga culture was related to the San Dieguito by Heizer and Lemert (1947), Treganza and Bierman (1958), and Treganza and Malamud (1950). Warren and True reject this association because there are:

. . . only a few similarities between it and the San Dieguito complex. These include the heavy, leaf-shaped blades of Phase I, scraper plane IA, the crescentic stone, and possibly the core hammers. Most of the artifacts of the Topanga culture show little resemblance to the corresponding categories of the San Dieguito materials. Furthermore, the importance of milling stones and the relatively small number of blades (knives) in the Topanga assemblage is in direct contrast to the artifact complexes described for San Dieguito, Playa, and Lake Mojave (Warren and True 1961:265).

It is their position that with the association of the La Jolla and Pauma complexes, there is a reasonable correlation between the Topanga culture and the Milling Stone stage patterns in the San Diego area.

Again, it must be noted that this tie between the La Jolla and Pauma complexes is one which is difficult to evaluate. Therefore, association of Topanga and Pauma would not necessarily entail an association between Topanga and La Jolla. Similarly, there is little in the literature discounting the association between Pauma and San Dieguito other than Warren, True, and Eudey's 1961 article.

With the general acceptance of a prehistoric framework comes the inherent problem of associating archaeological constructs with actual cultural systems. While it is quite reasonable to label certain assemblages, it is another thing to assign cultural significance to those constructions. Indicating an apparent difference between patterns labeled Topanga, San Dieguito, Oak Grove, and so on is reasonable; however, equating those differences to different peoples and different cultures is a substantial step in logic.

Warren and True participated in another investigation of the coastal areas of Southern California in 1963. With the addition of Robert Crabtree, an investigation was made of two sites on Batiquitos Lagoon, SDi-211 and SDi-603. This investigation was done under the Federal Highway and Revenue Act of 1956 for the California Division of Beaches and Parks and the Division of Highways. The report (Crabtree, Warren, and True 1963) provides a substantial amount of data concerning the excavation but falls into the same pattern as the earlier documents in applying rather than assessing constructs.

It is apparent from these early 1960s articles that the concept of the La Jolla pattern has been well accepted by regional archaeologists. Rather than attempting to "straw man" all of the arguments presented for California prehistory on the basis of poor pattern definition, it will be necessary to tentatively accept the preliminary constructs in order to evaluate other detailed investigations already published.

Perhaps one of the most significant of these discussions is the argument for change in the coastal environment. As noted above, arguments for the siltation of coastal San Diego lagoons were presented as early as 1961 by Warren, True, and Eudey and Shumway, Hubbs, and Moriarty. In 1963, Warren and Pavesic presented a summary of the arguments for these coastal changes. It is the purpose of their 1963 paper to "attempt to elucidate a picture of ecological changes against which the cultural developments in the San Diego coast may be projected" (Warren and Pavesic 1963:411). Data is presented from excavations of SDI-603 on the edge of Baticuitos Lagoon. The primary element for discussion of ecological change is the differential occurrence of Mytilus sp., Chione sp., and Aequipecten sp. This information is combined with radiocarbon dates to support an argument of the replacement of rocky foreshores with sandy beach and mud flats and is used as the foundation for explaining cultural development in the area.

In their discussion, they state "that the lagoons silted in and reduced the food supply of the aboriginal populations along the San Diego coast appears to be an obvious and accepted fact" (Warren and Pavesic 1963:418). This accepted fact is based on logical arguments presented most clearly by Shumway, Hubbs, and Moriarty (1961) and on an argument presented by Warren, True, and Eudey (1961).

The evidence on which this "siltation concept" is based comes from two sources. The most substantial is the archaeological record. By correlating radiocarbon dates on shell recovered from a site at the southern edge of the lagoon, Crabtree, Warren, and True (1963) documented variation in the use of shellfish. Upon reviewing the radiocarbon dates in conjunction with a core sample taken from the lagoon, the archaeologists proposed a marked decrease in the availability of certain varieties of shellfish. The second source of evidence used to explain this shift is a discussion of the hypothetical relationship between a series of ecological variables. Of primary importance is the rate of siltation in association with the rise in sea level.

The siltation concept presents several problems. While the decline in use of shellfish resources is apparent through the radiocarbon dates collected in San Diego County, the assumption that that decline may necessarily result from a decrease in the availability of shellfish could be misleading. The decreased use could have, as easily, resulted from non-resource-associated causes. It requires a leap in logic to assume that cultural evidence from archaeological sites directly reflects prehistoric physical development of lagoons.

The fact that the decrease in shell on archaeological sites may result from something other than ecological changes is recognized by Warren and Pavesic in 1963. They suggest that some of the perceived variation might be a function of "food preference for the prehistoric inhabitants" (1963: 412).

The rejection of this or other cultural explanations is not adequately addressed in any of the early articles on lagoon siltation. It would seem reasonable that firm documentation of this ecological change would require evidence from nonarchaeological sources. Only with information gained through sources not filtered through cultural systems can confidence be placed in the conclusions.

At the present time, such noncultural information is limited to rare core samples taken in coastal lagoons. Prior to the 1963 Warren and Pavesic article, only a single core had been taken from Batiquitos Lagoon. Again, a single sample does not permit assessment of the reliability of the resulting information.

In 1966, Pourade published a summary of work done in the Far Southwest concerning early man. This document is a compilation of work by Malcolm Rogers, H. M. Wormington, Emma Lou Davis, and Clark W. Brott. It presents an in-depth discussion of the San Dieguito pattern originally proposed by Rogers in 1929. The work covers a large area, including portions of California, Arizona, and Nevada. While it presents no specific new data, it does provide the most contemporary discussion of Malcolm Rogers' beliefs concerning the San Dieguito pattern. Of particular concern at this time is the southwestern aspect, which includes portions of San Diego County and the majority of the Baja Peninsula in Mexico.

The San Dieguito complex is presented in three phases: San Dieguito I, II, and III. Rogers indicates that "in the southwestern aspect, the first phase is missing and the second and third are present" (Rogers 1966). As with earlier publications, conclusions drawn in the 1966 report are primarily qualitative in nature. Unfortunately, the report summarizes conclusions more than it does archaeological evidence, and while this is undoubtedly a factor of presentation, it does limit the interpretability of the end results.

The problem seen in many of the archaeological reports completed to date is again reflected in the 1966 publication. Perhaps the best example of the application of data rather than the evaluation of structure is seen in a caption on a photograph of crescentics. In discussing the antiquity of these items, the editor notes:

The association of crescentic stones with antiquity seems to be well established. They have been found in early sites from Oregon to Baja California. A few examples have been unearthed in late Indian deposits, including Catalino and Luiseno, but these finds can be explained by remembering that every man has a natural curiosity for the unusual. The late Indians probably came upon the crescents on old sites, picked them up out of curiosity, and took them home (Pourade 1966:63).

This stretching of the imagination is necessary when explaining aberrant characteristics yet desiring to maintain a given developmental structure.

A particular concern of the 1966 publication is the relationship of San Dieguito pattern and La Jolla pattern materials, as well as the proposed division of the San Dieguito materials into the three phases. Rather than reject any arguments concerning these patterns because of inherent problems with extrapolating from the remains to the cultures which created them, the constructs can be evaluated based solely on their internal consistency, temporarily ignoring the proposed relationships between them.

The distinguishing characteristics between San Dieguito II and San Dieguito III assemblages include the introduction of small projectile points and crescentic stones and the relative degree of sandblasting and patination (Rogers 1966:61). Rogers indicates that some of the characteristics are "indistinguishable."

The use of surface modification as a dating technique has a great number of difficulties. Rogers notes that "when using patina as a basis for determining the relative age of similar artifacts, the artifacts compared must be fashioned from the same lithic material, as different media do not decompose at the same rate" (1966:33). In addition, different chemical constituencies of the soil and meteorological conditions would have an unknown effect on the rate of patination. Temporarily ignoring surface alteration as an index of age, the introduction of two general artifact classes are left as distinguishing San Dieguito II and San Dieguito III assemblage points and crescentics.

The temporal and suggested cultural relations between San Dieguito II and III elements rest "upon horizontal stratigraphy and geology in the southwestern aspect" (Rogers 1966:61). The foundation for this stratigraphic comparison is not clearly presented in either this article or the results of the excavation at the Harris site published by Warren in 1966. Warren's report on the Harris site (1966) summarizes the work of Malcolm Rogers in the mid and late 1930s. This summary provides an invaluable discussion of what has come to be one of the most important archaeological sites in southwestern prehistory.

A discussion of cultural phase divisions by James Moriarty was also published in 1966. In this report, Moriarty attempts to "use carefully selected radiocarbon dates from well-controlled stratigraphic situations in archaeological sites, at levels where typological changes occur (1966: 27). As with many of the archaeological publications of the time, this one presents little data concerning stratigraphic typological change, and although a large number of radiocarbon dates are presented, it is difficult to evaluate the validity of the relationships drawn. Unfortunately, sufficient raw data is not presented to permit an evaluation of the reliability of the results, and it remains to the discretion of the reader to evaluate the significance of radiocarbon dates obtained every 50 centimeters (Moriarty 1966:23).

As the pace of archaeological publications and investigations increases, the terminology becomes more and more entrenched and the labels tend to take primacy over the patterns they describe. Before any ultimate evaluation of the constructs can be made, therefore, it is necessary to have a clear-cut understanding of exactly what is meant by the terms employed. With this in mind, what is actually meant by the terms employed can be discussed. For example, in 1967 Warren presented "The San Dieguito Complex: A Review and Hypothesis" in American Anthropologist. In this article, he indicates:

From the distribution of these artifacts, the San Dieguito complex, at the present time, can be defined as comprising: leaf-shaped knives of several varieties; small leaf-shaped points; stemmed and shouldered points generally termed "Lake Mohave" and "Silver Lake" points; ovoid, large domed, and rectangular end and side scrapers; engraving tools; and crescents. Knife blanks may be included but are probably limited to sites where tools were manufactured.

Some regional variation is apparent, with greater numbers of small points and engraving tools occurring in the desert region (Warren 1967:177).

As long as this is the definition employed for the San Dieguito pattern, much is to be gained in the term's application. A priori acceptance of cultural homogeneity within this pattern would be a mistake, however. Similarly, equation of the pattern with a cultural system and its comparison with other patterns and systems can only be done with the greatest caution. This takes on greater significance as remains dealt with become more recent.

In the reviews above, little has been done in the way of discussion concerning the ceramic-bearing elements of San Diego County. These patterns, labeled Mission Indians (Rogers 1929), Yuman III (Rogers 1945), San Luis II

(Meighan 1954), and Late Prehistoric (Wallace 1955), are proposed to have developed into the ethnographically recorded occupants of the region. Undoubtedly, the most substantial treatment of this Late Prehistoric stage was compiled by True in his doctoral dissertation, Archaeological Differentiation of Shoshonean and Yuman Speaking Groups in Southern California (1966). This document summarizes the work of several archaeologists in the coastal areas of Southern California. It was written to test the possibility of "identifying and isolating cultural traits which can be used to differentiate ethnic and cultural units on the basis of archaeological data (True 1966:204). The report presents substantial detail and provides the explanatory connection between the archaeological data and the purpose of the thesis. These are two elements of particular concern in this report: the division of Luiseno and Diegueno assemblages and the implications of this separation for the cultural development explanation provided for San Diego and vicinity.

In his discussion, True introduces another informational factor into the development of the prehistoric explanations, that of linguistics. Temporarily accepting the existence of a "broadly based Milling Stone Horizon in the area" (1966:287), True proposes two separate explanations for the interface of the Milling Stone base and the more recent patterns. The first of these involves:

. . . the continuation of the basic milling stone base, modified by the introduction of an acorn economy, modified by the introduction of cremation disposal of the dead and by a continuous series of influences from the desert area to the southeast. This development culminated in the historic Yuman speaking Diegueno occupation of the southern portion of San Diego County, California (True 1966:291).

The second development explanation states:

In the area occupied by the Shoshoneans, the basic milling stone pattern was terminated and replaced by the mortar and pestle. Here the change was more marked than in the previously described Diegueno area, and milling stone elements were greatly reduced in importance. The same appears to have been the case with scrapers and hammerstones (True 1966:292-293).

These two developmental explanations are summarized as follows:

The significant thing here so far as the culture history is concerned is the separate development in the two areas: one representing a more or less continuous development out of a milling stone subbase, and the other a replacement of the original pattern with a new way of life carried by a new and different people speaking a different language [emphasis added] (True 1966:293).

The basis for the argument of the continual development in one area of San Diego County and not in another is obviously based on two factors: archaeological data, stressing the importance of the milling stone relative to mortar and pestle, and linguistic evidence.

Without going into too much detail, it is necessary to briefly outline the foundation on which the linguistic assumptions are based in the manner in which they are applied to the archaeological record. The configuration of ethnographically recorded languages is illustrated in Figure 1. Represented are three language stocks: Hokan, Uto-Aztecan, and Penutian. Within these stocks are a series of language families and within each family are several distinct languages. The Hokan language encompass the Chumash and Yuman language families of Southern California. The Uto-Aztecan stock encompasses Luisenic, Hopic, and Tubatulabalic.

Given this distribution of languages and their suggested relationship to each other, linguists and archaeologists have proposed explanations for the prehistoric movement of populations. Perhaps the most influential of these explanations involves the assumption that at one time there was a proto-Hokan language group throughout Southern California which was interrupted by the intrusion of Uto-Aztecan speakers. This is best reflected in True's synopsis of some of the possibilities which influenced the cultural development of San Diego County:

1. The distribution of a substantial portion of the Milling Stone Horizon tends to coincide with the suggested distribution of the Hokan speaking peoples in California and the Great Basin prior to the Shoshonean intrusion.

2. Many basic elements in this Milling Stone Horizon are in fact the same as those elements considered to be diagnostic of a "Desert culture" level of existence.

3. The continuity between the local San Diego area milling stone base and the historic Yuman (Hokan) speaking Diegueno is significant. Since there is no evidence for a break in this sequence, there is some basis for the suggestion that the milling of the Milling Stone Horizon at least in this area, probably was the result of Hokan speaking peoples.

4. A similar situation probably prevailed within the Chumash territory to the north, where a number of traits were shared and where it is reasonable to suggest that the historic Chumash developed out of a similar and probably related milling stone substratum. This is more apparent if the specialized ceramic elements typical of the Diegueno territory are stripped away.



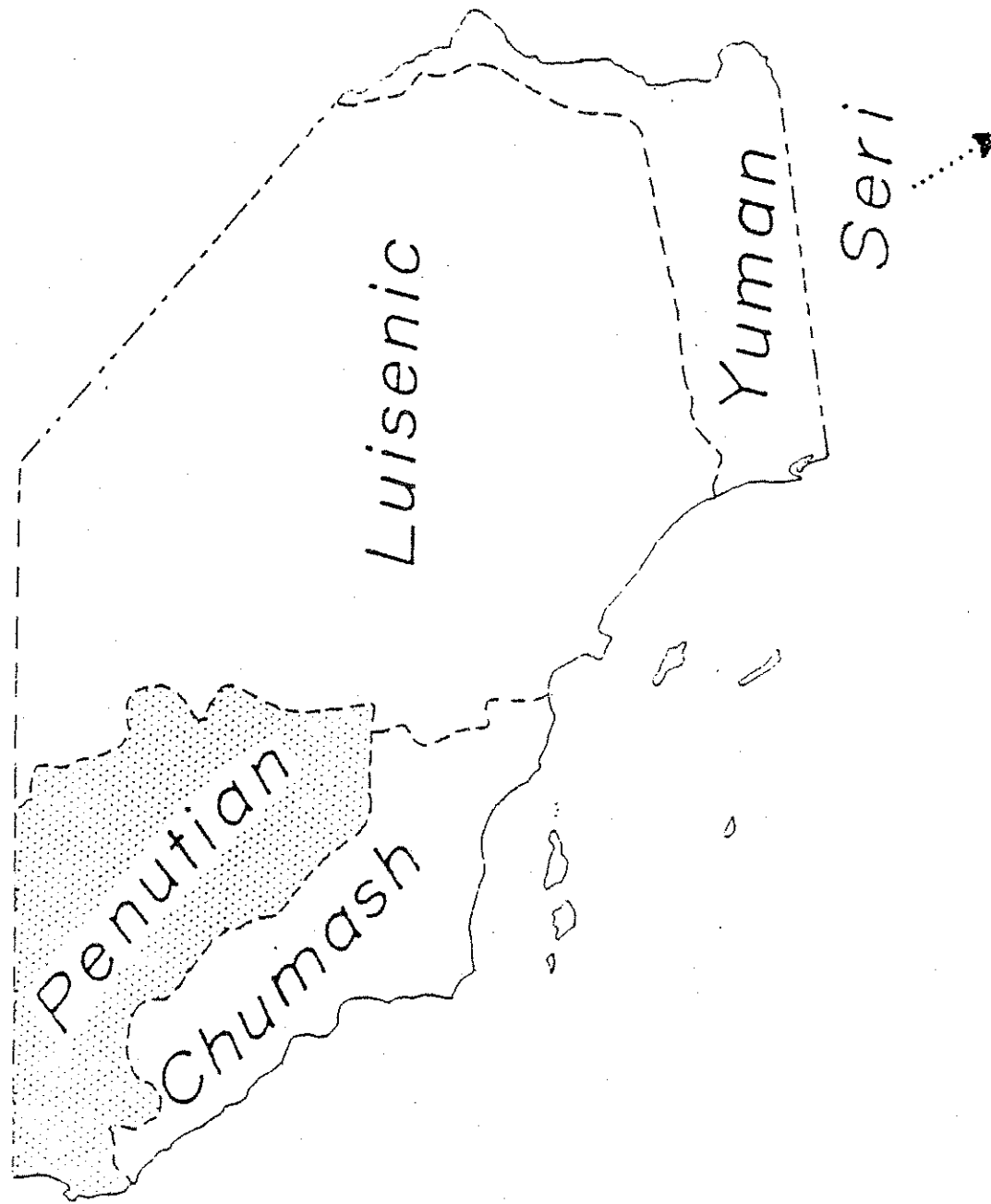


Figure 1. The configuration of ethnographically recorded languages is illustrated above.

5. These apparent similarities are found only in the Diegueno (Hokan) area and are not present in the Shoshonean Luiseno territory (True 1966:294-295).

The concept of the "Shoshonean intrusion" is an integral part of True's presentation on the continual and noncontinual occupations of San Diego County. However, it does require the acceptance of several assumptions, both linguistic and nonlinguistic in nature, which are very tenuous. The concept of the disruption of a Southern California Hokan group by the intrusion of the Uto-Aztecan languages and their separation into the Chumash and Yuman language families would require a "linguistic difference" between Yuman and Chumash, which would be similar to the differences within the Uto-Aztecan language stock. If the dispersion of this latter stock caused the primary separation of Yuman and Chumash languages, then the difference between those Hokan languages and the expansion of the Luisenic language family from its parent stock should have occurred in the same general time frame. Similarly, if the Yuman languages occupied the same area both before and after the Uto-Aztecan intrusion, then the time depth within the Yuman languages should be greater than the time depth between the branching Luisenic intruders.

Surprisingly, neither of these requirements can be substantiated when comparing Southern California languages. The time depth between Yuman and Chumash is apparently of much greater antiquity than the difference between the Luisenic language family and its parent stock. This would, therefore, require a divergence of Yuman and Chumash languages well before the appearance of the Luisenic. Similarly, the time depth within the Yuman language family appears to be not substantially greater than the time depth within the California Uto-Aztecan languages. This would tend to indicate expansion of Yuman languages and expansion of Luisenic languages in California at not too different times.

Because of the problems with the linguistic interpretation expressed in True's dissertation, it is apparent that the linguistic evidence as presented cannot be used as a foundation for an argument of the continual occupation of the southern portion of San Diego County by Milling Stone and Late Prehistoric peoples. Evidence must, therefore, be based solely on archaeological and not on linguistic evidence.

The work in San Diego County archaeology of the preenvironmental impact era is concluded with the work of Warren in 1968 and True in 1970. "Cultural Tradition and Ecological Adaptation on the Southern California Coast" was published by Warren in 1968 and is a reexamination of cultural patterns throughout the Southern California coast and a discussion of the area's ecological adaptation. True's work centered in the Cuyamaca Mountains and resulted in the introduction of the "Cuyamaca complex" as still another Late Prehistoric pattern.

The basic thrust of the article reassociates previously discussed patterns into five basic traditions: the San Dieguito, Encinitas, Campbell, Chumash, and Shoshonean and Yuman. The work associates a variety of patterns with these traditions and describes them concisely. The association, however, appears to be based primarily on nominal variables associated subjectively.

Of particular interest from Warren's discussion is his association of patterns within the "Campbell tradition." "This tradition is equated with the artifact assemblages and sites of the Hunting people . . . and apparently related sites farther south" (Warren 1968:2). Included within the Campbell tradition is not only the Hunting pattern but also the remains described by Peck at Zuma Creek (1955), on Catalina Island (Meighan 1954), and at Topanga Canyon (Treganza and Bierman 1958; Johnson 1966). These associations are based primarily on the occurrence in the latter assemblages of projectile points, knives, and mortars and pestles (Warren 1968:3). He also indicates that materials typically found in the Encinitas tradition are evident at these sites.

Zuma Creek was probably occupied for a fairly long period and the characteristic tools of the Encinitas tradition are found in great number, while the tools of the Campbell tradition appear relatively infrequently (Warren 1968:3).

This reassociation of some of the Southern California patterns indicates the difficulties faced in addressing prehistory as late as 1968. Needless to say, much work is needed to address the substantial information base available in a quantitative manner and, more importantly, a presentation of that evaluation in a way which will permit others to evaluate the conclusions drawn on more than an experience base.

True's report on work conducted in the Cuyamaca Mountains introduced a new term into the myriad of existing concepts, the Cuyamaca complex. He set the Cuyamaca complex off from the San Luis Rey pattern described by Meighan (1954) and the Yuman pattern described by Rogers (1945) on a trait-by-trait comparison basis.

As with San Diego County archaeology, other areas of Southern California saw a substantial increase in the amount of published material during this period. It is obvious that a detailed discussion of all the literature of these areas cannot be presented here. It is necessary, however, to briefly discuss a few of the contributions to Southern California prehistory.

In 1964, Owen, Curtis, and Miller reported on the results of excavations at the Glen Annie Canyon site in Santa Barbara. Curtis indicates that the Glen Annie Canyon site:

. . . has its closest artifactual agreement with the Zuma Creek site at Point Doom in Los Angeles County (Peck 1955), the Little Sycamore Shell Mound in southwestern Ventura County (Wallace 1956), and the Tank site, four miles inland from the Pacific Ocean in Topanga Canyon, Los Angeles County (Treganza and Malumud 1950) (Owen, Curtis, and Miller 1964).

In summary, Glen Annie Canyon was seen as a representative of the Oak Grove pattern and was presented as one of the only summaries of a specific site characteristic of this general pattern (Owen, Curtis, and Miller 1964: 210).

In 1965, an excellent summary appeared discussing archaeology and ethnography of the Chumash. Two articles are of particular interest: Glassow (1965:19-80) and Harrison and Harrison (1965:91-178). Both of these reports describe remains of Chumash sites, one an inland manifestation and the other coastal. Both present excellent data on resources which are fairly well documented as Chumash in origin. Because of shortcomings in previous reports, little comparative analysis is accomplished; however, both of these summaries provide an excellent data base for future comparative work.

In 1966 William and Edith Harrison published a report on work at the Aerophysics site and the Corona del Mar site in the Santa Barbara region. The purpose of this report was to describe the results of the excavation of these sites to correlate the description of materials with other Hunting manifestations, to examine the possibility of phases for the Hunting people, and to discuss the historic role of these people in local prehistory.

The report is similar to Harrison's dissertation (1964) and presents an excellent summary of previous researchers' positions on the nature of the Hunting pattern and its relationship to Oak Grove and Canalino patterns. Based on simple trait-by-trait comparisons, the authors feel that materials derived from the Aerophysics site represent an early occupation of the "Hunting people." These materials are combined with materials from the Corona del Mar site and from the site described by Carter (1941) into what is termed the Extranos phase (Harrison and Harrison 1966:64). Diagnostic characteristics are listed in the report and a 2900 B.C. is applied to the phase.

The second phase of the two-phase system proposed in this report is termed the Del Mar phase. The definition of this phase is derived ". . . from Rogers' synopsis of many Hunting people sites rather than an intensive investigation of a single or several related sites" (Harrison and Harrison 1966:64).

Of specific interest is the contemporaneity proposed between the Extranos phase of the Hunting pattern and the Oak Grove. Using dates obtained from the lower stratum of SBA-78 (Mikiw site), Harrison and Harrison suggest:

This stratum revealed the superimposed burials of the Oak Grove period, with the upper burials (fully extended) dated between 3350 B.C. and 2500 B. C. (Harrison 1964:287-90), thus bracketing the ca. 2900 B.C. average dates from the Aerophysics site. Another Oak Grove site from the coast near Los Angeles, the Zuma Creek site (Peck 1955), has been dated at 3000 B.C. (Bright 1965:370) which places it as contemporary with the Aerophysics site as can be achieved through radiocarbon dating. Because of these absolute dates from later manifestations of the Oak Grove people ranging from Santa Barbara to Los Angeles, and the three absolute dates from Aerophysics, it is apparent that the Extranos phase of the Hunting people is contemporary with a phase of the Oak Grove people (Harrison and Harrison 1966:66).

The authors also discuss different characteristics distinguishing Oak Grove and Hunting peoples. This primarily qualitative discussion draws a picture of two different contemporary systems. The conclusion arrived at by the authors is as follows:

In summary then contemporaneity between these two phases is substantiated by radiocarbon dates. Comparisons between their respective cultural inventories reveals significant cultural differences. Seasonal occupations as a cause of these distinctions are ruled out, because both phases share a common physical environment, and because they possess completely different and completely different and mutually exclusive burial patterns. The conclusion seems inescapable--these phases represent culturally variant societies existing side by side in the same archaeological region (Harrison and Harrison 1966:68).

As with many Southern California archaeological publications, this document provides excellent descriptive data concerning two prehistoric sites. Consistent arguments are presented concerning given constructs

and in-depth foundation for the absolute dating of these is well documented. As with many reports, however, the artifactual and associational data which would interrelate these sites with other sites is weak.

The Harrison and Harrison report has specific importance to the understanding of San Diego area prehistory in that it discusses the contemporaneity of at least a portion of the Oak Grove and Hunting patterns. With Warren's equation of the Hunting pattern with the "Campbell tradition" and the suggestion that this tradition appears, however briefly, in San Diego County at the Harris site (Warren 1968:4), an interesting problem for San Diego area prehistory occurs. As is suggested by Warren, True, and Eudey (1961), a similar relationship may exist for the La Jolla pattern and the Pauma and La Jolla and Hunting and Oak Grove may be parallel. Harrison and Harrison's discussion of the former, then, would provide information necessary for understanding the latter.

One of the elements which has been used to coordinate Southern California prehistory has been burial patterns. In 1967, patterns of inhumations in Los Angeles County were discussed by King. In comparing his results of investigations at Sweetwater Mesa, King creates a framework based on mortuary complexes and carbon 14 dates (King 1967:60-64). The earliest complex described involves "flexed burials or reburials under cairns of milling stones and other large rocks" (King 1967:61). This is followed by extended burials and, finally, flexed burials, usually face down.

With the many archaeological publications appearing in this period, it will be useful to have a brief summary of the major events and the modifications to the prehistory framework achieved. Perhaps the most significant element of the work during the 1960s was the trend toward general acceptance of the prehistoric framework. Rogers' triad, San Dieguito/La Jolla/Yuman, has now become firmly entrenched in literature.

In San Diego County, two phases of San Dieguito materials were defined, San Dieguito II and San Dieguito III; three phases of La Jolla were suggested, I, II, and III (Moriarty 1966); and a myriad of Late Prehistoric patterns were proposed. Some of the various structures suggested during this era are summarized in Table 2.

Several problems present themselves as a result of the review of literature to this date. These problems primarily center around the ability to extrapolate from the archaeological assemblages described to actual cultures or peoples. Proceeding from past to present, the first of these difficulties is in the validity of the San Dieguito complex as a reflection of a distinct ancient cultural system. As described by the reports summarized above, a San Dieguito complex in the San Diego area has come to reflect an assemblage with a variety of patinated, flaked

TABLE 2  
THREE STRUCTURAL EXPLANATIONS FOR PREHISTORY, 1959-1971

Moriarty 1966	Harrison and Harrison 1966	Warren 1968
Diegueno II		Chumash
Diegueno I	Canalino	Shoshonean
La Jolla III		Yuman
La Jolla II	Hunting-Del Mar Phase	Encinitas
La Jolla I		
San Dieguito	Oak Grove-El Capitan Phase	Encinitas
Pre-San Dieguito(?)	Hunting-Extranos Phase	Encinitas
	Oak Grove	San Dieguito
San Diego Area	Santa Barbara Area	Santa Barbara
		San Diego

lithic materials and a lack of milling implements. Because the pattern has been considered one in which the cultural system which created it lacked milling technology, it has been placed prior to the Milling Stone Horizon. The primary evidential basis for this has been the work conducted at the Harris site by Malcolm Rogers and Claude Warren. The lack of quantified comparative information, however, does not permit the detailed review of Hunting pattern materials, Pauma pattern characteristics, or data from such sites as Topanga Canyon, Zuma Creek, and Little Sycamore. As area authors have drawn comparisons between these milling-bearing assemblages and the flaked stone records which have been labeled San Dieguito, such a comparative analysis is needed.

Particular problems which present themselves in relating the various sites, variants and patterns of Southern California include such things as the occurrence of crescentics throughout sites presently placed in different periods, the varied point and blade types evident, the variation perceived in burial patterns, and the differential occurrence of a variety of milling equipment. It would seem obvious, for example, that we cannot be satisfied with the explanation that the occurrence of crescentics throughout different site types ". . . can be explained by remembering that every man has a natural curiosity for the unusual" (Pourade 1966:63). Similarly, it must be recognized that overreliance on a single, albeit detailed, sample such as the Harris site cannot singularly direct prehistoric explanation. This was emphasized by Dr. Paul H. Ezell in reference to the anomalous deposit at the Harris site (1979).

Another point of particular interest is the differential occurrence of stone and shell beads in early coastal sites. While beads have been found throughout the littoral regions of Southern California, they seem to appear more frequently in the north and much less in the south.

As with questions concerning the San Dieguito, various Milling Stone patterns, i.e., the "Milling Stone Horizon" (Wallace 1955), the "Encinitas tradition" (Warren 1968), and the "early Horizon" (Owen 1964), similarly have yet to be adequately integrated. While the recognition of similar assemblage characteristics throughout this stage is apparent, so are the numerous differences. The primary difficulty stems from the nearly exclusive use of nominal trait-by-trait comparisons in relating particular resources. Obviously, the shared traits of milling technology, inhumations, and, primarily, crude flaking procedures have displaced the differences seen in projectile point and blade types, specific burial procedures, particular milling technologies, and differential artifact type and class representation. Detailing these concerns will, ultimately, require more than a presence/absence comparative discussion. Differences in terminology alone make this approach highly questionable as a procedure for refining cultural development explanations.



The most recent period proposed for prehistory, the Late Prehistoric stage, has received very little attention in the literature to date. The Chumash sites excavated in the northern portion of Southern California represent the great majority of these late period investigations. In San Diego County, only the report on Meighan's San Luis Rey sites (1954), McCown's Luiseno excavation (1955), and True's work in the Cuyamaca Mountains (1970) have been reported in any detail. True's work (1966) discussing the archaeological differentiation of Shoshonean and Yuman groups represents perhaps the best summary of Late Prehistoric archaeology yet completed.

#### The Post-Environmental Era: 1972-Present

In 1969, the National Environmental Policy Act was passed requiring in-depth environmental analysis for proposed federal projects. This was followed in 1970 by the preparation of CEQA, which served much the same function for the State of California. The year 1972 was selected as the beginning of this period of discussion because of the "Friends of Mammoth" court case, which ultimately required environmental analysis on private development programs. As a result of this legislation and court decision, there has been a flood of archaeological investigations.

Unfortunately, contract archaeology does not have the benefit of selecting sites to solve particular research questions. While this problem is not unique to this period, it does begin to play a more substantial role in the research results obtained.

It would be impossible to summarize all of the cultural resource management research done in Southern California. The following discussion, therefore, will be limited to certain key representative reports on the San Diego area.

The first major contribution to archaeology resulting from environmental analysis in the San Diego area was prepared by Kaldenberg and Ezell in 1974. This report summarizes the results of archaeological mitigation of the Great Western sites near Olivenhain, California. The report, which ultimately led to the completion of Kaldenberg's master's thesis, discusses an excavation of what was ultimately considered "a triple-component archaeological site, represented by artifacts of the San Dieguito cultural tradition, the La Jolla complex, and the Kumeyaay peoples" (Kaldenberg and Ezell 1974:335).

The distinction between the various components is based on a number of variables. The differentiation between the "La Jolla" and "San Dieguito" materials is made through quantification of a series of nominal variables. The definition of the San Dieguito complex is as follows:

63.15% of all pushplanes located in Site A were recovered in levels V-XIII. Other typological evidence indicates that the phase of San Dieguito represented at Site A is San Dieguito III, i.e., endscrapers account for a significant number of scrapers in the San Dieguito strata--46.15% of all endscrapers encountered at Site A were located in the San Dieguito level. Further support for a San Dieguito III classification is that 48.14% of ovoid scrapers (types 23, 24, 25) were located in the basal cultural level of Site A. It seems probable that these scrapers were manufactured by the San Dieguito peoples, but retouch and differential patination indicate that many of the ovate scrapers were reworked and reused by the later peoples present at Site A.

Additional indications that the base culture is represented by the San Dieguito III phase are provided by the following data: 60.86% of all choppers (type 9) and chopping tools (type 10) were located in this stratum; 70% of all blades (type 7), most of which were percussion flaked, were located in the San Dieguito level; and 66.6% of all knives (type 15) located at Site A appeared in the San Dieguito cultural stratum (Kaldenberg and Ezell 1974:335-336).

This argument uses these relative frequencies as a basis for assigning arbitrary levels to cultural periods. It is interesting to note, however, that when artifactual materials are taken from the site as a whole and grouped into the three proposed categories--Kumeyaay, levels I and II; La Jolla, levels III and IV; and San Dieguito, levels V through XIII--50 percent of all artifacts occur in the San Dieguito strata, with 23 percent occurring in the Kumeyaay and 28 percent in the La Jolla strata. This would mean, of course, that within any given type, it would be expected that 50 percent of the total represented throughout the site would occur in the lower levels. The question then faced is the basis on which the "San Dieguito" assemblage material can be differentiated from the earlier patterns. Two are readily apparent: the presence and absence of groundstone and the presence and absence of ceramics. Ceramics occur in the first two levels and groundstone in the first four. A confident statement, therefore, is that a substantial portion of the Great Western site lacks milling. Whether or not this is due to a premilling component or some other cultural factor is undetermined.

One of the important considerations discussed in the report is the occurrence of shellfish remains throughout the vertical extent of the site. It is therefore suggested that San Dieguito peoples exploited shellfish:

According to Warren et al. (1961:28), this complex differs from the San Dieguito by its dependence upon shellfish and the gathering of vegetable foods. But at Site A we are unable to employ this as a criterion for distinguishing the La Jolla from the San Dieguito occupation, since the San Dieguito people also extensively exploited marine resources. What we can use, though, is the advent of the milling-grinding stone technological phase to suggest a characteristic cultural change at level IV, approximately 40 centimeters subsurface (Kaldenberg and Ezell 1974:346).

Similarly, the report indicates that Olivella beads were recovered from the San Dieguito levels (Kaldenberg and Ezell 1974:344).

The importance of the Great Western site for San Diego County and Southern California prehistory cannot be underestimated. The report specifically points out problems concerning the definition of the San Dieguito pattern and its relationship to the La Jolla and presents valuable information for assessing that relationship. It is only because the foundations for the conclusions are explicated with specific quantitative assumptions that a detailed evaluation of the report and its conclusions can be made.

Another report of importance for San Diego County prehistory is the work done by Dr. Ezell on the Camp Pendleton marine base in 1975. His investigation of an aboriginal cemetery at Las Flores Creek provides an in-depth discussion of burial patterns for a period of local prehistory. The results of his investigation make the placement of the site into the defined prehistoric framework difficult. The discovery of a number of inhumations flexed on the side, back, and stomach, and in one instance with the inclusion of a large segment of whalebone placed atop the burial, are characteristics of patterns found further north and not indicative of the San Diego area. Ezell obtained a date from shell of  $\pm 2060$  (L.J. 3173) for the site, which places it at the temporal intersection of the Milling Stone and Late Prehistoric stages (Ezell 1975:57).

The problem of relating milling stone assemblages with flaked stone assemblages characteristic of the San Dieguito pattern has also been approached by Kaldenberg and Bull (1975). Their investigation of the Rancho del Dios site along the San Dieguito River in San Diego County found flaked lithic materials of highly patinated felsite in association with milling implements. This further complicates the definition of, specifically, San Dieguito materials and its juxtaposition with Milling Stone pattern assemblages. It emphasizes the problem of defining a pre-milling occupation in San Diego County and must be considered in light of Hunting pattern and Pauma pattern assemblages. The problem inherent in

differentiating cultures based on the presence or absence of single technological classes, as potentially reflected in the Great Western report (Kaldenberg and Ezell 1974), reflects the tenuous nature of the present arguments.

The results of excavations at Molpa were published in 1974. This report summarized excavation data collected between 1955 and 1957 and represents one of the few documents completed on excavations not associated with the environmental review process.

The report is ". . . directed toward the San Luis Rey II component, and the more precise definition of this phase was the prime objective of the investigation" (True, Meighan, and Crew 1974:93). The authors present the Molpa results as the type site for the San Luis Rey II complex. They indicate that three separate components are represented in the deposits at Molpa, the San Luis Rey I and II complexes and the Pauma complex. They note that "For the purpose of this report we omit description of the material recovered from the test trench area (Pauma Complex). In addition, the limited number of units penetrating the SLR-I component and the relatively small sample recovered from these units makes it difficult to say more about the San Luis Rey I than has been already published. The emphasis here is therefore directed toward the San Luis Rey II component" (True, Meighan, and Crew 1974:26). San Luis Rey II is equated with the ethnographically recorded Luiseno (1974:77).

The Molpa report indicates that "By definition, the beginning of the San Luis Rey II is marked by the introduction of ceramics into the area" (1974:94). What the Molpa report represents, then, is a detailed site report describing an assemblage with ceramics with an ethnohistoric reference for a Luiseno occupation. A preceramic variant is evaluated by comparing excavation results with information collected by Meighan from SDi-132 (1954).

Although stratigraphic sequencing of cultural phases is not demonstrated firmly at Molpa, a comparative analysis of the Molpa collection and the preceramic collection from SDi-132 does provide information on change through time (True, Meighan, and Crew 1974:97).

The authors use the argument of a preceramic variant of the San Luis Rey complex as the basic assumption for presenting a discussion of culture change in this area of San Diego County. The reader is referred to the discussion of the San Luis Rey I report earlier in this presentation. To the extent that the consideration of SDi-132 as a preceramic phase of the complex is accurate, the Molpa discussion of culture change is of particular value. Additional work is required, however, to confirm the temporal and cultural relationship of these two phases.

The post-CEQA era suffers from the application of the prehistoric framework in much the same way as the work of the 1960s. Again, the tendency to apply rather than evaluate has stifled the progress of the development of prehistoric cultural histories.

In 1976, Carrico summarized work done at the Los Compadres site near Encinitas, California. Tentatively accepting the La Jolla construct, the Los Compadres report provides important evaluative information concerning the proposed prehistoric development. Of particular interest is the only partial acceptance of the idea of replacement of Mytilus by other species of shell. This replacement was postulated by Warren as evidence of the siltation of coastal lagoons and the decreased availability of rock-dwelling mollusks. Carrico points out several difficulties in discussing the relative occurrence of various shellfish species, concerns which are critical to the siltation argument (Carrico 1976:136-139).

The arguments for the interrelationship of the Milling Stone pattern and the Late Prehistoric pattern of the coast of San Diego County have also been addressed extensively in The Archaeology of Villa la Cumbre: Results of Testing Three Archaeological Sites at Batiquitos Lagoon (Bull, Norwood, and Hatley 1977). This report discusses the results of an excavation of a site at the eastern end of Batiquitos Lagoon. While this report also generally accepts the prehistoric framework discussed above, it calls into question the siltation argument as a causative factor in prehistoric development. This document uses True's criteria for differentiating Luiseno and Kumeyaay assemblages (True 1966). In doing so, it proposes that the site is a remnant of the Luiseno occupation and suggests the lack of continuity between Milling Stone stage use and Late Prehistoric occupation.

In assessing the occupation of the lagoon area, Bull, Norwood, and Hatley suggest the following:

This lack of continuity provides for an alternative explanation for the paucity of radiocarbon dates between 3,000 and 1,000 years B.P. that has been obtained for lagoon-related sites. A break in the occupation of the area could either represent the removal of La Jolla peoples and their replacement by Luiseno populations, thus permitting the La Jolla-Kumeyaay continuum [in another area of the county], or it could result from the removal of the maritime orientation, the La Jolla, and the replacement of that group by desert-bearing cultures, specifically, the Kumeyaay.

Linguistic evidence supports the latter, but no evidence recovered from the Villa la Cumbre tests was located which could evaluate the contention of Kumeyaay populations (Bull, Norwood, and Hatley 1977).

The direct use of linguistics in prehistory appears again in the report on the archaeological resources of the coast of Camp Pendleton (Bull 1975). In this document, and again in 1977, Bull relates the archaeological framework with the proposed explanation for the development of Far Southwest languages. The linguistic explanation employed through the first two eras of archaeological development involved the continuous occupation of Southern California Hokan-speaking peoples with a subsequent intrusion of Uto-Aztecan languages. Bull summarizes the work of linguists in proposing an expansion of not only Uto-Aztecan languages but also the Yuman languages of the Hokan stock. This explanation does not require the continual occupation of the coast in either the Luiseno or Kumeyaay areas. The explanation takes into consideration the distance between the Yuman and Chumash languages of the Hokan stock, as well as the suggestion by Langdon (1975) that there is a greater degree of similarity between the Seri and Chumash than between the Yuman and Chumash or the Yuman and Seri. The Seri-Chumash relationship has been tentatively called Southern California Hokan by Langdon (1975). Bull (1975) proposes a correspondence between the Southern California Hokan and coastal Milling Stone stage patterns. This spatial cultural continuum is replaced in San Diego County by incoming Diegueno and Luiseno, ultimately resulting in the ethnographically recorded language situation. This explanation differs from that presented by True (1966) and others and casts a different light on the prehistory of San Diego County in general.

One other cultural resource management report deserves special mention at this time. Work at the San Dieguito Estates site has resulted in a series of radiocarbon dates exceeding 8000 B.P. (Norwood and Walker 1979). These dates closely parallel the radiocarbon dates obtained by Kaldenberg and Ezell (1974) for the Great Western site. As was discussed briefly above, Kaldenberg and Ezell assigned the Great Western assemblage and the associated dates to the San Dieguito pattern. Norwood and Walker (1979) assign dates from the same general time frame to materials considered characteristic of the La Jolla pattern. These two sets of data and associated assemblages perhaps provide the best indication of the general weaknesses of understanding prehistory. It is apparent that a great deal of work needs to be done in the logical consistency of prehistoric explanation.

This discussion of work done after the passage of CEQA has been kept to a minimum. This is because the great majority of the work done under the CEQA guidelines has provided little new interpretation for local prehistory.

It must be remembered that the problems encountered in Southern California prehistory in no way limit the other values obtained through archaeological investigation. Work in demographics, least cost analysis, settlement patterns, and other topics have been well addressed in the literature. In addition, specifically since the advent of CEQA, a great deal of new and enlightening data has been made available.

In summary, the post-1972 cultural resource investigations have tended to raise more questions than they have answered. While local prehistory has not been rewritten, a substantial amount of effort has been placed in its review. What is of primary importance as a result of this work is the tenuous nature of the San Dieguito complex as defined on the coast of San Diego County and its real or perceived relation to the La Jolla pattern. Similarly, the nature of the La Jolla pattern and its cultural relationship to Late Prehistoric resources is also important.

#### A. REEXAMINATION OF THE ROGERS TRIAD

That there are at least three somewhat different archaeological patterns in San Diego County cannot be disputed. Based on comparative trait lists, these three patterns can be characterized as follows:

- Pattern 1: ceramics, finely worked projectile points, cremations, and the appearance of rock art;
- Pattern 2: crudely flaked lithic material, milling equipment, inhumations, shellfish remains, and coastal locations;
- Pattern 3: pressure flaked local fine-grained volcanics and metavolcanics, an abundance of leaf-shaped knives, chipped stone crescentics, and a wide range of scraper types.

The cultural and temporal relations of these assemblages are not clearly understood. It is generally accepted that each of these represents a temporally distinct, culturally unique system. It can be seen from the discussions above that these assumptions are tentative at best.

Several difficulties appear when attempting to oppose the various constructs. These are listed summarily below:

1. If there is a premilling Early Man occupation in Southern California, why is it, on the coast, limited to San Diego County and not manifest in Ventura, Los Angeles, or Santa Barbara counties?
2. With no stratigraphic evidence for the association of San Dieguito III with early San Dieguito phases, what connection is there between the proposed Early Man occupation of San Diego County and the same occupation of the Far Southwestern deserts?
3. If there is a substantial occupation of coastal San Diego County for approximately 6,000 years prior to the appearance of ceramic-bearing "Late Prehistoric" cultures, what specific activities were occurring further inland? This has been stated as the problem of "inland La Jolla" occasionally referenced in the literature.

4. If the evidence described in the Santa Barbara region for the Hunting pattern is accurate, what is the explanation for the lack of this system within the southern counties of coastal Southern California?
5. To what extent are the differences perceived throughout the area a function of specific adaptation rather than particular cultural or temporal differences? A key element to this concern is the degree to which each of the patterns reflects generically distinct systems. Of course, solution to the problem of the cultural and temporal relationships between these patterns will go a long way toward resolving all of the problems discussed here, particularly if they can be integrated over a sufficiently large area.
6. If there is an Early Man occupation evident in inland San Diego County is there of an intermediate occupation for the same area.

It is obvious that without the development of the time machine, the correct explanation for local prehistory will never be known for certain. All that can be hoped for is that an explanation can be derived which will be consistent spatially, temporally, and culturally. Several potential explanations present themselves. It remains for detailed investigations to evaluate these and other potential proposals.

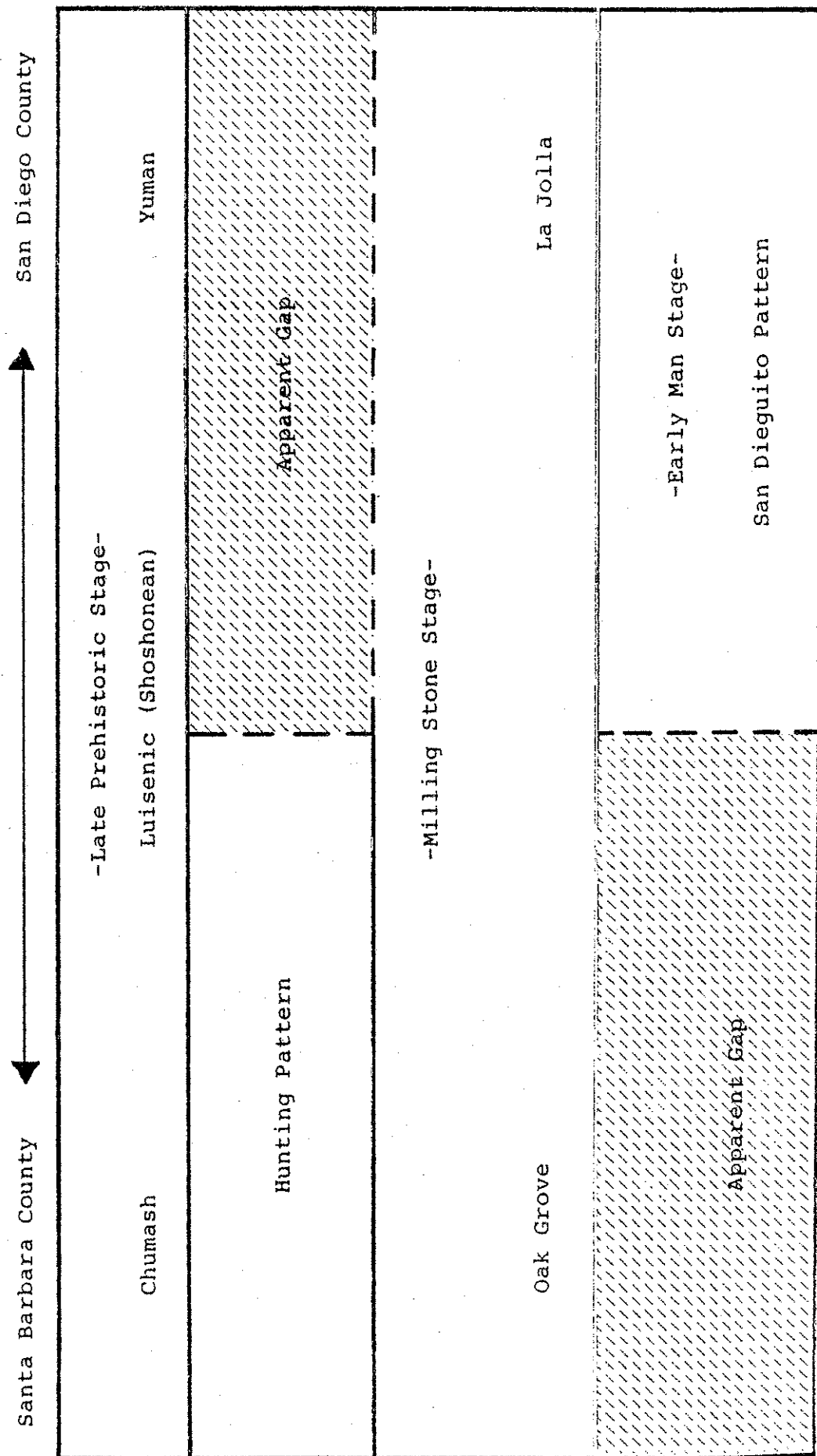
There are two possible courses for the solutions to these problems: either within the presently accepted framework or with significant outline modifications. Evaluation of these problems within the existing structure requires the formulation of consistent hypotheses for testing with new data. While it is possible that the existing explanation is correct, i.e., there may in fact be an Early Man occupation in San Diego County and not further north, there are other explanations which permit the resolution of these problems without the logical inconsistencies. One such explanation is presented below.

A proposed structure with two apparent gaps is presented in Figure 2. A solution which is immediately apparent is the equation of the Hunting pattern and the San Dieguito pattern. This has been suggested through the association of the San Dieguito with Topanga (Treganza and Bierman 1958) and the inclusion of Topanga within the Campbell tradition (Warren 1968), thereby associating it with the Hunting pattern.

Several problems exist with this correlation. The Hunting pattern is a milling tradition and the San Dieguito III pattern, as proposed, is pre-milling. The acceptance of San Dieguito III as premilling may be premature.



FIGURE 2  
COASTAL SOUTHERN CALIFORNIA



Work done by Kaldenberg and Bull (1975) reported milling and San Dieguito - type materials on the San Dieguito River. Warren (1968) indicates a milling component at the Harris site, the "San Dieguito type site," and uses it as evidence for a brief intrusion of the Campbell tradition in San Diego County.

The determination of San Dieguito III as a premilling pattern stems from two apparent sources, the Harris site and work by Malcolm Rogers in the California and Arizona deserts. The Harris site has been termed the "San Dieguito type site" (Warren 1968) and is presented as stratigraphic evidence for the separation of La Jolla and San Dieguito, as well as the premilling nature of the San Dieguito. Rogers' work in the Far Southwest deserts ultimately resulted in the adoption of the San Dieguito terminology and the correlation of crude technologies, such as "Malpais" and "San Dieguito I and II," with San Dieguito III (Rogers 1938). These two factors are the apparent foundation for the premilling label for the San Dieguito.

It is interesting to note that there is no published reference available for the stratigraphic association of the three San Dieguito phases. Similarly, the problems of basing conclusions on a single sample, limit the reliability of the Harris site data. One of the past researchers of the Harris site recently suggested that the site may be an anomaly (Ezell 1979).

It would appear quite possible that the assumption that San Dieguito III is premilling, even nonmilling, is invalid. With this assumption, it may be possible to associate the San Dieguito III pattern and the Hunting pattern.

For the purposes of this discussion, this combined pattern will be referred to as the Hunting pattern because of the hunting/milling technologies previously associated with this pattern. San Dieguito III is laid aside for now because of its premilling connotations.

If such a correlation is possible, the question arises as to the relationship of this combined pattern, the Hunting pattern, with "Milling Stone stage" materials.

Simply stated, the Hunting pattern can occur before, during, or after Milling Stone patterns. Using information gleaned from the Santa Barbara region, the Hunting pattern occurred with the later phase of the Oak Grove pattern (Harrison and Harrison 1966). In San Diego County, the San Dieguito III variant of the Hunting pattern may well occur with the La Jolla, relating absolute dates from the Great Western site (Kaldenberg and Ezell 1974) and San Dieguito Estates (Norwood and Walker 1979).

Clarification of this dilemma rests primarily with the cultural identity of the various Milling Stone patterns. Of particular interest for this discussion is the reliability of accepting the La Jolla pattern as a distinct cultural group. The representatives of this pattern make up Pattern 2 discussed above. While previous researchers have definitely shown a distinct material association, the cultural significance of the resulting pattern has been generally accepted. The possibility of the La Jolla pattern being a reflection of a limited function in a restrictive setting has not been adequately addressed. While the cultural nature of the La Jolla may be significant, it seems equally reasonable to define it in terms of the area in which it occurs, coastal San Diego County, and one of the primary functions, shellfish collection and processing. As such, the assemblages of the La Jolla pattern could well transect a series of culturally distinct occupations, representing a unique adaptive system.

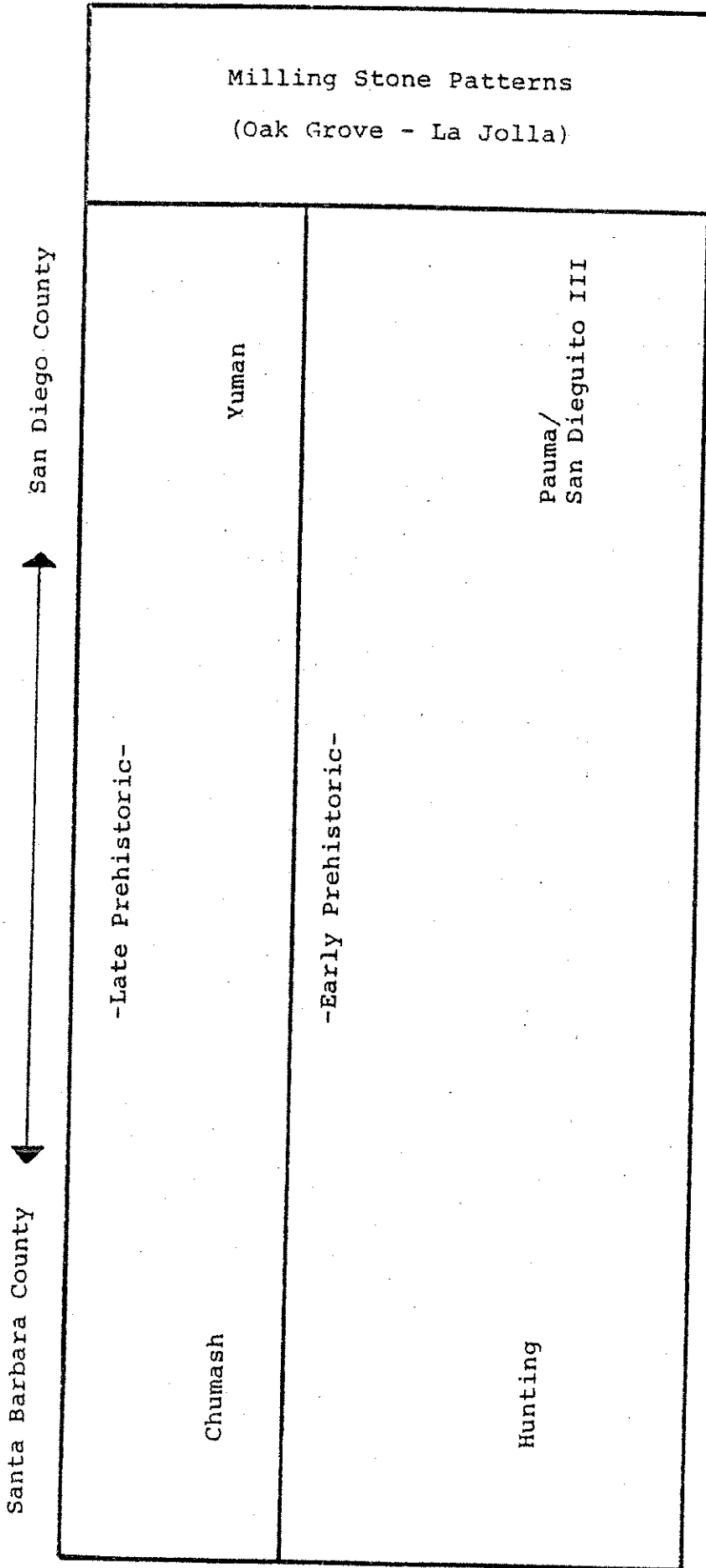
The fact that Milling Stone assemblages and Hunting pattern sites occur contemporaneously cannot be disputed. While the possibility that the La Jolla and other regional patterns and variants are the results of the same cultural group has not been addressed, this relationship has been approached for the Oak Grove and Hunting patterns for the Santa Barbara region (Harrison and Harrison 1966:68). The conclusion drawn in the latter relationship is that the Extranos phase of the Hunting pattern and the latter phase of the Oak Grove ". . . represent culturally variant societies existing side by side in the same archaeological region" (Harrison and Harrison 1966:68).

The basis for this conclusion is threefold: they share a similar physical environment, they possess completely different and mutually exclusive burial patterns (Harrison and Harrison 1966:68), and they possess differences "vast enough to validate significant cultural differences" (Harrison 1964:347). The reliability of the conclusion based on this evidence falls to the reader for evaluation. Such a discussion for the San Diego area might well take on a different complexion.

While it is not within the scope of this paper to provide detailed information for the evaluation of the cultural significance of the La Jolla pattern, a few remarks will be useful. As discussed above, La Jolla pattern sites are limited to the coastal zone of San Diego County and vicinity. They have in common not only coastal location, but also a focus on coastal resources, specifically shellfish, and a limited range of artifacts and non-shell ecofacts. In addition, sites with typical materials range from as recently as 1500 B.P. as long ago as 8500 B.P. (see discussion above).

The relationship between the La Jolla and Pauma has been discussed as a response to a changing environment (Warren, True, and Eudey 1961) and, therefore, as a cultural association. Similarly, Rogers (1945) indicates that La Jolla II middens and Yuman middens are often so similar as to make distinction impossible.

FIGURE 3  
MODIFICATION OF FIGURE 2



Given the above factors, it would seem reasonable to suggest that the La Jolla pattern, Pattern 2, is an environmentally determined one and not necessarily representative of a specific cultural group. With this in mind, it is possible to propose a modification of the structure presented in Figure 2. This modification is represented by Figure 3. In this outline, there are only two primary occupation periods: Late Prehistoric and Early Prehistoric. The former includes such patterns as Chumash, Yuman, and Luisenic, and the latter includes the Hunting, Pauma, and San Dieguito III patterns. The assemblages presently associated with the La Jolla pattern would represent environmental adaptation crossing both the Late and Early Prehistoric periods.

With this structure, it is possible to reevaluate the six points addressed above:

1. The problem of an Early Man occupation in San Diego County and not in other areas of Southern California is eliminated with the equation of the San Dieguito III, Pauma, and Hunting patterns.
2. The tie between San Dieguito assemblages in the desert and San Dieguito III materials on the coast is not as significant for local arguments because of the San Dieguito/Hunting relationship and the incorporation of milling into the San Dieguito III assemblage.
3. There is no apparent temporal gap between Late Prehistoric inland assemblages and Early Man materials because of the equation of Early Man (San Dieguito III) with other Early Prehistoric patterns. With the La Jolla being ecologically rather than culturally specific, an inland manifestation would not be expected. Culturally related materials could result from either Late or Early Prehistoric patterns.
4. As with the explanation for (1), the problem of the lack of a Hunting pattern in San Diego County is moot.
5. The primary evaluative element for both the old and present explanation centers around an evaluation of the nature of adaptive rather than cultural or temporal differences. It is quite possible that the observed patterns could be explained with any single factor or a combination of these factors. Resolution of the cultural significance of the La Jolla pattern becomes critical for the resolution of the problems with the prehistoric framework.
6. The solution to this problem is the same as for (3) above. With no necessary gap between an Early Man occupation and a Late Prehistoric occupation, the problem does not exist.

## CONCLUDING STATEMENT

The purpose of this discussion has been to provide a critical evaluation of prehistoric explanation employed in Southern California generally and coastal San Diego County in particular. In completing this brief discussion, a modified explanation has been proposed. Both the modified explanation and the original have problems and require the formulation of hypotheses and execution of tests. Similarly, there is a need for new, innovative explanations which will provide a more reliable foundation for interpreting archaeological evidence.

The basic thrust of this presentation has been an attempt to open minds to potential deficiencies in the existing informational and interpretational base for Southern California prehistory. From this, it is hoped that a more logically firm basis may be adopted for area prehistory, ultimately permitting the extension of archaeological research from broad developmental specifics to generalized anthropological research.

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GEOLOGIC SUPPORT FOR THE AGE DEDUCED BY ASPARTIC  
ACID RACEMIZATION OF A HUMAN SKULL FRAGMENT FROM  
LA JOLLA SHORES, SAN DIEGO, CALIFORNIA

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Abstract: *Recently reported U/Th series measurements on the skeleton of Del Mar Man have suggested a date of only 11,000 BP, rather than the 48,000 BP deduced from aspartic acid racemization reported by Bada, Schroeder and Carter in 1974. The Del Mar skeleton was without useful geological provenience by which the racemization age might have been supported, but another specimen, a human frontal bone from La Jolla Shores, was dated at the same time at 44,000 BP, and the fact that this sample did have useful provenience has been generally overlooked. This paper offers evidence which tends to support the racemization age of the La Jolla Shores skull.*

Introduction

The La Jolla Shores site is known only from descriptions by Malcolm J. Rogers, who was later to become active in Southern California archaeology and the San Diego Museum of Man. Rogers, then a citrus grower in Escondido, California, with a keen interest in archaeology, examined the site in 1926 as it was being demolished to make way for a real estate development. He left careful notes and maps, both in plan and elevation, and collected a number of human skeletal remains from various levels of the site, but of course was unable to form any conclusion regarding the age of the fossils. He felt, however, that the site was a very important one.

Before its removal, the feature containing the site, a sand ridge (Figure 1, 2) or bay bar formed when the lower land surrounding it was covered by the sea, rose about 8 meters above the shallow, brackish pond or lagoon which lay between it and the beach. When the grading crew noticed what appeared to be human bone in their steam shovel, the engineer in charge notified the staff of the San Diego Museum (later to become the Museum of Man). Edgar Lee Hewett, then the Director, told them not to bother with the bones as they were undoubtedly recent and of no importance, but Rogers, learning of the discovery by some means not disclosed, hurried to the site and examined it with care. The following is from his unpublished account:

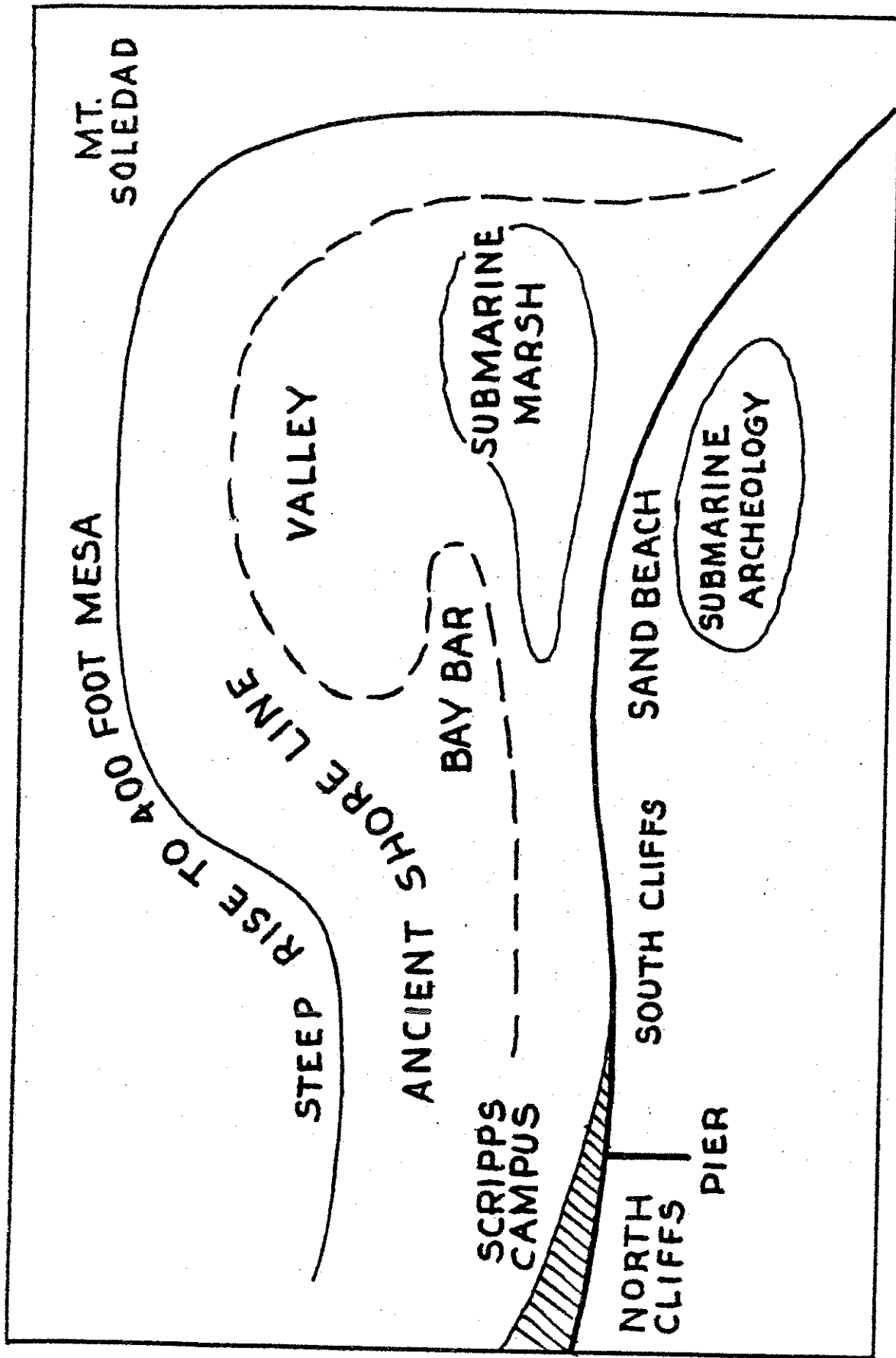


Figure 1. The 1926 La Jolla Shores bay bar and associated features. (After Carter, 1980).

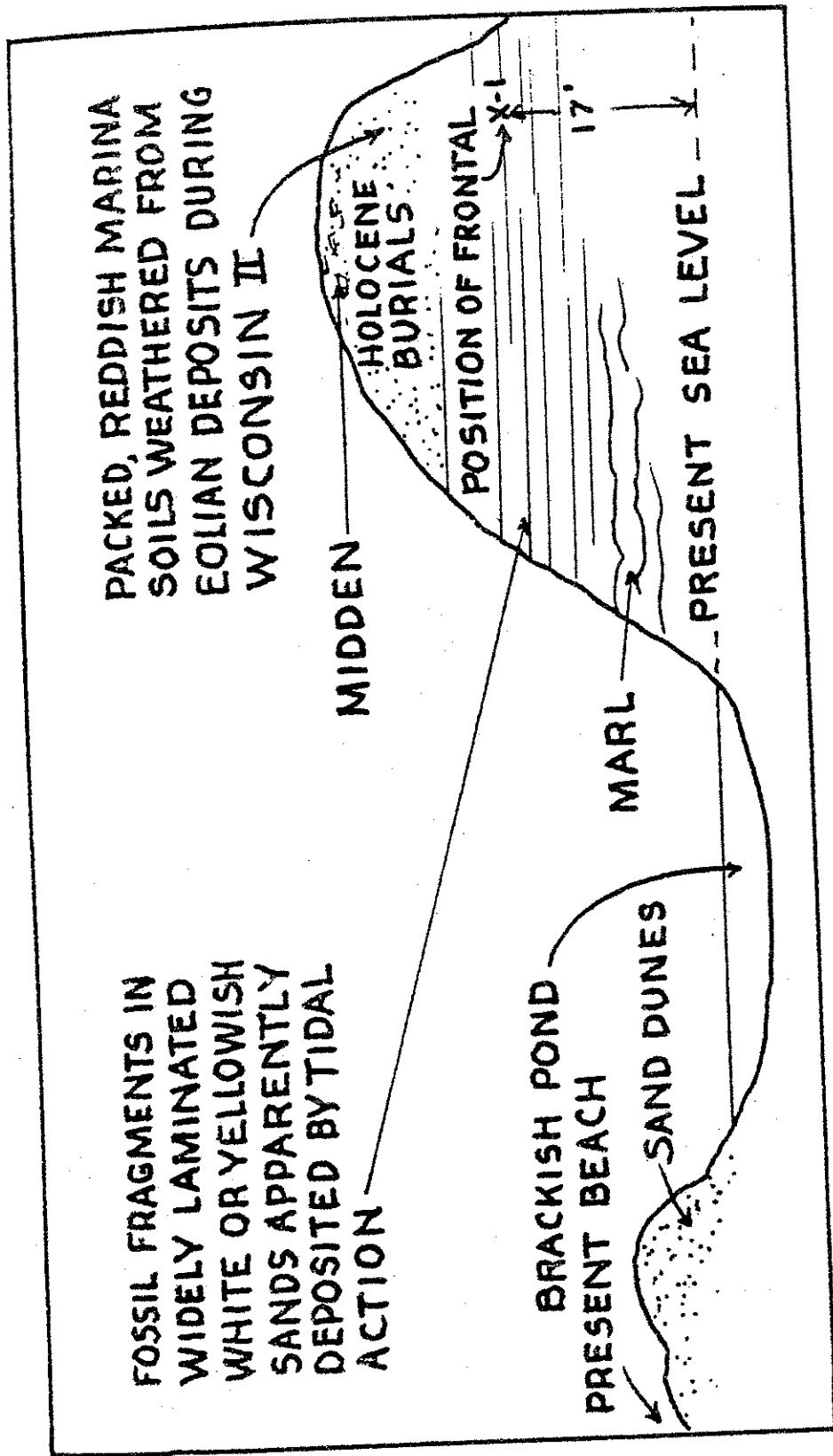


Figure 2. Cross-sectional diagram of the 1926 La Jolla Shores bay bar or sand ridge which contained human skeletal remains, including the fossil frontal bone dated at 44,000 BP by aspartic acid racemization techniques by Jeffrey Bada in 1973.

In April, 1926, I was informed that human remains were being encountered in grading a tract of land at La Jolla, California. The site is known as La Jolla Shores, and is situated on an embayment 2 miles north of the city of La Jolla. On visiting the site I observed the following conditions; a long ridge which paralleled the seashore was being torn down to fill a depression between it and the shore. A shell-midden of an average depth of one foot had already been scraped off into the depression, and a steam shovel had completed a cut seventy feet wide and seven feet deep through the ridge. No skeletal material had been encountered in the midden, but in the sub-soil one interment (sic) and a few isolated human fragments, all slightly mineralized, were found.

Recognizing the importance of the site, I notified the owners of its scientific value, and obtained their interest and cooperation even to the extent of stopping the steam shovel when necessary to map, photograph and properly remove remains which might be subsequently encountered. As I was unable to carry on the work independently, I attempted to interest a scientific institution in the project, but failed.

During the ensuing three months, the site was completely demolished, and with it probably the most important prehistoric station in southern California. Throughout the period of the leveling, I was able to visit the site but four times; recover a certain amount of the skeletal material; make observations; and plot some of the finds on the company's maps (the latter are in the Museum's archives).

The human remains from the upper levels were internments, and undisturbed, but the isolated mineralized fragments from the lower levels had been broken and transported previous to their mineralization. Although the latter may have been derived from burials, they certainly had been removed from their original position through natural causes.... The heavily mineralized human frontal, mapped as X-1, was taken from the bottom of a steam-shovel cut by Mr. M. C. Burke, the company engineer. I was notified and shown the original position of the bone the next day by Mr. Burke. As it is mapped, it is probably within a few inches of the position in which it was found.

The eroded margins of the bone and its isolated position indicate transportation, probably previous to its mineralization. It is grayish in color and covered with a calcareous film. The diploid cells are partially filled with translucent crystals which have been identified. As the bone has not been examined by an anatomist, nothing can be said as to its racial significance. I believe, however, that it represents no radical departure from known American crania.



The human frontal bone was placed in Rogers' collection along with skeletal material from this and other sites, and transferred to the museum shortly afterward. The frontal bone and notes and maps concerning it were almost, but not quite, forgotten in the decades that followed.

Malcolm Rogers had been trained in the natural sciences at Syracuse University, had worked as a mining geologist, served in the Marine Corps during World War I, and taken up citrus ranching following his discharge. As early as 1919 he was investigating archaeological sites in the vicinity of his ranch in Escondido. In 1928 he became Curator at the Museum of Man, and he was appointed Director in 1943. For over 40 years, he was extremely active in the field, both along the coasts of Southern and Baja California and in the deserts to the east, laying the groundwork for what we know today about the San Dieguito and La Jollan people. In 1960 he was struck and killed by an automobile. His life and work have been well summarized in the Copley book Ancient Hunters of the Far West (1966).

Shortly after Rogers' association with the Museum of Man, George F. Carter, then a high school student, began dropping in at the museum after school and became a sort of unpaid apprentice, washing pottery and doing other odd jobs. Eventually, after receiving an AB degree in Anthropology at the University of California, he became a paid member of the staff, and the title, Curator of Anthropology, was invented for him. However, Carter's views on the antiquity of man in America contrasted so strongly with the highly conservative approach of Rogers that Carter left the museum in the late 1930's for graduate work in Geography at the University of California at Berkeley and his doctorate in that field, which he felt would be useful in his studies of early man.

After the end of World War II and service as an analyst in the Latin American Division of the Office of Strategic Services, Carter, now a professor of Geography at Johns Hopkins University, began returning to San Diego in the summers to do field work on the Pleistocene marine and stream terraces in the coastal region. Finding what he considered to be evidence of human occupation exposed in some of the terraces, features which he attributed to periods of eustatic high sea levels during the Sangamon Interglacial, he began to concentrate his attention on the evidence for early man in the San Diego area, and in 1957 published Pleistocene Man at San Diego. This book quickly became, and still remains, highly controversial, although many of his then radical views have since become widely accepted.

In 1970 the Buchanan Canyon site was discovered (Minshall 1976), and the large collection of crude quartzite tools and cores recovered strongly supported Carter's earlier claims of human artifacts in the interglacial terrace at Texas Street, only about a kilometer from Buchanan Canyon. During a reinvestigation of Texas Street and excavation in Buchanan

Canyon in 1973, Carter, now a Distinguished Professor at Texas A & M University, heard of a new method for dating bone being developed by Jeffrey Bada at the Scripps Institution of Oceanography in San Diego. This was based on the racemization of amino acids after the death of an organism, and could be measured using very small amounts of bone or shell, producing dates far beyond the limitations of radiocarbon systems. Carter, the only person who knew from personal experience the circumstances surrounding the recovery of the Del Mar and La Jolla Shores skulls, and appreciating their possibly great antiquity, persuaded Bada to attempt to date them by aspartic acid racemization measurement. The results were 48,000 BP for the Del Mar bones and 44,000 BP for the frontal from La Jolla Shores (Bada et al, 1974).

Since most scholars in America are convinced of man's relative recency in the Western Hemisphere, Bada's dates have been widely disbelieved. Although his methods and results were carefully checked by obtaining, on several specimens, ages by both racemization and  $^{14}\text{C}$  measurements that were in substantial agreement, serious inconsistencies have since been reported that tend to discredit the racemization process. This is particularly true of an aspartic acid racemization date of 70,000 BP on a human skull from Sunnyvale, California (Gerow, 1981), in which the geological data and other aspects insistently suggest recency. A date for the Del Mar skull of only 11,000 BP, obtained by U/Th series, has been announced by Bischoff and Rosenbauer of the U. S. Geological Survey (1981).

#### Interpretation of the Geological Evidence

Geological support for the racemization age of the Del Mar skeleton is largely lacking, even though Bischoff and Rosenbauer maintain that their U/Th results are "not only internally concordant, but they are in good agreement with the geological evidence." Actually, there is no useful geological evidence. The entire block of soils in which the Del Mar skeleton was found has been eroded and bulldozed away since the bones were removed in 1929, and only photographs and Rogers' notes remain. He described the soils as "estuary sands" overlying Tertiary sandstones. If so, they must have been deposited well back in the Pleistocene to have reached their modern elevation some 16 meters above sea level, but there is of course no way to know how or when the fossils were deposited in them.

The geological provenience of the frontal bone from La Jolla Shores is very different and far more useful for determining the time and manner of its deposit, although interest has centered almost exclusively on the Del Mar skull because of its greater completeness and supposedly greater antiquity. In contrast, the La Jolla Shores frontal has been generally overlooked and ignored except by Carter, who has discussed it at some length (1980:162-165) and Minshall, who also offered a description of the fossil, its provenience and circumstances of recovery (1976:125, 126).

Quoting again from Rogers' notes:

The archaeology of the lower stratum (in which the frontal was found) is purely a geological problem, linked up with the history of changing sea-level and will yield only to the latter's solution. My interpretation to date is that the thin Lit. I (later designated La Jolla I) stratum in the sea-cliff at W-199 formerly extended south around the La Jolla Shores bay and that the scattered mineralized human bones were from burials washed out of it by tidal action during its destruction. The white sand stratum (in which the frontal was found) is widely laminated.

The critical aspects of the geological problem reported by Rogers, a trained and accurate observer, are considered to be the following:

- (A) The position of the frontal had an elevation of 17 feet (5.1 meters) above present sea level, as recorded and mapped by Rogers.
- (B) The frontal must have been at or near sea level when deposited by fairly gentle tidal action, judging by the laminated appearance of the sand matrix and the preservation of rather fragile bone.
- (C) The sea level relative to the land must have been stable or falling following deposit to permit the preservation of the land form.
- (D) The slightly abraded and rounded edges of the frontal suggest transportation, as pointed out in Rogers' notes, and no agency other than tidal action can be visualized that would be capable of breaking up an earlier burial and redepositing the fossils substantially undamaged.

Carter interprets the data as supporting Bada's racemization age of 44,000 years for the human frontal bone. He does so on the basis of a generally stable Southern California coast, but a sea level that has been fluctuating throughout the Pleistocene in response to glacial and interglacial climate phases, and generally falling as the earth's water is steadily being diminished. Using figures suggested by Evans (1971), he believes that sea levels worldwide were about 4 meters higher than the present during the interstade between Wisconsin I and II about 40,000 BP. This would have permitted unusually high tides or storm waves to destroy an already ancient burial and redeposit the remains in a slowly-forming bay bar of the La Jolla Embayment, a surface which would then have been less than two meters above sea level. As sea levels fell during the ensuing glacio-pluvial, the bay bar gradually accumulated additional sands which weathered into the reddish Marina soils containing the Holocene midden materials and skeletal remains that overlay the white laminated sands.

There are three areas of uncertainty and disagreement concerning the above model: the presumption of stability for the coast, the eustatic sea stand 40,000 years ago, and the origin of the landform containing the fossil frontal--whether the laminations are water-laid or subaerial dune formation.

On the question of tectonic stability, there is almost universal agreement, among scholars that have studied the San Diego coast, that regional diastrophism associated with the juncture of the Pacific and American Continental plates has been occurring over the last 120,000 years. Ku and Kern (1974) have deduced an average rate of uplift of between 11 cm and 14 cm per 1,000 years, based upon their U/Th series dating of the Nestor Terrace at Point Loma and its present height above sea level. Suggestions that local faulting has increased this rate in certain areas have not been supported by reliable evidence. Local deformation has occurred.

Because of the difficulty in identifying areas that have been completely stable tectonically throughout the Late Quaternary, there is little agreement on past sea levels, particularly during Interglacial and Interstadial periods of marine transgression. Estimates vary widely, but Evans' figure of 4 meters above present sea level during isotope stage 3, the major Wisconsin Interstade, is generally rejected. The preponderance of opinion suggests that the sea was at or slightly below modern levels then. Final determination of past sea stands must await the development of more precise methods of measurement and dating. Lacking such determination, it seems reasonable to assume that 40,000 years ago, under conditions apparently very similar to the present, the sea stood at or close to its present level.

The origin of the landform that contained the La Jolla Shores frontal is easier to resolve. Although the area in question is no longer available for study, we have good descriptions of the geological stratigraphy, made on the spot by an observer trained in geology and noted for his careful and conservative reporting. There is little doubt the lower fossils were at or very close to sea level when they were deposited by the sea. The laminated white sands were unequivocally attributed by Rogers to deposition by flowing water. Higher levels were unlaminated and could only have been the result of eolian dune-building behind the receding beaches of the Late Wisconsin. Since the lower sands were laminated and the upper sands were without this distinctive marking, despite being of the same composition and being in the same location, the only reasonable explanation is that they were deposited by different agencies, namely, water and wind.

It is highly significant to note that applying Ku and Kern's average rate of tectonic uplift places the fossil frontal bone at exactly 5.1 meters or 17 feet above sea level, the elevation reported by Rogers for it, over a period of 44,000 years, the age deduced for it by aspartic acid racemization (Figure 3).

METERS OF CHANGE IN ELEVATION

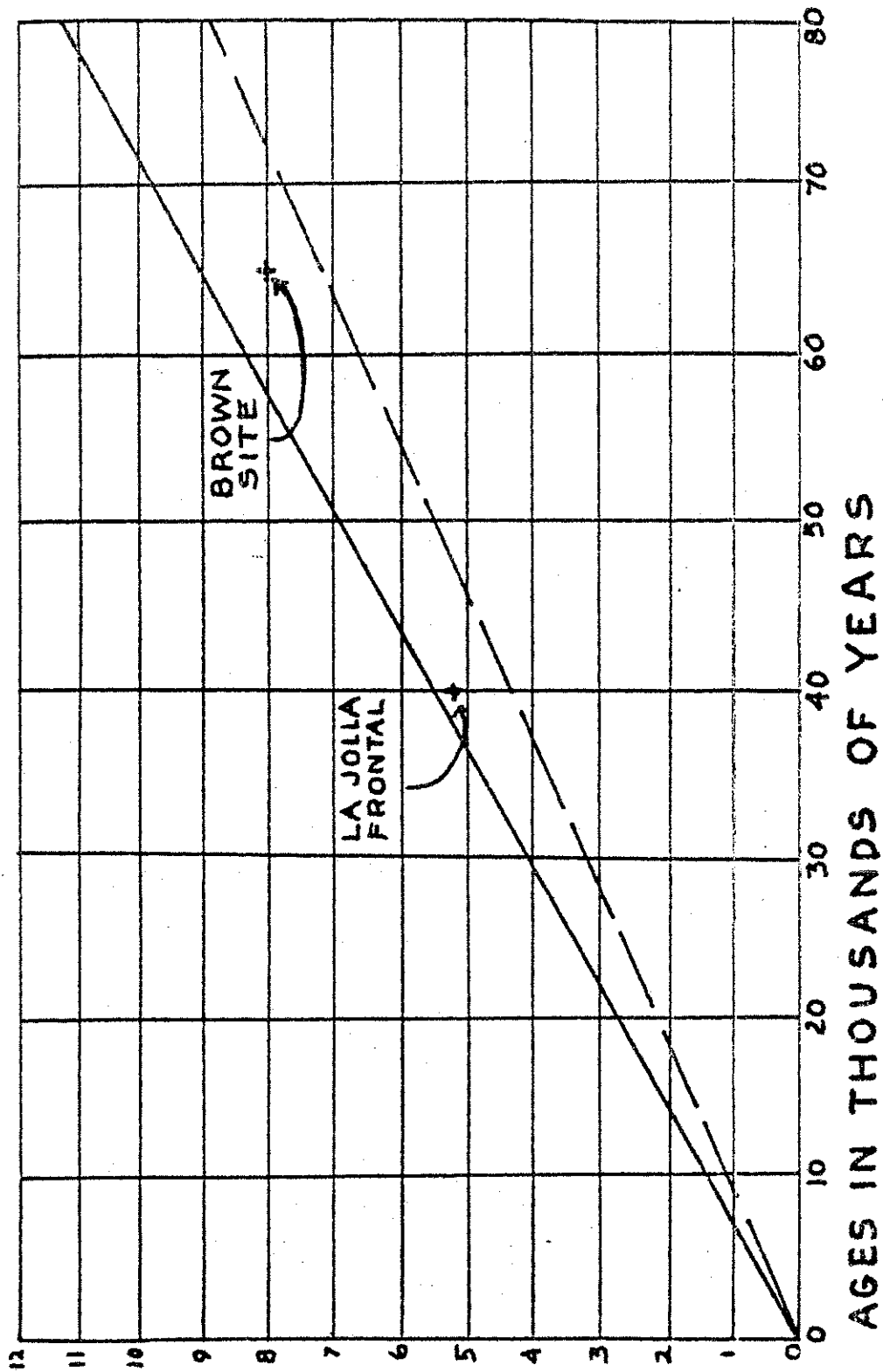


FIGURE 2. Apparent changes in elevation and deduced ages of the La Jolla Shores human frontal bone and the Charles H. Brown Site fossil river channel plotted against Ku and Kern's 1974 estimated average rate of tectonic uplift for the San Diego coast of between 11 cm per 1,000 years (dashed line) and 14 cm per 1,000 years (solid line).

Substantial support for the accuracy of the proposed rate of uplift is provided by the elevation above the modern flood plain of a former channel of the San Diego River, about a mile upstream from the tidal zone at the Charles H. Brown archaeological site. This artifact-bearing gravel stratum, estimated on the basis of its pedological and geomorphic characteristics to have been deposited and then abandoned by the river between 60,000 and 70,000 years ago (Minshall, 1981), is 8 meters above the present flood plain, falling exactly between the parameters deduced by Ku and Kern for 65,000 years.

### Conclusions

The fact that the apparent increase in elevation of the La Jolla Shores frontal agrees exactly with Ku and Kern's estimate of the rate of uplift, and that rate also agrees exactly with the observed rate of uplift at the Brown Site, may just be a remarkable coincidence. Ku and Kern published their report in 1974, in the same year that Bada, Schroeder and Carter announced the racemization age of the frontal bone, but there was no connection nor data sharing between them. The Brown Site was still undiscovered in 1974.

Other mineralized bone fragments were reported by Rogers as coming from below the level of the undisturbed La Jolla burials, and one of these, a rib bone with an olivella shell bead cemented to it, was dated by Bada at 28,000 BP. The other fragments were described as "unidentifiable," and only the frontal had definite provenience. It was seen and removed from the bottom of the cut, while the other bones were scattered through the sands and were first seen when dumped out by the shovel. The elevation and means of deposit of the rib and fragments are unknown.

The argument that 44,000 BP is too early for man in Southern California is refuted by the evidence at the Brown Site, where human presence is convincingly demonstrated at least 60,000 years ago. Still unresolved is the height of sea level at 40,000 BP. If it was far below the present sea stand, and we know it did not rise to the present level until a few thousand years ago, how are we to explain the laminated sands in which the frontal was deposited? If we attribute the lamination to subaerial activity rather than tidal action, isn't it rather strange that the lamination ended above the position of the frontal bone, even though two meters more of sand were later added in the same location and environment?

The question of the validity of the racemization age of 44,000 years for the La Jolla Shores frontal bone cannot be finally resolved by the kind of circumstantial evidence offered here. Taken together, however, the indication of deposit by flowing water plus the correlation between the deduced rate of tectonic uplift and the proposed age and observed elevation of the fossil frontal bone strongly suggest that the latter is indeed more than 40,000 years old.

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A MAJOR CHALLENGE TO "SAN DIEGUITO"  
AND "LA JOLLA"

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*This paper concerns change in the conduct of archaeology within the San Diego region. Local practice is seen in a transitional period that will result in new approaches to material culture definition, chronology, and explanation. For illustration, data from a test-excavation program at five sites near Del Mar is portrayed as seriously anomalous and a new approach is suggested. This approach accounts for the anomalies and others within the region, as well as for non-anomalous findings, and is recommended for future refinement, elaboration, extension, and testing.*

Over the last twenty-odd years, historians and philosophers of science have paid considerable attention to the role of anomalies in helping scientific communities achieve significant change (some might say improvement) in the models which guide their perspectives, goals, methods, and conclusions. An important proponent and symbolic leader of such work is Thomas S. Kuhn (1970, 1977), who defines an anomaly as some datum or phenomenon which does not agree with predictions or expectations that derive from prevailing models of scientific "reality." An anomaly will challenge such models unless it can be encompassed as a special case, explained by minor revision of the models, or discounted on the basis of improper procedure, faulty logic, or the intrusive effect of other factors (including instrumentation). Kuhnian analysts and their cousins suggest that model revisions and special case explanations accumulate with time and with increasing ability of the explanatory system to define what should and should not occur; accompanying refinement and specificity, is an escalation of conformable and, ultimately, non-conformable data, i.e., normal and anomalous information. As complexity of the explanatory system approaches some limit of cumbersomeness, accumulating and increasingly recognized anomalies lead some members of the scientific community to propose new models that not only embrace or "explain" the anomalies, but also deal effectively with much of the old "reality." Kuhn and others view this as a repetitious cycle, wherein each system of models passes through stages of rebellious formulation; consolidation and acceptance; growth by extension and refinement; escalating challenge; and ultimate collapse and replacement.



Although archaeology, like many social sciences, generally lacks the kind of specificity in methods and results that makes a Kuhnian analysis simpler among so-called "hard" sciences, efforts in this direction have already been made and may be expected to continue. For instance, my own master's thesis (Hanna 1982) traces the paradigmatic biography of Malcolm J. Rogers, an archaeological pioneer in the San Diego area, as well as much of the American southwest and northwestern Mexico. Charles Bull has pursued similar interests in his History of Prehistory (Bull n.d.), of which a condensed version was presented at the 1983 Annual Meeting of the Society for California Archaeology and the Southwestern Anthropological Association (SCA/SWAA) and another (Bull 1983) is included in this volume. A master's thesis by James Moriarty, Jr. (n.d.) will address similar concerns when completed.

From these and other sources, there is abundant evidence that San Diego area archaeology is in a transitional period. Nor is there doubt that anomalies have been encountered for some time and are being seen with increasing frequency at present. For example, Dr. Paul H. Ezell has called for inclusion of milling technology as typical of "San Dieguito" assemblages in a recent publication (Ezell 1982) and his SCA/SWAA address (Ezell 1983) which is also a part of this volume. Drs. Claude Warren (1983) and Makoto Kowta (1983), in their SCA/SWAA addresses, called for similar flexibility in the culture-chronological patterns and material culture inventories which they defined some time ago. Kaldenberg and Ezell (1974) and Kaldenberg (1976) noted disturbing irregularities in the stratigraphic occurrence of "San Dieguito" and "La Jolla" material at the Great Western Site. Norwood (1980) observed supposedly younger subsistence-settlement patterns at radiocarbon-dated "La Jolla" sites within Fairbanks Ranch. Even Malcolm J. Rogers had begun to seriously doubt some of his earlier conclusions about "San Dieguito" prehistory by the end of his life in 1960 (Hanna 1982), although these are not clearly addressed in the posthumous publication of his Ancient Hunters of the Far West (Rogers et al., 1966), which is an edited and revised version of his original draft. Numerous other examples could be given, including my own present efforts, but it is perhaps sufficient to say that disturbing anomalies and unorthodox explanations have been widespread and increasingly common within the past few years.

If the local archaeological community is, indeed, in a transitional period that is subject to the influence of anomalies, then it is important to know what the past and still prevalent, as well as competing and increasingly current models are. A major goal in past research has been to define culture-historical material culture units and trace their spatial-temporal distributions. This goal remains important today, partly because cultural history pervades the published and unpublished literature, and partly because Cultural Resource Management (CRM) practice favors adherence to past

models as a way to make management recommendations that governmental agencies can readily understand. One of the first things most local archaeologists want to know, when they discover a prehistoric site, is its "age" with respect to the cultural history originated by Malcolm J. Rogers and modified subsequently by others. One of the first things a governmental agency asks is how information from that site can contribute to our current understandings of that period in prehistory. With the recent passage of Assembly Bill 952, which seeks to avoid redundant data recovery and mandates a focus on "unique archaeological resources," there may be an increased emphasis upon essentially culture-historical concerns. A list of "Significant Archaeological Research Questions" that was recently adopted by the San Diego County Board of Supervisors includes matters which do not fall exclusively within the purview of cultural history, but the majority of questions seek either to flesh out culture-historical depictions with details about subsistence-settlement adaptations or to test the validity and utility of adaptive processes or relationships advanced to "explain" a fairly orthodox culture-chronological interpretive system. Yet, what do we really know (or think we know) about cultural chronology and how relevant are our definitions?

The local antiquity of man is debatable, with some people suggesting occupation far back into the Pleistocene and others rejecting such claims for various reasons. The earliest generally accepted archaeological culture is termed "San Dieguito," which is often dated from roughly 12,000 to about 7,500 B.P. and portrayed as a hunting-focused adaptations that can be recognized as including (Davis et al. 1969; Rogers et al. 1966; Warren 1961):

- 1) various chopping tools
- 2) various plane-tools
- 3) rounded-end-scrapers
- 4) side-and-end-scrapers
- 5) ovoid scrapers
- 6) long-stemmed point/knives with weak shouldering
- 7) San Dieguito Type I knife/points with one pointed and one rounded end, a lozenge cross-sectional shape, and general narrow-thick configuration
- 8) San Dieguito Type II knife/points made as thinned bifaces
- 9) several styles and sizes of crescentics.

Variants of the "San Dieguito" pattern have been defined throughout much of western North America. Divided into "aspects" (Rogers et al. 1966), these variants are sometimes identified in association with certain types of supposed house circles, cache circles, and non-functional or representational intaglios. "San Dieguito" is often seen as lacking a milling technology, especially in the San Diego area, but Ezell (1982; 1983) considers milling to be characteristic of even early "San Dieguito" from the Pacific Ocean to trans-Pecos Texas, while Warren (1983) implies milling technology for at least late "San Dieguito."

An intriguing possibility which deserves consideration is D. L. True's (1958) "Pauma Complex," which is defined for prehistoric assemblages in northeastern San Diego and adjacent areas and purportedly includes elements of both "San Dieguito" and "La Jolla." Flaked-lithic tools are seen as commonly available felsitic materials and "imported" jaspers and chalcedonies, some of which are actually available locally and west of the Transverse Range. Flaked-stone tool forms include leaf-shaped and triangular unnotched points, blade-based items, plano-convex scrapers, and crescentics. Manos are fashioned from shaped and unshaped cobbles and may be either unifacial or bifacial. Metates may be packed or ground and are often basins, rather than slab forms. Dependable dates for the "Pauma Complex" are not available and this pattern is seldom identified outside the geographical area for which it was defined.

The final culture-historical period is termed Late Archaic by some and Late Prehistoric by others. In northern San Diego, it is equated with the "San Luis Rey" sequence (Meighan 1954; True et al. 1974), which is considered ancestral to ethnohistoric Luiseno. In the rest of San Diego, the Late Prehistoric is equated with the "Yuman" sequence (Rogers 1945; Warren 1968) and regarded as ancestral Kumeyaay or Diegueño. A "Pre-ceramic Yuman" period has been suggested for roughly 3,000 to 1,200 B.P. (Moriarty 1966; Smith and Moriarty 1982), which overlaps estimates for the "La Jolla" pattern. Termination of the Late Prehistoric is conventionally dated to the foundation of Spanish missions, such as Mission San Diego de Alcalá in 1769, but more recent aboriginal cultures are seen as modified continuations of Late Prehistoric patterns.

Late Prehistoric economies are viewed as variants of a generalized hunting-gathering adaptation that exploited virtually all floral and faunal resources from coastal to mountain to desert zones on a seasonal basis. Populations extending west from the crest of the Transverse Range are usually considered to have been territorially distinct from those extending east to the mountain crest, although alliance and cooperation between the groups is seen in light of archaeological and ethnohistoric evidence for trade, intermarriage, and similar material culture inventories. There have been occasional suggestions of horticulture and/or incipient agriculture in late Late Prehistoric societies, although the assessment is complicated by historical factors. Among suggested horticultural/agricultural practices are regular burning, restricted impoundment and channelization of runoff, seed-broadcast, intentional culling and perhaps planting of wild flora, and cultivation of crops most closely associated with the desert American West and Mesoamerica. Late Prehistoric material culture is usually recognized on the basis of ceramics, cremations, a refined flaked-lithic tool inventory (including certain projectile points, such as Desert Side Notched), and a fully elaborated milling technology (including slick/basin and bedrock mortars, as well as various manos and pestles). So-called "Pre-ceramic Yuman"

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assemblages are similar, save for the absence of ceramics, to ceramic-bearing "Yuman" assemblages; they also share features with "La Jolla," which has led to suggestions of temporal overlap and possibly social agglutinative processes.

This outline of cultural chronology has been understood or interpreted in various ways over the 50-odd years of local archaeological research history. It is fair to say that many interpretive efforts have been directed by a desire to describe and define a given site's record, link it to a major culture-historical category or propose a new one, and then make sweeping generalizations about temporal-spatial distributions of the patterns, phases, and sometimes new "complexes" that have been "recognized" at that site. While this practice may have been understandable and necessary during our early disciplinary history, when almost nothing was known and prevailing models favored culture-historical definition, large-scale generalization from single sites has probably significantly biased our understandings and definitions of regional patterns. Small sample size, haphazard sampling strategy, and major methodological differences between site samples have also reduced control over the comparability of data within a regional framework. Indeed, there has been no concerted regional approach, although there have been numerous regional extrapolations. Perhaps sadly, most of what is currently accepted as culture-historical fact (both material culture definition and chronology) has come from just such a context and has been almost indiscriminately applied well into the CRM era.

Where interpretation of the archaeological record has gone beyond mere culture-chronological definition, it has frequently taken the form of demographic change (including migration) advanced to explain apparent typological breaks between culture-chronological categories. For example, from 1929 to the present, "San Dieguito" people have been suggested as having moved from desert areas west of the Transverse Range, in the process displacing such inhabitants as may or may not have already been present. Similarly, "La Jolla" folk are seen as having moved in from elsewhere, perhaps the Channel Islands region, to displace "San Dieguitoans." Moreover, "Pre-ceramic Yumans" are portrayed as having moved in from the Lower Colorado-Gila Rivers area and adjacent desert region, first to amalgamate with and ultimately displace more traditional coastal "La Jollans." So-called "Ceramic Yuman" culture is sometimes seen as the result of trait dissemination or population influx from the east, or possibly both, while population movement (the "Shoshonean Wedge") is used to explain the appearance of "San Luis Rey" culture, i.e., Luisenic people. Migration of Late Prehistoric populations in association with the brief history of (Holocene) Lake Cahuilla has often been seen as a major factor in the distribution and formation of Late Prehistoric archaeological cultures east and west of the Transverse Range.

Another type of interpretation might be termed the "permissive" hypothesis of cultural ecology. Especially popular since the late 1960s, this view assumes that all successful cultures were adapted to their natural and social environments, so that speculation on the pattern of cultural-environmental adaptation may be advanced to both depict and generally date meaningful temporal-spatial units or patterns. Not surprisingly, this approach does little to challenge but does much to fill in orthodox cultural histories. An example of this perspective is the idea that "San Dieguito" big-game hunting was feasible because Late Pleistocene natural environments were cooler, moister, and therefore more productive of large mammalian terrestrial fauna than is contemporarily the case. Similarly, the "La Jolla" pattern has long been associated with the existence of shoreline features and marine biota profiles that differed significantly from those of today. The existence of Late Prehistoric patterns is popularly believed to correlate with environments that roughly approximated those of the modern or at least early historic period. Although the "permissive" ecological hypothesis embodies causal assumptions, it has usually been advanced as an after-the-fact explanation and not as a testable proposition. Elements of this outlook can be seen as far back as Rogers' work in the 1930s (Hanna 1982), even though he was reluctant to specify either the approach or the assumptions on which it was based.

Directly, causal arguments have been advanced within culture-ecological interpretation since the late 1960s, gained increasing favor in the 1970s, and have attained full respectability on the local scene in the early 1980s. Again, however, there has often been an emphasis on explaining the details or justifying the scenario of orthodox culture-history; the idea has generally been that pattern changes in the archaeological record are reflections of adaptive response to directive changes in natural environment. For example, loss of environments favoring large terrestrial fauna has been advanced to explain loss of the "San Dieguito" pattern, the development or successful importation of milling technology, and consequent evolution toward or influx of "La Jolla" pattern occupations. Smith and Moriarty (1982) are clearly interested in this kind of explanation, since they link southward restriction of "La Jolla" coastal villages to a parallel erosion of the lagoonal, estuarine, and foreshore environments upon which "La Jollans" were purportedly dependent. Similar cultural-environmental directional issues are exhibited by much of the CRM literature, including the possible influence of over-exploitation upon molluscan populations, e.g., the idea that Donax sp. shellfish remains may typify Late Prehistoric coastal sites because "La Jollans" had depleted Argopecten sp. and Chione sp. beyond a threshold of economic utility. Another notion along much the same lines is that loss of Lake Cahuilla in the Salton Trough precipitated large-scale population movements and changes in economic adaptation, these effects being seen in subsequent Late Prehistoric deposits and the nature of material culture inventories revealed by them.

A very different approach, the systems-oriented investigation of regional subsistence-settlement adaptations through time and space, has gained considerable local lip service since the mid-1970s but has yet to be implemented or attempted beyond the context of large CRM projects that contain several sites. Even in the latter circumstance, there has been a greater emphasis upon after-the-fact speculation than upon explicit formulation and testing of relevant hypotheses that reflect a regional sampling approach. One result has been that large, multi-site CRM projects identify potentially significant anomalies but cannot really deal with them, save to suggest that further research may lead to modification of existing understandings about chronology and archaeological culture depictions or definitions.

There seems to be a common thread linking most of the interpretations and explanations which will be found in existing literature, both published and unpublished, both pre- and post-CRM. This thread is the defense, elaboration, and extension of orthodox culture-historical reconstruction. Even where anomalies have been reported, and they are not so uncommon, they seem very often to be presented in the light of vaguely promising possibilities for future research. Where possible, they are used to precipitate change in material culture inventories without necessarily challenging the overall structure. Ezell's (1982; 1983) inclusion of milling in "San Dieguito" might be classified in this manner, as he apparently retains the idea that "San Dieguito" is a meaningful cultural, temporal, and spatial unit. Where recent CRM reports identify anomalies, they are noted more for their unusualness than for their significance, which is to say that a new model is seldom advanced to explain them and the reader is left to decide how they should be dealt with. I do not imply that the types of interpretation mentioned above are without value. Quite the contrary, they are particularly appropriate directions for research and embrace certain questions that may be crucial for meaningful change to come about. However, what demands attention is the degree to which our recognition and use of anomalous data is directed by the goals and interpretive models which lie behind our work. To demonstrate my meaning, I will discuss results from test-excavations of five sites at the proposed North City West (NCW) 7th. Neighborhood, which is south of San Dieguito River Valley, east of El Camino Real, and north and west of Black Mountain Road.

The NCW 7th. Neighborhood is a highland coastal plateau area comprising several mesas that are separated by deeply entrenched side drainages to the San Dieguito River, to the north, and to Carmel Valley, to the south. Vegetation is a mixture of Coastal Sage Scrub and Chaparral species. Soils are sandy loam and sand, while bedrock is sandstone and cobble conglomerates. Erosion and historic disturbance have been severe in places but are generally moderate to slight. The area was never farmed. Rodent activity varies from moderate to minimal. Five prehistoric sites have been tested to date; a sixth is currently being tested.

Four of the five tested sites contain mostly shellfish remains, some fire-affected rock, a small quantity of flaked-lithic and groundstone tools, and very little stone-flaking debris. All four shell middens would be identified as "La Jolla" deposits by most archaeologists, especially as the shell is primarily Argopecten sp. and Chione sp., along with some others, but does not include Donax sp. Basal dates were obtained for each midden through radiocarbon analysis of shell adjusted via  $C^{13}/C^{12}$  ratios. The dates were 6310  $\pm$  120 B. P. (SDM-W-2308), 5500  $\pm$  150 B.P. (SDM-W-2306), 4910  $\pm$  120 B.P. (SDM-W-2311 or SDM-W-26B), and 1590  $\pm$  130 B.P. (SDM-W-2307). Since these are basal dates, each site extends to some temporal point of younger age, overlap between sites is likely, and the youngest may extend well into the Late Prehistoric period's approximate range. The sequence embraces that of 7,500 to 3,000-2,000 B.P. suggested for "La Jolla" by Moriarty (1966), or 11,000 to 1,500 B.P. according to Smith and Moriarty (1982). According to Smith and Moriarty (1982), the NCW 7th. Neighborhood area was abandoned by "La Jollans" by about 3,000 B.P., but this appears unlikely on the basis of our radiocarbon dates. It deserves mention that a Desert Side Notched point was earlier (Hanna 1981) found on the surface of SDM-W-2307, whose base dates to 1590  $\pm$  130 B.P.

The fifth tested site, SDM-W-26A, is very different from the others in several respects. It contains several loci, of which only one contains faunal remains: a few shell fragments outside the area of midden deposit, a few shell and bone fragments (including an antler flaker tip) in one of eight midden-test units, and one shell fragment on top of the midden elsewhere. Other SDM-W-26A loci include another occupational midden, several quarry-workshop scatters, two major quarry-workshop areas, and an old "shrine" or ceremonial center (about which more later); two other occupational deposits are suspected but not directly evidenced by available data.

To convey a sense of the importance adhering to SDM-W-26A and the anomalous data which it contains, let us consider a hypothetical case. Imagine we are within a few miles of the contemporary Pacific coast, within a mile of the now-silted and artificially filled Del Mar (San Dieguito) lagoon, and somewhat more distant from the former lagoon at the mouth of Carmel Valley and Soledad Canyon (Los Peñasquitos lagoon). Say that we find a site which is full of Chione sp. and a few other types of shell, contains abundant evidence of slab metate milling technology, has cobble-outlined and cobble-lined and cobble-platform and non-cobble hearths, has some stacked-stone and stone-outlined features that are not hearths, is wealthy in flake- and cobble-based tools (planes, choppers, scrapers, etc.), includes thick and thin unifaces and bifaces, has several crescentics, and also contains ceramic sherds. Given these data, we would probably identify "San Dieguito," "La Jolla," and Late Prehistoric components.

If we were to remove ceramics from this hypothetical case, it is perhaps likely that no Late Prehistoric component would be identified; recall that there are no Late Prehistoric points or cremations in our picture and



bifacially thinned tools can be "earlier." Let us now remove most of the shellfish remains, leaving only a small surface scatter near or outside the midden. We might then feel less secure about our identification of a "La Jolla" component, but would probably retain it on the basis of other (stone) artifacts and hearth features. Let us now also remove the crescentics. While we might still identify a "San Dieguito" component on the basis of other material, we might be tempted to identify some variant of the "Pauma Complex." There is a good chance that we would ultimately identify an earlier "San Dieguito" and later "La Jolla" component, perhaps with some intervening hiatus, as the "Pauma Complex" has been identified only in northeastern San Diego and is not defined to include a slab metate milling technology. We would in no case posit a Late Prehistoric occupation.

This is precisely the situation with SDM-W-26A, whose main midden (Locus 8) was tested with eight excavation units, twelve shovel-test pits, three unit-based circle-plot surface collections, a zonal surface collection, and opportunistic recovery of index artifacts. Other SDM-W-26A loci were evaluated through resurvey, transit-mapping, shovel-test pits, opportunistic recovery of index artifacts, zonal surface collection (part of Locus 2), and collection of raw lithic material (the major quarry at Locus 12). Transit-mapping and photography were employed at the "shrine" (Locus 1), which is a mesita of Lindavista Formation sandstone that supports a scattered hearth, low-density flake-tool-hearthstone scatter, one stone-outlined circle, five weathered rockpiles or cairns, a straight-aligned stone structure, a zig-zag stone structure, and a three-sided stacked stone structure. Sightings across Locus 1 feature pairs yield lines-of-sight to such major regional topographic relief features as La Mesa (behind Ensenada and Tijuana), Tecate Peak (visually between Cowles and Fortuna Mountain, the San Dieguito River Gorge (above the Harris site, visually), the highest peak of Mount Palomar, Mount San Geronio, and Mount San Antonio.

We know a great deal about past interpretations of SDM-W-26A. The site was first recorded by Rogers (N.B. 6) in 1929-30 during a regional survey. Rogers' attention focused on Loci 1 and 8 and the major quarry-workshop Locus 12. The course of his evaluations can be seen clearly in the notebook record (Rogers N.B. 6:31):

Practically everything is on the surface and only a film of soil is left on the red sandstone. On the north end of this ridge where there is an outcropping of river gravel [Locus 12] is an extensive stone flaking station. (1) sandstone metate (1) porphyritic metate. One plane (felsite) finely secondarily chipped. Many proto-planes and hand choppers here. Site covers an acre [references to Locus 8].

Only a few *Chione fluctifraga* were found here. Very many hearths. Much felsite for an old site. On the highest point of the ridge is a bare platform of sandstone [Locus 1] which probably was a shrine. It measures 60' in diameter. On it blocks of sandstone have been arranged in small and large circles.

The notebook record also identifies SDM-W-26A as a "dual site" comprising "proto-Scraper Maker/La Jolla II" and "Scraper Maker/San Dieguito II." Rogers' formal site record mentions "San Dieguito II" and "Littoral II" (La Jolla) occupations. The site record also describes SDM-W-26A as a "Highland accretion midden" and further states:

ARCHITECTURE: On the highest point is a red crag outcrop 60' in diameter. On it some 8 small and large circles of sandstone blocks are arranged. These are like SD-I circles of the central aspect, but are probably SD-II or La Jolla II house circles and cache circles uncovered by erosion of the covering soil. Some are too disturbed to make out their original form. They are the only ones ever found in the Southwest Aspect until one gets down into Lower Calif. Many SD-II and Lit. II hearths are present, and the roasting platforms of the latter people. The SD people usually made hearths without confining rim cobbles, but when they did they are neat circles of evenly graded cobbles 14" to 16" in diam. A few rectangular ones have been found also.

HISTORY: This was first a SD-II site, and then not used again until Lit. II times. Both cultures' occupations are of about equal intensity although the Lit. II built the thin bedded shell midden. The SD people did not build shell middens and seldom ate shellfish.

REMARKS: On the north margin where old beach gravels are uncovered is an extensive quarry, mostly Lit. II work. Metates and manos are scarce here. Some worked stone in the hearth on the platform are fire cracked, and are of the La Jolla II phase. Probably their stone circle houses are of the La Jolla II origin.

Notice that Rogers identified "San Dieguito" and "La Jolla" components, with an intervening hiatus, on the basis of materials that he attributed to culture-historical units. Note, also, that he made no mention of a Late Prehistoric occupation at the site. The distribution of artifacts, features, and ecofacts at SDM-W-26A was used to make culture-historical assignments because they fell into preconceived material culture definitions and seemed to agree with a successional scheme. Further, ideas about mainland adaptation appeared to be supported or were made to agree

with Rogers' culture-chronological outlook, e.g., his statements about "San Dieguito" hearths and shellfish exploitation. Rogers may have identified the major quarry-workshop as "mostly Lit. II work" because the artifacts at that locus are primarily split cobbles, discarded or unfinished blanks, large percussion flakes, and items which are morphologically similar to "La Jolla" crude tools.

In addition to making a formal site record and notebook entries, Rogers collected items which he felt to be diagnostic and took several photographs of SDM-W-26A Loci 1, 8, and 12. These are all available at the San Diego Museum of Man. Comparison of the collection and accompanying item-by-item notes with Rogers' other site-specific data reveals that he placed major reliance on the same indices of culture-historical categories that are widely used today. It is unlikely that Rogers conducted subsurface tests at SDM-W-26A because his notes contain no mention of such work and he failed to appreciate the extent, depth, and structure of occupational mid-den deposits.

SDM-W-26A was visited several times after Rogers' survey of 1929-30. CRM-type surveys found only some of what Rogers reported, and the main midden (Locus 8) went unrecognized until its rediscovery by a survey team under my direction (Hanna 1981). As we now know, the site is considerably more complex and Locus 8 is both larger and deeper than Rogers and later surveyors (including Hanna 1981) realized. Since the bulk of anomalous data from SDM-W-26A resulted from testing at Locus 8, the methods that were employed should be briefly discussed.

A major goal of Locus 8 testing was to achieve good control over natural and cultural depositional and erosional history. Some structural indications were evident on the surface in the form of soils, sheet-washed areas, gullies, density and type of vegetation, bedrock, sandstone and cobble conglomerate outcrops, slope gradients, and the distribution of cultural material. An initial three test units (one meter to a side) were excavated just east and downslope of a prominent north-south ridges. Two were purposeively placed in an area of moderately dense surface artifacts; the other was placed outside this zone in an area that had been eroded through sand nearly to bedrock.

Results from the initial units suggested surface duff/humus and several centimeters of vertical leaching had obscured the presence of a more substantial deposit. Twelve shovel-test pits were therefore excavated and the soils screened at locations across the locus that permitted exploration of developing ideas about general structure and depositional/erosional possibilities. Two additional test units were placed at hearths in an eroded area (downslope and east of the initial units) whose soil was similar to one of the initial units and the deepest levels of the other initial units. A sixth unit was placed in an area southeast of the mesita,

where shovel-test pits had revealed deep deposits that all earlier surveys had failed to detect. A seventh unit was set fully inside the midden area that was initially tested, positioned to intrude partway within a large suspected hearth. Circle-plot collections were conducted at the fourth, fifth, and seventh units to increase quantity of surface materials associated with hearths and improve comparability between what were thought might be temporally distinct areas. Extending this logic, the entire locus was divided into four zones for a surface collection of potentially or theoretically diagnostic ("index") artifacts. These zones correspond to the ridgetopping midden zone, the eroded area downslope from it, a less eroded area farther downslope, and the area lying east, south, and west of the mesita (where largely buried deposits had been discovered). An eighth test unit was ultimately added to explore a partially exposed pile of stones near the northern extent of the eroded area; this feature was found to be a cobble-platform hearth.

It bears mention that twelve shovel-test pits were also excavated at four other SDM-W-26A loci, a "goodie" collection was conducted at a small part of one of these loci, opportunistic "index" items were recovered and plotted wherever encountered, and the entire site was resurveyed and mapped. In addition, raw material samples were recovered from the major quarry workshop locus at the site's northern end (Locus 12) and compared with samples taken at natural outcrops throughout the San Dieguito and Carmel Valley drainages, as well as with stone artifacts from SDM-W-26A and the other tested sites.

Laboratory work beyond the cataloging level included microscopic examination of flakes/debitage, cores, and stone tools; dimensional measures of flakes from the initial Locus 8 units; consultation with two local knapper specialists (Rod Reiner and Richard Cerutti) as well as literature research, this for examination of production and use characteristics of all flaked-lithic materials; and formulation of descriptive groups or "types" for all tools on the basis of size, shape, damage (where use was the readily inferable cause), and regular configuration of functional features (edges, ventral surfaces, sides, etc.), within a group's other characteristics. The same procedures were utilized with the other tested sites as well.

Some additional notes on field procedure will suggest the importance of stratigraphy and association during excavation of test units and later treatment of recovered materials. Although a ten-centimeter interval was used as the basic vertical control, individual features were excavated independently by levels and their materials kept separate from other features and non-featural unit levels. Within each level (feature or general unit), natural strata were further segregated and the results kept separate during all phases of laboratory work and analysis. The same segregational rules were observed with respect to soil samples for Munsell color-coding and soil descriptions. Plot sheets were drawn at the surface, the bottom

of each ten-centimeter level, for features, and for major soil transitions. A similar approach was taken with shovel-test pits, in that cultural materials were segregated by soil type and the depths of transitions recorded in the notes.

Briefly, the following statements can be made about Locus 8.

- 1) The general structural progression in soils from bottom to top is sandstone bedrock including small areas of cobble conglomerate, pockets of clay and sandy clay, sand, sandy loam, vertically leached sandy loam, and a cap of duff/humus.
- 2) This entire sequence is preserved in the ridgeline midden, which is located mostly east and downslope of the ridgeline in the northern part of the locus. Lateral groundwater movement has leached organic molecules from younger, overlying strata. The cultural deposit is about forty to forty five centimeters deep.
- 3) In the eroded area downslope from the ridgeline northern midden, the only strata above bedrock are clay pockets, sandy clay, and sand; some of the latter may be leached sandy loam. Midden development may never have been favored here, due to slope gradient, distance from the top of a runoff surface, and situation at a position where groundwater discharges regularly (less permeable bedrock crops out a short distance downslope). The cultural deposit is about twenty centimeters deep.
- 4) In the southern part of the locus, erosion and collapse of the mesita margin have produced substantial sand deposits on gentle slopes to the northeast, east, southeast, and south. This occurred during and after occupation, so deep cultural deposits are largely buried. At Unit #4, southeast of the mesita, fissured bedrock was encountered at about sixty centimeters depth. Severe lateral and vertical leaching have not favored preservation of organically rich midden, although saline flotation may produce sufficient charcoal for radiocarbon dating.
- 5) West of the ridgeline there is little, if any, true subsurface deposit. In the northern part of the locus, an area of nearly sterile sandy loam extends westward to native sand with surface and slightly embedded artifacts. West and southwest of the mesita there is sand with primarily superficial and a few embedded artifacts.

- 6) Shellfish remains are known to exist in only three places. A low-density scatter lies on and slightly embedded within the sands northwest and west of the mesita. One shell fragment was found on the surface northeast of the mesita. A miniscule amount of shell was recovered from a hearth in one of the ridgecapping midden test units.
- 7) There is no stratigraphy other than the gradual soil sequence and a complex superposition of hearths, fired soil layers, compacted soil layers, and pits cut through a fired-and-compacted layer in Unit #4. However, these stratigraphic elements are very well preserved, to the extent that individual hearths can be directly related to surrounding compaction layers, natural soil differences remain gradational outside of man-made features, very minor disturbance can be detected in direct relation to root growth, and there is no evidence of rodent burrows in any test unit (rodent burrowing was seen in only one part of the ridge-capping midden on the surface). The paucity of rodent burrowing is probably due to substantial resident soil moisture inimical to the burrowers.
- 8) Association is ideal at all parts of the midden, including the eroded area. Examples are recovery of artifacts on the surfaces of fired and compacted soil structures, within such structures, and inside hearths (including cobble-outlined, cobble-lined, cobble-platform, and non-cobble varieties); direct associations of compacted surfaces with hearths; associations of such surfaces with man-made pits (both linked by a partially leached ash structure; and recovery of artifacts within these pits (in Unit #4).
- 9) There is no evidence of an occupational hiatus in the cultural deposits. Rather, continual re-use of the site with nothing more substantial than seasonal breaks is suggested by the gradual progression of soils, the frequency of intrusive pitdigging for hearths and possibly other kinds of features, direct evidence of hearth reconstruction or renewal (matching fire-cracked stones from widely separated places within individual hearths), and a broad continuity in behaviors as suggested by kinds of artifacts from bottom to top of deposits.
- 10) The stratigraphic placement of "index" artifacts and technologies, controlling for featural intrusion and in the presence of ideal stratigraphic and associational measures, depicts a single cultural pattern that changed only slightly over time. A slab-metate and shallow-basin-metate milling technology appears to have become more elaborate from the bottom to the top of the deposit. This is

10) (continued)

especially clear in the distribution of handstone varieties. Chopping, scraping, pulping, and battering tools of the "San Dieguito," "La Jolla," and Late Prehistoric patterns are found from bottom to top of the deposit, but in mutual association and inverted, as well as purportedly proper sequence. Supposedly, "San Dieguito" crescentics occur on the surface with Late Prehistoric, "San Dieguito," and "La Jolla" tools; above and below sealed subsurface strata that contain "La Jolla" and possibly Late Prehistoric items, and within a man-made pit. The compaction surface surrounding the pit contain both handstones and other "La Jolla" or more recent items indicating an economy based partly upon vegetal exploitation and milling.

- 11) Although there are no ceramics in the locus (at least, none were found), a Late Prehistoric occupation is indicated by the radiocarbon date of  $610 \pm 50$  B.P. obtained on charcoal from a perfectly preserved cobble-lined hearth (Unit & Feature 1) at the top of the ridgetopping midden. Charcoal samples were analyzed by a specialist and identified as some kind of conifer (probably Pinus sp., possibly P. torreyana) and an unknown, arid-adapted woody shrub (apparently not a species that exists on or near the locus today).
- 12) Water may have been obtained aboriginally from the margins of the locus and the site in general, as well as from adjacent drainages. The sandy loam and sand store precipitation readily and discharge it at contacts with less permeable bedrock, especially where both have been pierced by erosion channels. Surface seepage was observed from November of 1982 through April of 1983, after an approximately 18 inches of seasonal precipitation and two prior years of greater-than-average rainfall. During the same period, canyons adjacent to Locus B (and SDM-W-26A in general) exhibited intermittent surface flow of brief duration.

Rather surprising results were obtained from the other SDM-W-26A loci, as well. An example is the radiocarbon date of  $670 \pm 80$  B.P. on charcoal from a small cobble-outlined hearth within a large area of Locus 13 that otherwise contains only a very low-density surface scatter of quarry and flaking debris, and an occasional tool. One of these tools is a so-called Type I San Dieguito knife/point. Another cobble hearth (whether outlined, lined, or platform is not clear) is partially exposed at the top of the other midden (in Locus 9). Although neither this hearth nor the Locus 9 midden were examined by test units, shovel-test pits and surface evidence suggest striking parallels to the Locus 8 ridgetopping midden. Included among

these parallels are situation on and downslope (eastward) from the ridge-line, the same soil progression, an absence or extreme paucity of shell, and the occurrence of Late Prehistoric, "La Jolla," and "San Dieguito" indicators on the surface. At Locus 1 (mesita), comparison of a stacked-block structure with a 1929 photograph of it reveals rapid weathering. Together with feature-pair lines-of-sight to ethnohistorically important peaks in the region, rapid weathering over a fifty-four year period suggests Late Prehistoric age is more likely for Locus 1 features than the identification made by Rogers. Further, Locus 2 features do not seem remotely like "house circles" or "cache circles," and Rogers' idea that the platform features were uncovered by erosion is doubtful. Flakes, manos, and hearth stones on the mesita surface, although few, are comparable to evidence the ridge-capping midden and eroded zones of Locus B. The existence of artifacts in sand and between weathering sandstone blocks around the mesita edge implies that cultural debris atop the mesita platform is probably younger than material from the subsurface at Unit #4 in Locus 8. At Locus 2, just north of Locus 8, is a quarry-workshop area with tools and production debris materials at Locus 8.

Given the information presented above, it is clear that Malcolm Rogers' interpretation of SDM-W-26A occupational history is seriously flawed. Some of the field results are strikingly anomalous in terms of the stratigraphy and associations to be expected in light of orthodox cultural history, while the validity of index artifacts and traditional material culture assemblage definitions is suspect. The absence of evidence for a hiatus between "San Dieguito" and "La Jolla" occupations is matched by a  $610 \pm 50$  B.P. radiocarbon date at the terminus of a supposedly "La Jolla" midden. Stratigraphic and associational anomalies throughout Locus 8 demand some kind of explanation, particularly as preservation has been ideal in the absence of major structural deformation in the deposits. Linkage between many of the SDM-W-26A loci extends doubt to the site as a whole.

It is important to understand that provisional links have been established between SDM-W-26A and the four shell midden sites. The range in shell midden radiocarbon dates is from  $6310 \pm 120$  B.P. to  $1590 \pm 130$  B.P. Since there are basal dates, each shell midden site was occupied to some more recent time; thus, the occupational sequence must extend to some point later than about 1500 B.P. Both radiocarbon dates from SDM-W-26A,  $610 \pm 50$  B.P. and  $670 \pm 80$  B.P., indicate the terminal age of the site; how far the record extends back in time remains undetermined. Overlap with the shell middens is possible and would help explain the presence of closely similar groundstone, flaked-lithic tools, tool-production debris, and even shell (the small quantity at SDM-W-26A) at all five sites. Unhappily, the quantity of non-shell cultural material from the shell midden sites is very small, one of these sites has been destroyed by grading of soil-test observational exposures, and the others are unlikely to receive further work and too small to make it worthwhile. Further study of the shell midden sites will be restricted to samples during subsurface testing.



The question, of course, is what to do with the anomalies. Since they challenge widely held understandings of cultural history and some of the basic assumptions that are used to "date" archaeological deposits, one can expect attempts to discount the anomalies. One way may be to challenge field procedures as statistically unreliable because random sampling was avoided in favor of purposive unit placement, shovel-test pit placement, and collection zone definition. A challenge of this type might be impressive, but would not explain the stratigraphic and associational data that were obtained under ideal preservation conditions.

Another avenue of challenge might be that tool groups or "types" were defined more on the basis of general formal description than as a result of detailed microscopic examination and statistical analysis. However, the generalized formal approach favors comparability with traditional literature and the pragmatic aspects of culture-historical identification as it is still employed by many CRM and research practitioners. To check for comparability, several professional colleagues were asked to view the collection and sites. Their reactions were in general agreement with two hypotheses about contemporary attitudes, namely (1) orthodox cultural history retains considerable respect as seen in use of index artifacts and culture-historical assemblage definitions, and (2) there is widespread confusion and uncertainty about the usefulness and validity of orthodox cultural history, index artifacts, and assemblage definitions. In brief, the approach to typology which was implemented served its purpose by emphasizing the anomalous nature of recovered data, thus highlighting a need for major changes in perspective, goals, and approach.

A qualitatively different way of dealing with the anomalies can also be expected. This is to accept field and laboratory procedures, thus agreeing that unusual data were encountered, but then "explain" the anomalies by revising orthodox cultural history. For instance, the problem of milling technology having been found in association with and stratigraphically below "San Dieguito" material is resolved by Ezell's (1982, 1983) hypothesis that milling characterized this pattern throughout much and perhaps all of its temporal range, at least in some geographical areas. Warren's (1983) recent statements about inclusion of milling technology in at least later "San Dieguito" phases have the same effect. Similarly, it has been widely observed by archaeologists, often informally but occasionally in print, that the occurrence of crescentics in "La Jolla" and Late Prehistoric deposits is too common to support the idea that these supposedly "San Dieguito" artifacts were collected from earlier sites by later prehistoric peoples. There is also a fairly widespread idea that some crescentics are "San Dieguito" ritual items but others of the same class were produced much later, while some researchers suggest a distinction between "ritual" and "tool crescentics."

There are several problems with modifying artifact inventories for established culture-historical categories. One difficulty is that modifications can seriously alter depictions of technological and social adaptation, subsistence-settlement systems, geographical range, and perhaps temporal range, but they may do so without explicit recognition. That is to say, deceptively minor changes in material culture definition can have consequences of major importance without forcing comprehensive modification or direct testing of interpretive systems inherited from the past. This way of dealing with anomalies avoids their real value as signposts for new research directions and precludes effective exploitation of the archaeological deposits from which they derive.

A related difficulty with modifying culture-historical artifact inventories is that one ultimately faces a situation in which accumulated changes leave virtually nothing to deal with. Lacking dependable control over relative distributions of cultural patterns through time and space, the archaeologist will find his/her feet planted firmly in a foundation of shifting sand. Each new datum may shake that foundation, reemphasizing the need for control but paralyzing efforts to achieve or even attempt it. CRM archaeology could not survive such a catastrophe; given the pace of land development, there might not be enough of the in situ data base left for others to exploit.

There is a better way to deal with the anomalies encountered at NCW 7th. Neighborhood sites; extended, this alternative applies to archaeological practice throughout the San Diego region and farther afield. A two-step approach is suggested. The first step is verifying anomalies by explicitly testing them in the context of orthodox interpretations of such ideal sites as SDM-W-26A. The second step is generating and explicitly testing alternative interpretation at suitable sites within the region in general. Investigation of less-than-ideal sites (referencing preservation in particular) can be brought within the same kind of approach.

As an example of verification procedures, consider some of what can be done with the NCW 7th. Neighborhood sites. Better temporal control can be achieved by radiocarbon-dating upper-level shell recovered from the four shell middens during subsurface testing, and by radiocarbon-dating soils, featural charcoal, or saline-floated soil organics to be recovered from the base of SDM-W-26A middens (Locs 8 and 9) during future studies. At the SDM-W-26A middens, a combination of purposive and stratified random sampling procedures for unit placement can trace out known hearth and compacted or fired soil features, test for intrusive effects of sampling strategy, and provide statistically controlled measures of spatial and temporal distribution in type and intensity of aboriginal behaviors. Block- and strip-excavation are strongly urged for areas of Locus 8 that are known to contain well-preserved occupation surfaces and other features of the type that can reveal much about activities at major settlements. Such data would help depict the

repertoire and distribution of on-site behaviors within radiocarbon-controlled temporal limits and in the context of broad-based, stratigraphically and associationally controlled material culture inventories. Peer review of such information can permit tests of replicability among practitioners' definition and use of orthodox interpretations, as well as their confidence in and alternatives to orthodoxy. With addition of focused special studies in tool-production/use technologies, explicit formal and functional tool analyses, palynological examinations, and the services of a Late Pleistocene/Holocene geologist-pedologist, one could evaluate the validity of orthodox "index" data from several directions, thus validating or invalidating data which are apparently anomalous with respect to conventional understandings.

Providing the NCW 7th. Neighborhood anomalies are verified, what may be done by way of generating and testing alternative explanations? As one example among many alternatives, consider the following and its relation to the anomalies under discussion.

Without necessarily abandoning the general outlines of accepted local chronology (another alternative), two postulates may be advanced. The first is that substantial cultural conservatism has prevailed in the San Diego region for 12,000 years or more of prehistory, such that generalized hunting-gathering has been the economic foundation of social organization and technological change would be cumulative but seldom replacive. A consequence of this postulate is that material culture should become more diverse through time. A second consequence is that older technological forms should be retained in later assemblages. A third consequence is that regional change in material culture should be generally gradual, with sudden and large-scale "horizon" markers being uncommon.

The second postulate is that regional response to changes in natural and social environment over 12,000 or more years of prehistory has been primarily manifest as changes in seasonal and geographical parameters of subsistence-settlement patterning. One corollary is that natural resources which have remained relatively constant in quality, quantity, and location should, within the context of cultural conservatism, have attracted the same human activities as manifest by the archaeological record at a given site. A second corollary is that inconstant or changing natural resources should have exerted differential locational attractions on human activities through time, which would be revealed as changes in the archaeological record at a given site.

By positing cultural conservatism in this model, we relegate to secondary status the influence of population movement, changes in regional population size, and possible changes in ethnicity. However, we do not ignore these factors and we view changes in structural complexity of social organization as reciprocals of relations to natural environment.

An important consequence of the second postulate is that it defines polar extremes in the kinds and sequencing of technologically/behaviorally diagnostic evidence anticipated from intact archaeological deposits. At one end of this spectrum is the site which experienced little or no change in types of activity through time because resource "pulls" remained constant. At the other end is the site which experienced changing "pulls" and therefore reveals different activities in different strata; these could be misinterpreted as significant culture-historical markers. The best opportunity for testing along this continuum is the occupational site that has well-preserved strata and evidence of several primary economic activities. Activities tied to "constant" resources should be identifiable from those tied to "inconstant" ones, and adaptive changes in response to resource changes should thus be both discernible and datable.

The twin postulates of cultural conservatism and subsistence-settlement pattern response to "constant" and "inconstant" resources suggest a dangerous possibility. Suppose that the stratigraphically controlled material culture assemblage changes at a culture-historical type site, such as the Harris site (Warren 1966), in fact reflect subsistence-settlement pattern modifications within an essentially stable regional adaptation, rather than major technological changes and cultural successions. The misinterpretation should go unrecognized for several years because of the regional generalization based upon it would seem to explain portions of the prehistoric record at other sites; incompatible data would be missed, ignored, or made to comply in some fashion; attention would be focused on elaborating the interpretation already in use; and considerable difficulties would have to be experienced before the traditional outlook became subject to re-evaluation. This situation is one which Bull (1983) warns us of in his observations about the importance of regional sampling.

Regional sampling presents a paradox. Archaeology is not locally able to pick and choose its loci of research, since most studies are performed by CRM practitioners in response to political and market factors. One way around this limitation is testing of regionally relevant models of individual sites or site clusters within a given CRM project, and some progress in this direction has been made by the San Diego County Board of Supervisors decision, taken in April of 1983, to adopt a list of "significant archaeological research questions" developed by a task force committee. Nevertheless, it will remain important for each practitioner to also implement approaches which he/she may find have special applicability in each case. Another way around the limitation is utilization by others of models suggested at sites other than those they are dealing with, which will require alertness to similarities and considerable sharing of ideas and information. Although it falls short of the idea, this kind of regional testing can integrate challenges to orthodoxies that are presented by anomalous findings and interpretations based upon them.

The model outlined above can be applied to the NCW 7th. Neighborhood sites. I suggest that SDM-W-26A exhibits the influence of at least three "constant" environmental factors:

- 1) the attraction on quarry-production activities of abundant, good-quality raw material at local cobble conglomerate beds;
- 2) the attraction on vegetal collection and milling activities of abundant and varied flora on the mesa, down bordering slopes, and in adjacent side drainages; and
- 3) the attraction on settlement location of seasonally dependable water supplies at mesa-margin seep/springs and in adjacent side drainages.

Candidates for similar attraction on settlement are eastern exposure, presence of a long slope in the lee of prevailing winds, and incidence of an unparalleled panoramic view. It is suggested that the "constant" factors found expression in the same kind of subsistence and settlement practices at SDM-W-26A throughout its occupational history. Given cultural conservatism, this would explain the presence of milling implements stratigraphically below and above, as well as in association with "San Dieguito" material; the similar distribution of "La Jolla" features and artifacts in relation to "San Dieguito" and Late Prehistoric material; and the presence of 600-odd B.P. radiocarbon dates at the top of the deposit. An apparent absence of Late Prehistoric ceramics, points, and cremations would seem to imply brief seasonal occupation and restricted economic pursuits; although this is only a provisional view, it would seem to agree with further points noted below.

A similar approach can be taken to similarities among the four shell midden sites, and to similarities and differences between them as a group and SDM-W-26A. Suggested as relatively "constant" environmental factors at the shell middens are:

- 1) attraction on quarry-production activities of on-site cobble conglomerate outcrops;
- 2) attraction on vegetal collection and milling activities of floral resources on the mesas, down some bordering slopes (a few are cliffs), and in adjacent side drainages; and
- 3) attraction on settlement location of seasonally available, but perhaps no abundant, water supplies at mesa-margin seep/springs and in adjacent side drainages.

Elevation into prevailing winds (there is no shelter) and provision of good view may or may not have been relevant. The three "constant" factors do not seem to have been as emphatic as at SDM-W-26A, judging from contemporary setting. A major difference between the shell middens as a group and SDM-W 26A may well have been the former's situation near the former margin of San Dieguito-Del Mar lagoon and estuary, as well as the adjacent foreshore environments. It is suggested that the shell middens reflect, in part, subsistence-settlement adaptation to molluscs of the lagoonal-estuarian system. Since this must be seen as a relatively "in-constant" resource which experienced directional modification with siltation and sea level change, it should find expression in the shell midden sites. Expression should be in the form of subsistence-settlement pattern change, and this should show up most clearly in comparison with SDM-W-26A.

I suggest that initial occupation of NCW 7th. Neighborhood may have been at one or more of the shell midden sites. As the lagoonal-estuarian system continued to decay in economic viability and milling technology became more elaborate, SDM-W-26A would have become more attractive as an occupational center. For a time, the shell midden sites may have become special function outliers or satellites of SDM-W-26A, which was itself very likely a seasonal settlement. Ultimately, it may have become impossible to exploit lagoonal-estuarian fauna from the shell midden sites, so that marine-exploitation stations were moved nearer the coast and perhaps to it. With further decay in the viability of marine adaptation, a recognizably "La Jollan" pattern could indeed have moved south in the manner suggested by Smith and Moriarty (1982). Throughout this entire sequence, SDM-W-26A is seen to have remained essentially unchanged, save that eventually it, too, may have come less viable as a settlement and therefore been transformed into a minor occupation spot at which quarry production and collection-milling activities were still undertaken. The history of hearths at SDM-W-26A is clearly associated with vegetal processing, since only one was found to contain any shell, while hearths at the shell middens generally differ in form and their history is clearly associated with shellfish preparation.

The occurrence of very similar flaked-lithic and groundstone technologies at the shell midden sites and SDM-W-26A is thought to indicate both cultural conservatism and the "pull" of roughly equivalent resources. That tools and flakes are much rarer at the shell middens is seen as evidence of restricted importance in requisite activities at those sites, and may support the hypothesis of satellite status with respect to SDM-W-26A. The satellite hypothesis is given added weight by the extreme paucity of shellfish or any other faunal remains at SDM-W-26A. By the time that a recognizable Late Prehistoric pattern of ceramics, characteristic points, and cremations had developed, SDM-W-26A may itself have become a kind of special function satellite seasonal settlements situated at the

coast, in Carmel Valley to the south, or in the San Dieguito River Valley to the north. As such, SDM-W-26A would not have attracted cremation activity, nor much of the hunting behavior associated with points, and may have lost so much of its former importance as a vegetal resource exploitation station that ceramic vessels were not required. It is also possible that the resources exploited at SDM-W-26A in its terminal phase were not of a kind to require ceramic vessels, or that visits to the site were so infrequent that not enough sherds were deposited to have yet been encountered. I am setting up a case for biotic change in the SDM-W-26A vicinity sometime during the Late Prehistoric, and the presence of a conifer and some other fuel in the recent hearth may be compatible with this depiction.

The foregoing is but one interpretation that might be derived by applying the quite simple two postulate model to NCW 7th. Neighborhood sites, just as the model is only one of many alternatives. Several features of this different perspective, however, perform well enough in a Kuhnian sense that it deserves refinement and testing. For instance, the model provides a mechanism which explains the anomalies at NCW and links them to similar results obtained by others elsewhere in the region. It also encompasses regularities which are explained by the prevailing cultural history, although it does so by providing a different interpretive framework. Further, the new perspective supplies a different approach that predicts what is yet to be encountered archaeologically; thus, it is testable and capable of elaboration, refinement, and expansion. Finally, and perhaps most importantly, the model codifies many of the dissatisfactions, new interests, and non-culture-historical outlooks shared by the contemporary local archaeological community.

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## A NEW LOOK AT THE SAN DIEGUITO CULTURE

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*Abstract. Since 1975 archaeologists working in the Big Bend area of Texas and in Southern California have been discovering evidence which is causing them to re-examine the concept of the San Dieguito Culture. Some of the history of that process, and some of the evidence, together with some theses derived as a consequence are presented in summary form.*

A fortunate chance conversation made Dave Hanna and me aware that each of us, without the knowledge of the other, had been working along parallel or converging paths for some time. Without that, it had not seemed to me worth laying only one case before my fellow archaeologists until I had more than just surface evidence to report.

Until recently most, if not all, of us dealing with the culture pattern known as San Dieguito have thought that it was identified in part by a rarity of milling implements amounting to their virtual absence from the artifact assemblage we thought distinguished it. As Warren and True (1961:262) expressed it, "...seed grinding tools are extremely rare, if present at all." Indeed I, at least, had come to regard that absence as one of the recognition features distinguishing the San Dieguito pattern and contributed to orthodoxy by so presenting it to students. To put it another way, if milling implements were associated in a given assemblage, the pattern represented could not be classed as San Dieguito.

My faith in that construct received its first jolt when, in October of 1978, Julian Hayden showed Richard and Sue Carrico and me some artifacts from an area of Texas between the Pecos and Rio Grande Rivers. Because the terms "Trans-Pecos" and "West Texas" are not used by all to apply specifically to that region, the term "Big Bend" will be used to refer to that part of Texas south of New Mexico (Fig. 1) in this paper. The flaked implements were quite congruent with what we had come to think of as representing the San Dieguito II phase, except for one thing. That was the development of desert varnish on them--comparable to that on the Malapais specimens collected by the late Malcolm Rogers, now in the San Diego Museum of Man and pictured in Ancient Hunters of the Far West (1966:34). With them were two incomplete milling stones, muller and slab, and the development of desert varnish was as great on the fractured as on the unfractured surfaces and on the flaked tools. They were presented by Dr. A. A. Andretta, the finder, as representing an assemblage, a bona fide association.

With that stimulus Hayden and I visited Dr. Andretta in Alpine, Texas, three times between 1978 and 1980, visiting sites and examining stone artifacts. In 1979 Andretta had brought an extensive collection to San Diego for comparison with specimens here. At the end of that session, I had developed some hypotheses which, along with other ideas, were published in 1982 in Texas (Ezell 1982). Probably for that reason they have not received very wide circulation. Dave Hanna's new information has encouraged me to examine the situation further, so let me present some of these hypotheses to you here. They are:

- 1) the occurrence of San Dieguito sites and implements of all phases, including the Malapais, in the Big Bend far exceeds anything known elsewhere; crescentics, however, have not yet been reported for Texas;
- 2) milling implements, both mortar-and-pestle and muller-and-slab, are a part of the San Dieguito artifact assemblage in the Big Bend, at least;
- 3) another kind of milling implement, the gyratory crusher recognized by Hayden (1969:34:3:154-161) in Northwest Sonora and also reported for California was also even more common in the Big Bend;
- 4) the San Dieguito pattern had not been developed in Southern California-Southern Arizona after all, but had been imported from somewhere to the east-southeast of that region (wherever its ultimate source); and
- 5) the San Dieguito pattern was probably earlier in the Big Bend of Texas than in California.

Age estimates of the Texas specimens were provided by Hayden, drawing on his extensive experience with desert varnish on artifacts in sites on the Sierra Pinacate in Sonora (1976:41:3:274-289). How far the Texas dating can be applied to the Southern California situation remains to be examined, but that is not the problem I am interested in here. My current interest is in the validity of the hypothesis that, in Southern California, milling stones are after all a part of the San Dieguito artifact inventory. If they are, then the question of why, for so long, they were thought not to be, might be worth exploration.

In December, 1981, a crew was carrying out an archaeological survey in the northern part of the Camp Pendleton Marine Corps Base. On the first day of the survey, Steve Van Wormer reported to us a site (Fig. 1, SDi-9584) which he said had San Dieguito II elements and milling stones, so the entire crew was taken to examine it. Two points, perfectly

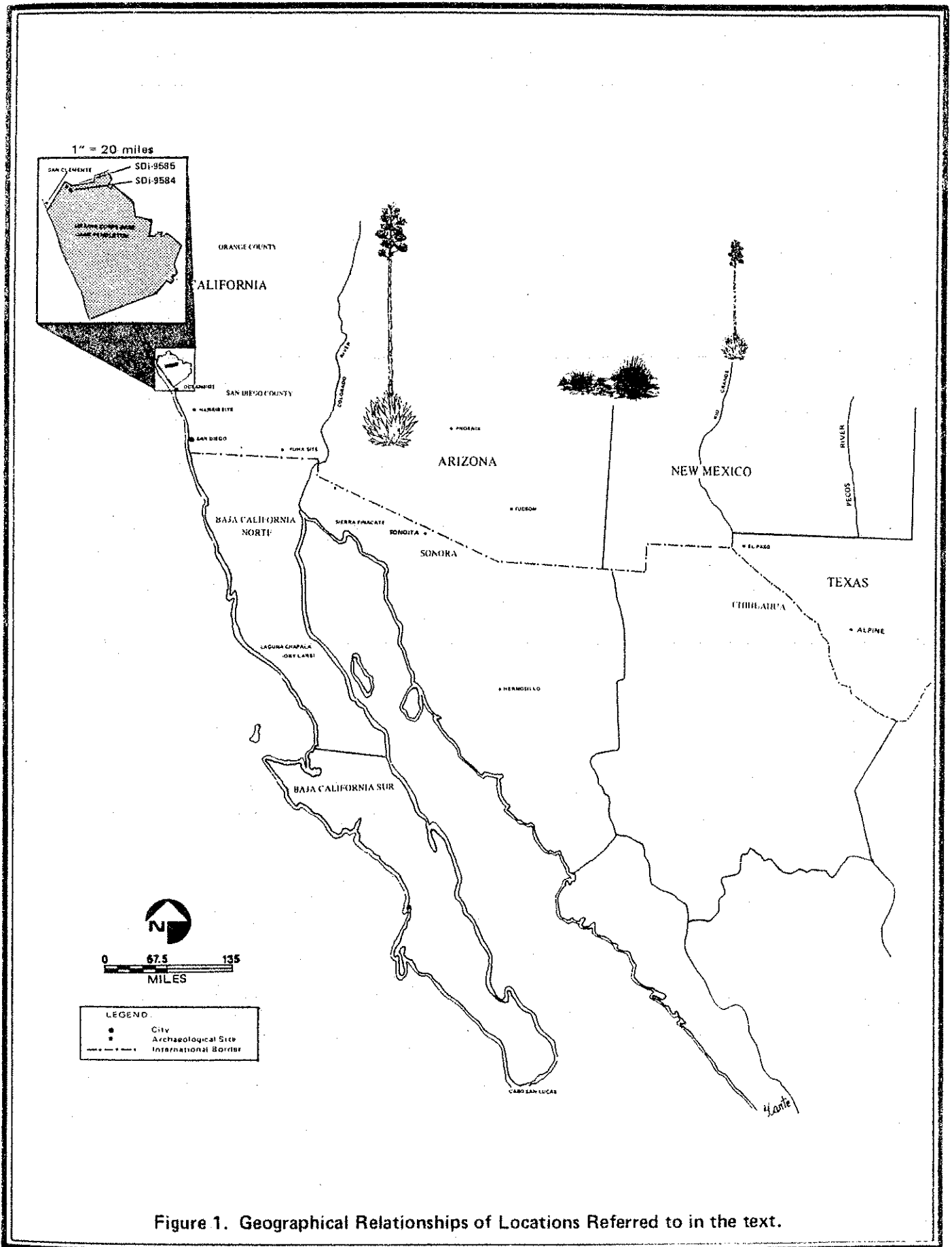


Figure 1. Geographical Relationships of Locations Referred to in the text.

compatible with San Dieguito III, were found; in addition, there were hammerstones, flakes, choppers, and scraper-planes which not only were compatible with San Dieguito III, they exhibited the degree of oxidation which we have come to associate with San Dieguito III stone artifacts in San Diego County. And, as Van Wormer had said, milling implements--muller and fragments of slabs--were also present in number. Of those tools, those which are of a material (such as granite) to be susceptible to it exhibit a degree of weathering far beyond what is found on milling implements of horizons later than the San Dieguito III.

Orthodoxy dies hard, perhaps more so in someone my age, so I was reluctant to accept the site as simply a San Dieguito site. Instead I muttered vague reactions about a "multicomponent" site in keeping with that orthodoxy. The next day, however, within a mile airline from the first site, Randy Franklin found yet another such site (Fig. 1, SDi-9585). No points were found, although flakes small and thin enough to denote pressure flaking were found. The same complement of other artifacts were found, however, and in addition a fragment of a stone bowl and two discoids were also found. By this time, my confidence in the traditional picture of San Dieguito technology was considerably shaken. As I considered more of the evidence and began to remember the Texas Big Bend, my faith became even weaker.

Some of that evidence is the setting of the sites. Both lie high in the hills, just below and on the southeast (the leeward) side of ridges, so they are sheltered from the wind. The first site, the Van Wormer Site, is surrounded by fossil springs. The second site, the Franklin Site, is adjacent to a still-flowing spring, with fossil springs close by. Not only was no pottery found on either site, no site bearing pottery was found in the entire area (over 6,000 acres) surveyed. Instead, four other sites exhibited the same characteristics, although to so much less a degree that they are only provisionally classed with these two at present. The artifact inventory of the two sites observed on the surface is such that they can be confidently classed as multi-use, i.e., habitation sites. At neither site was there any darkening of the soil to mark a midden, despite evidence of subsurface material that a midden exists. Finally, there is the evidence of artifact positioning that the sites had not been disturbed from the time they were last occupied by the makers and users of the tools until a few foxholes were dug in them during World War II, and had not been disturbed since. Those few foxholes, incidentally, enabled us to determine without further disturbance of the sites that there is some depth to them.

By now, the usual explanations such as curation and/or reoccupation by carriers of another culture pattern cannot, in my mind, satisfy the Law of Parsimony as well as the explanation that we have, in fact, evidence

that the San Dieguito processed seed foods as well as hunted. How, then, have some of us thought the contrary? How has it happened that such sites have not been found before?

The answers to those two questions will require more study and more time for the presentation than is available here, but I would suggest some possibilities. The idea that San Dieguito technology did not include milling tools appears to have arisen as much out of what was not said as of what was said, so much so that Wallace, in his "Suggested Chronology for Southern California Coastal Archaeology" (1955:11:3:214-230) stated it as uncertain whether the San Dieguito inventory included milling stones or not. Nevertheless, the late Malcolm Rogers in his pioneering studies in Southern California archaeology first distinguished two technological patterns in part on the basis of presence or absence of milling stones (1929). Even so, it remained more implied than stated for quite some time. An example is his comment about the San Dieguito-Playa Complex as he called it then: "The absence of the metate and mortar is almost conclusive proof that they were not seed gatherers" (Rogers 1939:17). By the time he had prepared the rough draft manuscript published posthumously as Ancient Hunters of the Far West, he had seen that concept unchallenged for so long that, as with the rest of us, it had become taken for granted. "In fact, a chief difference between the contrasted patterns lies in the absence of the metate in the San Dieguito I pattern and its presence in the La Jolla I pattern" Rogers (et al. 1966:39).

As to how Rogers came to that conclusion, one should keep in mind that the anthropological theory of the times was essentially borrowed from Europe. As it pertained to lithic technology, it was expressed in the terms Paleolithic and Neolithic as they signified cultural states. This is apparent in his comment about the "Yuman" [sic--La Jolla?] shell mounds of the Pacific coast: "This flake industry is rendered more amazing by the presence of the metate and mano, usually considered indicative of a Neolithic stage of civilization" (Rogers 1939:22). The view that the San Dieguito were without milling stones was given massive confirmation by the excavations at the C. W. Harris Site where none of us found milling stones, and the concept of that site as the "type site" for the San Dieguito.

And so I return to the beginning of this paper, in a sense. If one grants that Rogers developed a frame of reference in which it was "known" that the San Dieguito lacked milling implements, then I suggest that he classified sites according to that frame of reference. If, in fact, the San Dieguito prove to have had milling implements after all, that would call into question the multicomponent classification attached to some sites.

Lest any think I am exercising hindsight to asperse the work of a pioneer, remember that others beside Rogers did exactly the same thing. For a long time we could plead that we had no evidence for any alternative. Since 1974 I, at least, and probably others who worked on the project, have not had that excuse. That year Russ Kaldenberg found shell beads, as well as other culture elements (but no milling stones yet) long thought not to have been a part of the San Dieguito pattern, in the San Dieguito horizon at the Great Western Site in San Diego County (Kaldenberg and Ezell 342-344). To the publication (by the Imperial Valley College Museum) of his report on that excavation can now be added Dave Hanna's findings, and if I can get it done, more information about the Camp Pendleton sites, in the hope that others may be altered to the possibility of delineating more fully and accurately that troublesome construct called San Dieguito.



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WHATEVER HAPPENED TO THOSE LAKE CAHUILLA PEOPLE:  
TEST EXCAVATIONS AND IMPLICATIONS NEAR  
TABLE MOUNTAIN, CALIFORNIA

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The analysis of data recovered from a test unit near Diamond-Chain Rockshelter in the rancheria of Ha'a'weer has focused research in the Table Mountain Study Area to address the development of the Late Milling Period in the Peninsular Mountains south of the San Luis Rey River and west of the Lower Colorado River in southern California. Research in the past five years has involved a careful survey and inventory of six square miles surrounding Gray Mountain and Table Mountain in the northeast corner of Jacumba Valley, some 3,000 feet above the Lake Cahuilla basin to the east (May 1976, 1980).

Selecting Albert H. Schroeder's Hakataya Folk Tradition Model (1957, 1958, 1960) over Malcolm J. Rogers' Yuman Migration Model (1945), the term Hakataya will be utilized to discuss those ancient people who introduced pottery-making into the southern California region. Schroeder presented convincing arguments to avoid linguistic terms, much as other southwestern archaeologists have adopted the terms of Anasazi, Hohokam, and Mogollon (1957).

He has proposed in a 1978 article that the Hakataya covered an area ranging from west central Arizona, southeastern California, west of the existing Salton Sea to the Pacific Coast and south into the northern Baja California. The Hakataya are characterized as being a "rock-oriented" people. This refers to the presence of rock-lined houses, gravel and boulder alignments, rock-filled roasting pits, rock-pile trail shrines, rockshelters, and bedrock milling stones (Schroeder 1960). Perhaps the most distinguishing trait is the presence of paddle and anvil thinned plain pottery, which is totally unrelated to the coil and scrape thinned pottery of the Great Basin and greater southwest.

The Hakataya are believed to have originated in the southwest around the Gila River country and been present when either some unknown Mexican cultures or the Hohokam introduced Vahki Plain around 300 B.C. (Schroeder 1960). Schroeder (Personal Communication: 1980) tends to think that the Hohokam entered the area later and interacted with the Hakataya who were

making it at the time of contact. Whichever proves to be the case, both were in possession of the craft by A.D. 500. It is at that time in which paddle and anvil thinned pottery was deposited in a sand dune in the Imperial Valley (Moriarty 1966:27) and Schroeder hypothesizes that Hakataya people began living in the area prior to the filling of Lake Cahuilla around A.D. 950.

### The Research Problem

The Hakataya Model has been applied to the southern California area to ascertain the development of the Late Milling Period in relation to the spread of ceramic-making cultures throughout the greater region. The previous Yuman model encountered serious limitations in dealing with historical linguistic boundaries and did not account for prior expansions and contractions of cultural territories (Schroeder 1957). It is quite possible that early Hakataya had occupied areas now settled by Shoshonean speaking people or some other group.

The Jacumba region of the Peninsular Mountains of southern California is an appropriate location to research the development of the Hakataya because the valley opens to the desert through two major drainages. These are the Devil's Canyon on the east and Carrizo Gorge on the north. Just east of both drainages and at the base of the mountains lies the ancient shoreline of Lake Cahuilla. That body of water extended over 100 miles from the Coachella Valley south to El Centro. It remained filled from an overflow to the Lower Colorado River between A.D. 950 and A.D. 1500.

The Jacumba Valley has been proposed to be a cultural funnel (Rogers 1966, May 1980) in which all people passing from the desert to the coast would have gone through the valley. On the northeast edge of this valley lies two prominent mountains, Table and Gray Mountains. Secondary drainages flowing off these mountains connect with the two major drainages to the lower desert. It is up near these mountains and in the secondary drainages that several huge occupation areas were encountered in the Table Mountain Survey (May 1976, 1980).

Schroeder hypothesized (1960:198) an early Hakataya movement across the Sonora Desert to the Laguna Salada in Baja California and up the Peninsular Mountains just through the Jacumba Pass. Mergence of the Cultural Funnel Theory and the Hakataya diffusion hypothesis suggest that answers to the problem of Late Milling Period development in southern California might be found in the large occupation sites around Table Mountain.

Just when this Hakataya intrusion occurred is very much a part of the question. Rogers' hypothesis that the pottery-makers did not move into the area prior to A.D. 1450 (1945) has been disproven by a date of A.D. 990±60 (Mount Soledad Radiocarbon Laboratory, LJ-3296) at Cottonwood Creek (SDi-777) in association with the earliest pottery-bearing levels and a date of A.D. 760±100 (Berryman 1982:404) along the San Diego River west of the mountains (Keith Polan: Personal Communication). It appears that Tizon Brown Ware is associated with the Hakataya Intrusion, but the Lower Colorado River Buff Ware made a much later appearance (May 1975).

Not only is this early period poorly understood, but there is almost no information concerning sites prior to A.D. 750 in the mountain region. While Early milling Period sites abound in the form of huge shell middens along the coast, nothing is known for certain prior to the introduction of pottery in the region in question. In fact, there seems to be an hiatus in the archaeological record between B.C. 500 and A.D. 500. To date, no researcher has attempted to address the question of what was going on in the mountains during this era or how a fully developed assemblage of Tizon Brown Ware appeared on the San Diego River in A.D. 750 and some 250 years later at Cottonwood Creek.

A research design on the Jacumba Valley was presented to the Bureau of Land Management's Symposium on Desert Archaeology in 1979, at San Diego (May 1980). That design proposed the previously discussed Funnel Theory and argued for test excavations to address the development of the Late Milling Period. Of the 180 sites inventoried in the Table Mountain Study Area, several large occupation areas appear promising for resolution to this problem.

Integral in that research design was the question of how the settlement system developed following the earliest intrusion. Possibly associated with this problem is the presence of some of the largest sites in the region in the Jacumba Valley. One of the sites in the Table Mountain Study Area comprises approximately twelve acres. One hypothesis has been advanced that following the desiccation of Lake Cahuilla, vast populations shifted residence from the lake region to mountain drainages (Rogers 1945:191; O'Connell 1971: 180; Weide et. al., 1974:1). Also, Florence Shipek (Personal Communication: 1976) has proposed that domesticated agriculture existed in those times and that this facilitated support of huge populations.

Regarding the support of populations along Lake Cahuilla, the problem becomes less clear. Phil Wilke (Personal Communication: 1977) has found evidence of domesticated squash and small corn husks in middens associated with the occupation around Lake Cahuilla, but thus far no remains have been found in the mountains. An examination of the western shoreline of the lake reveals that immense inland marshes must have existed, but Weide (1974)

also notes that the level of the lake fluctuated so much due to evaporation and inconsistencies in the rate of fill that stable populations of marsh forage seems in question. Nonetheless, fish and animals would have been abundant.

In order to organize this research problem, four time phases will be proposed for researchers to test:

Hakataya Intrusion .....	A. D. 500 - 900
Lake Cahuilla Phase .....	A. D. 950 - 1500
Post Lake Cahuilla Phase .....	A. D. 1500 - 1800
Anglo Contact Phase .....	A. D. 1800 - present

Of all the questions associated with this research problem, the hypothetical relationship between the diffusion of Lake Cahuilla Hakataya into the mountain settlement system and the historic Kumeyaay is certainly one of the most intriguing. Kroeber has documented what he perceived to have been a northern and southern division between the Kumeyaay (in those days, Diegueño was the most preferred term). Since that time, linguists and museum specialists have supported that contention (Ken Hedges: Personal Communication).

The archaeological hypothesis is that the Kumeyaay are the living descendants of the Hakataya in this region and that the settlements which developed before and after the Lake Cahuilla episode are responsible for the north and south distinction. More specifically, the Hakataya Intrusion Phase is manifested with classic Hakataya cultural elements which existed prior to the appearance of the well-developed Hohokam around A.D. 500. Sites in southern California from that period would lack artifacts characteristic of the Hohokam-Hakataya interaction which occurred after that time. When Lake Cahuilla filled around A.D. 950, few people in the low desert migrated up to the mountains due to the abundance of resources available and low population competition. During the Lake Cahuilla Phase, those already living in the mountains had essentially 500 years to develop their own settlement system. However, when the lake desiccated and the populations from that area had to move up with their mountain relations, the two groups were considerably different. Post-Lake Cahuilla Phase people from the lake area would have introduced dialectical differences, different traditions in pottery-making, different arrow point styles, and other distinctions which had developed down in the desert.

The archaeological program for testing the latter hypothesis must involve both test excavation and analysis and examination of museum collections. Care must be taken to establish a list of index traits which Kroeber

and others have identified as distinguishing the north and south Kumeyaay. This must be compared with the archaeological collections. Radiocarbon dates, temporally sensitive artifacts, and other chronology data must be made available to test the date of artifact appearance. It is anticipated that such elements as desert side-notched points, serrated points, obsidian, cryptocrystallines, figurines, and rock art elements will prove to increase in quantity in the Lake Cahuilla Phase.

### Native American Involvement

One line of investigation prior to field work, involved interaction with Kumeyaay people. It was also desired that one of the living Kumeyaay elders might recall the name of the sites north of Gray Mountain. A copy of the research design was given to Mrs. Rosalie Robertson of Campo Reservation, who in turn, discussed it with her family. On September 22, 1979, Mrs. Robertson, her family, and Ben and Linda Stoval of Boulevard joined the test investigation team prior to implementation of the test.

The Stovals joined the survey teams to learn and assist in site recordation. The surveyors filled out forms, made sketches, took plant specimens, and learned how to document without recovering artifacts. Mrs. Robertson and her family visited and inspected the locus known as Diamond Chain Rockshelter. It is one small area within the twelve acre site known as SDi-4296. Mr. Romaldo La Chappa, who is over 90 years old, recalled that the site was called Ha'a'weer by his people and that he visited the area to hunt Desert Bighorn Sheep. Also in attendance were Mrs. Robertson's brother, Tony Pinto, and a friend, Mrs. Fern Southcott.

It remains the wishes of the Kumeyaay people that archaeological research be conducted in non-ceremonial areas and only when it can tell something valuable about their culture. Mrs. Robertson and her family had read over the research design and agreed that the questions were of valid interest. However, there was certain jovial skepticism concerning prehistoric agriculture. It was agreed that all discoveries would be shared with these people and that they would be afforded an opportunity to examine the recovered collection. Ultimately, that collection will be housed in the Archaeology Museum at Palomar College.

### The Test Excavation

The test unit was located just west of the main opening of Diamond-Chain Rockshelter in the hopes that living activities would be detected. The test unit was 1.5 meters square and each level was excavated in ten centimeter increments. All loosened soil was screened in 1/8" mesh and

all recoveries were bagged. Analysis was conducted by Richard Norwood. Charcoal recovered from the 30-39 cm. level was submitted to the Mount Soledad Radiocarbon Laboratory. The shallowness of the midden precluded a pollen study. A small cluster of rocks in the southwest corner of the unit were left intact, as there was no reason to disturb the feature. Prior to backfilling, metal objects were placed in the corners to mark our disturbance and seeds scattered throughout the unit to ensure against future erosion. Buckwheat seeds were broadcasted over the unit and several plants replanted to discourage potential relic-hunters and to return the area to a natural condition.

Table 1 illustrates that the test unit at Diamond-Chain Rockshelter was rich in artifacts but very shallow. The excavation attained an average depth of 30 cm., although a few pockets were down to 39 cm. This suggests an intense but short occupation.

While the vicinity is rich in flakes and pottery sherds, the surface of the test unit contained only 13 artifacts. This suggests that colluvium from the nearby decomposing granite bedrock has had time to buildup over the occupation level. The surface artifacts floated-in on the sediments. Most of the other surface artifacts in the locus area have, in fact, been exposed by sheet erosion and small rivulets cutting down into the older surfaces.

It appears that the occupation layer at Diamond-Chain Rockshelter is between the 10-20 cm. levels. This interpretation is based upon the presence of heavy artifacts such as scrapers, choppers, hammerstones, manos, and metates which are concentrated in that layer. The only projectile points are also there. This was where the cluster of rocks was also found, presumably an old hearth. All the lighter artifacts could have floated in with the rain and sediments.

Dr. Timothy Linick of Mount Soledad Radiocarbon Laboratory reported a date of A. D. 1570±60 (LJ-5094) for the charcoal recovered in the 30-39 cm. level. This would place the occupation of Diamond-Chain Rockshelter just after the desiccation of Lake Cahuilla. Significantly, five of the eleven rock types for flaked stone are exotic and found down by the southwest shoreline of Lake Cahuilla. These imports include petrified wood, pink chert, banded chert, and a particularly fine grade opaque chalcedony.

The artifact assemblage from Diamond-Chain Rockshelter suggests a complete range of activities associated with a homesite, rather than a temporary camp. The heavy wear on the chopper and scrapers suggests something of a length of stay, but the shallowness of the deposit does not indicate that the Hakataya lived there too long.

TABLE 1

<u>Provenience</u>	<u>flake</u>	<u>debitage</u>	<u>core</u>	<u>tool- flake</u>	<u>scraper</u>	<u>chopper</u>	<u>hammer</u>	<u>tool</u>	<u>point</u>	<u>mano</u>	<u>metate</u>	<u>ceramic</u>	<u>micro- flake</u>
surface	4	4	0	0	1	0	0	0	0	0	0	4	0
0 -10cm	33	60	0	1	0	0	0	0	0	0	0	13	2
10-20cm	66	126	3	3	1	1	1	0	4	1	1	28	27
20-30cm	41	68	1	3	0	0	1	0	4	1	0	7	9
Totals	144	258	4	7	2	1	2	0	8	2	1	52	38



### An Interpretation

Given the research problem and the data from Diamond-Chain Rockshelter, a group of Hakataya people apparently moved from the southwest vicinity of Lake Cahuilla following the desiccation. This move occurred some time between A.D. 1510 and A.D. 1630, perhaps A.D. 1600. They brought with them cores and tools from sources near their old homeland and used them while they lived around Diamond-Chain Rockshelter. All about the drainage in which they settled, there were many other displaced families establishing temporary homesites. Adjustment to the mountain group, perhaps through political or distant clan lines may have been slowed by this time due to the immensity of the population influx into the area in that period of time.

### Re-Addressing the Research Design

At this stage of the research, it seems appropriate to re-address the research program. The hypotheses advanced in this paper, both based on scientific data and fantasy, need to be tested systematically. The location of future test units must take into consideration the kinds of activities which are represented by the artifact and feature clusters. More tests are needed in midden loci, where time depth and index artifact associations can be fixed. Perhaps aerial photographs ought to be employed in the mapping to pinpoint spatial relationships. Possibly each homesite could be dated and compared with the others to ascertain, if indeed, refugees from the drying Lake Cahuilla basin did camp at Ha'a'weer for a generation or so until a place could be found for them elsewhere. The question of the pre-historic Post Lake Cahuilla Phase effects upon the northern and southern Kumeyaay also must include museum research and interviews with linguists. Certainly, the work has only begun.

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STRATIFIED PREHISTORIC SITES OF CISMONTANE

SAN DIEGO COUNTY: THE NULL SET

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Abstract. After a rigorous investigation of several prehistoric cultural sites in cismontane San Diego, the authors have concluded that stratification is not only lacking, but was never a characteristic of the sites.

Based on the information obtained from these sites, it is suggested that rather than being exceptions, they represent the rule in terms of site formational history. It has become apparent that the subsurface component of these, as well as the majority of sites in this portion of the County, is the direct result of biopedoturbation, most notably due to the activity of the California Ground Squirrel.

Soils are not static and inert, as many imagine, but are subject to a variety of physical, biological and chemical processes that mix, heave, and stir, and radically alter their morphology. Francis Hole in 1960 (2) reviewed these processes and proposed a terminology that has been generally accepted for their classification. His terminology has been modified by Wood and Johnson (4) in a paper specifically concerned with their influence on archaeological sites.

Pedoturbation Processes\*

<u>Process</u>	<u>Soil Mixing Vectors</u>
Faunalturbation	Animals, especially burrowing forms
Floralturbation	Plants, root growth, treefall
Cryoturbation	Freezing and thawing
Gravitturbation	Mass wasting, solifluction, creep
Argilliturbation	Swelling and shrinking of clays
Aeroturbation	Air, wind
Aquaturbation	Water
Crystallurbation	Growth and wasting of soils
Seisitturbation	Earthquakes

\* Modified from Hole

While many of these processes are important in some San Diego County soils, mixing by burrowing animals has been most important at archaeological sites in the western part of the County. The most effective of these burrowing animals is the California Ground Squirrel, Citellus beecheyi, which burrows more intensively and to a greater depth than other burrowing animals in this County. These rodents confine their burrowing activity to specifically defined areas, and therefore affect the soils in which they live to a much greater extent than other species.

Their activity is often confused with that of the pocket gopher, Thomomys bottae, a very antisocial animal whose burrows are widely and more or less indiscriminately dispersed. Their burrows are much shallower than those of Citellus, and rarely exceed 20 cm. in depth. The latter commonly burrow to the full depth of the soil profile, bringing up material from the substratum and mixing this material with the overlying soil.

They seem to have an uncanny ability to detect previously disturbed soils, and so confine their burrows to soils previously disturbed by their own kind. They also burrow in recently deposited, well drained alluvium, and in soil materials disturbed by man. They therefore commonly burrow in recently emplaced road fills and earthen dams, where their burrows constitute a serious hazard to the safety of such structures. Some pedologists consider such mixing of stratified alluvium to be a necessary precursor to the initiation of pedogenetic processes in such alluvium.

We have identified two major groups of soils in Southern California that are influenced by ground squirrel burrowing (1). The first group are moderately fine textured, granular, calcareous soils that were formerly called Rendzinas. These soils are now classified as fine-loamy, mixed, thermix, Calcic Pachic Haploxerolls (3). Representative of these in San Diego County are the Linne clay loam soils, which occur on gently sloping uplands in the coastal plain in the western part of the County. These soils are developed in soft, light colored marl or calcareous sandstone. They have gray, granular, strongly effervescent clay loam surface soils, and light gray, granular to moderate medium angular blocky clay loam subsoils. Their substratum consists of white, firm, violently effervescent very fine sandy loam, which is penetrated by numerous crotovinas and animal burrows. These may extend to a depth of 1.5 meters or more, and are filled with dark colored materials resembling the surface soil. Crotovinas are difficult to distinguish in the surface and subsoil unless the soil is very dry, when they can be identified by their slightly darker color and more distinct granular structure.

The second group consists of moderately coarse-textured, weakly granular, non-calcareous soils that were formerly called Non-Calcic Brown soils. These are now classified as coarse-loamy, mixed, thermic Typic Xerochrepts (3).

Typical of these are the Vista coarse sandy loam soil, developed on ridge crests and steep north or east facing slopes in the foothills of the Peninsular Range. The surface soil is a dark grayish brown, weakly granular, neutral to slightly acid coarse sandy loam. The subsoil is a yellowish brown, massive, slightly acid coarse sandy loam. Crotovinas filled with dark colored, granular material resembling the surface soil are common throughout the surface and subsoil, and may tend into the underlying weathered rock to a depth of 1.5 meters or more. These soils are developed in deeply weathered, coarse-grained granitic rock.

These features common to soils in both these groups are:

- Weak to moderate granular or crumb structure
- Low chroma
- Irregular to broken A-C or B-C boundaries
- Numerous crotovinas and partially filled animal burrows
- Absence of argillic horizons.

On the basis of estimates of ground squirrel populations, and their rate of burrowing, we have estimated that they mix the entire mass of the soils we have described in about 360 years. This estimate is probably low, since they move a considerable amount of soil within older burrows without bringing it to the surface. It is difficult to estimate the volume of soil involved in such internal soil movement, but it may easily be as great or greater than that which is brought to the surface.

The density of ground squirrel populations may also be much greater in some locations during seasons of abundant food supply. It is difficult to estimate such populations that might have existed before the changes in the environment initiated by the introduction of domestic livestock and modern agriculture in San Diego County.

Since 1979, with the preceding information as a reference framework, we have had the opportunity to investigate several archaeological sites within San Diego County. Many of these had been previously studied by other researchers. Our initial appreciation of such sites was therefore conditioned by the understanding we gained from our reading of site forms and research reports written by our enlightened colleagues. Two words which are frequently found in these documents are midden and intact. Taken together, these words mean that a component of the site is an undisturbed cultural stratigraphy which has been preserved in the subsurface. Based upon our investigations, these two descriptors have been found to be the most abused words in the local archaeological literature, and wholly indicative of a flaw in the local archaeological paradigm. That flaw being a total misunderstanding of how subsurface components are generated which results in an accurate interpretation of cultural behavior at the site.

We would like to briefly introduce three sites which we feel are illustrative of this problem, and then discuss the ramifications of our work.

Site #1. Meadowbrook: SDM-W-915

This site was located in the La Costa area of Carlsbad on the crest of a small ridge, at an elevation of 550 feet above Mean Sea Level. The site was believed to be an expression of pre-pottery horizon gatherers and hunters and previous researchers had obtained a radiocarbon date on shell of 6880 ± 280 B.P. which placed it within the local La Jolla cultural complex.

The site was described by these researchers as follows:

SDM-W-915 is a midden locality covering approximately 4,000 square meters. Midden soil varies in color from medium brown to gray and is seen to contain relatively substantial amounts of decayed organic compounds in the area exposed by the grading cut. A midden depth of approximately 50 centimeters is clearly exposed in the center of the cut, with a gradual thinning to the east and west.

In the section of their report entitled "Research Potentials," they state,

Another factor making SDM-W-915 a prime candidate for useful research is the apparent integrity of the remaining midden. While surface impacts have certainly obviated the utility of micromapping, they do not appear to have significantly altered subsurface portions of the midden.

Our work revealed that the site soils could be classified as Linne Clay loam and Huerhuero loam. These soils are formed from interbedded, fine-grained Tertiary marl and sandstone of marine origin. The Linne soils are formed from the calcareous members of this assemblage. They have dark gray, granular clay loam surface soils, and slightly finer-textured clay loam subsoils, characterized by numerous crotovinas and ground squirrel burrows filled with material resembling the surface soil. The Huerhuero soil series have brown, massive fine sandy loam or loam surface soils, and strongly contrasting angular blocky clay subsoils.

Artifacts from W-915 were concentrated in the Linne soil, extending from the surface to bedrock, and were almost entirely absent from the Huerhuero soil. Where the disturbance of the Linne soil was greater (deepest), so also was the depth of the artifacts; this included the crotovinas which penetrated the bedrock.

Site #2. Oak Creek: SDi-1057

This site is located in the northern portion of Escondido, California, on and adjacent to a small knoll at an elevation of approximately 760 feet above Mean Sea Level. The site is believed to be an expression of Late Milling phase gatherers and hunters, and has a radiocarbon date of  $370 \pm 70$  years B.P. The site was described by previous researchers as containing extensive and largely intact subsurface deposits of prehistoric artifacts and cultural debris. Specifically, that two regions of the site remain substantially intact and contain significant, abundant and diverse prehistoric materials warranting further mitigatory research: (a) Locus A contains an intact subsurface component with buried milling features; (b) Locus B contains a stratified midden accumulation.

Our findings were that no region of the site was found substantially intact or particularly significant, and that the site contained no midden, stratified or otherwise. The stratification observed in Locus B was the result of post-1928 land modification. Historic disruptions and bioturbation had created the artifact-enriched soil conditions. These soils are typical of the thermic Typic Xerochrepts mentioned earlier.

Site #3. Western Salt: SDM-W-192A

This site is located in the northeastern portion of the City of Imperial Beach along the south shore of San Diego Bay at an elevation of approximately 20 feet above Mean Sea Level. The site is believed to be primarily an expression of pre-ceramic gatherers and hunters. Four radiocarbon dates on shell were obtained for the site; these range from  $6095 \pm 415$  B.P. to  $1150 \pm 60$  B.P. Previous researchers have stated that the deposits within the Western Salt property are of relatively great importance due to the fact they are largely undisturbed. Earlier researchers proclaimed that their excavations of approximately 160 square feet out of a possible universe of approximately 8200 square feet was a sample which comprised a greater proportion of the relatively undisturbed portions of the site.

Our findings were that artifacts had been incorporated into the Marina soils of the site by a combination of ants, small insects, earthworms, gophers, California ground squirrels and discing. No evidence of undisturbed cultural deposits were found.

In conclusion, it appears that archaeological researchers in San Diego County should obtain a fuller understanding of the processes which condition soil formation before making interpretations of the recovered archaeological assemblage. The working paradigms utilized by the majority of San Diego County archaeologists, that have soils accumulating by the superposition of layers, and that call all perceived assemblage patterns, cultural, should be greatly revised if not totally discarded.



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## HUNTER-GATHERER SEDENTARY ACTIVITIES

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This paper is more of an introduction to a research program than a "results and recommendations for the future" type of paper, since the project is in progress. The objective of the research is to describe and explain the types of activities conducted by sedentary groups within villages. For this paper, a sedentary settlement is defined as a permanent occupation of a site throughout the year. While taking sedentism somewhat for granted at the outset of the program, ongoing analysis of the components of the research should critically examine the concept during the course of the project.

Site SDM-W-2812A/SDi-9476 was recorded by the author early in 1982. The site is located between Jamul and Dulzura in south central San Diego County. Based on the presence of ceramics, it was classified as a late prehistoric site. As part of the environmental impact studies accomplished in the project area, surface and subsurface investigations were conducted at the site.

The purpose of the posthole excavation was to obtain preliminary information on the preservation, depth, and horizontal extent of the site. The sterile layer at the site is reddish clayey loam, in contrast to the dark brown silty midden deposit. Both the cultural and sterile deposits contain alluvial gravels.

The posthole excavation, although limited in nature, provided evidence for horizontal activity areas at the site. The northeastern area appears to be a locus for lithic tool processing and the southwestern area contains a large amount of shell and ceramics.

Several unusual aspects of the site were observed as a result of the posthole tests. A projectile point midsection, made of black fine-grained metavolcanic material, was found. The lower broken end of the point had been refinished for hafting. In addition, the soil from the posthole tests yielded an abundance of Mytilus sp. (mussel) shell. The presence of both Mytilus sp. and Protothaca staminea (clam) is unusual, since it represents exploitation of two diverse ecological zones, the sandy bay and offshore rocks.

Subsequent to the survey tests, a systematic posthole excavation, surface collection, and test unit excavation program was conducted at the site. Thirty-six postholes were excavated in a modified radial pattern to determine the extent of the midden. The soil from these postholes was screened

through one-eighth-inch mesh. Based on the surface artifact distribution and the posthole test results, a grid of 42 20-by-20-meter squares was placed over the site. Artifacts within this grid were collected and their locations were plotted on maps (Figure 1). Cores and tools were individually provenienced. Flakes, ceramics, and shell were not labeled but their locations were individually plotted. Based on the results of the surface collection and the posthole tests, two 2-by-2-meter units were excavated in the deepest part of the site (see Figure 1). Soil from these units was screened through one-eighth-inch mesh.

A wide range of material was collected from these tests. Most notable are the number of projectile points, flakes, and cores recovered. Since two quarry outcrops are located near the site, it is reasonable to assume that the porphyritic and fine-grained metavolcanics recovered from the site were quarried at these areas. The large number of cores and flakes indicates that raw material reduction was occurring. However, the number of finished tools is not proportional to the amount of raw material being processed. It can be hypothesized that raw materials were worked into blanks or finished tools and traded out of the area. Such analysis was beyond the scope of this initial testing, but the lithic artifacts from excavation units will be analyzed for level of reduction.

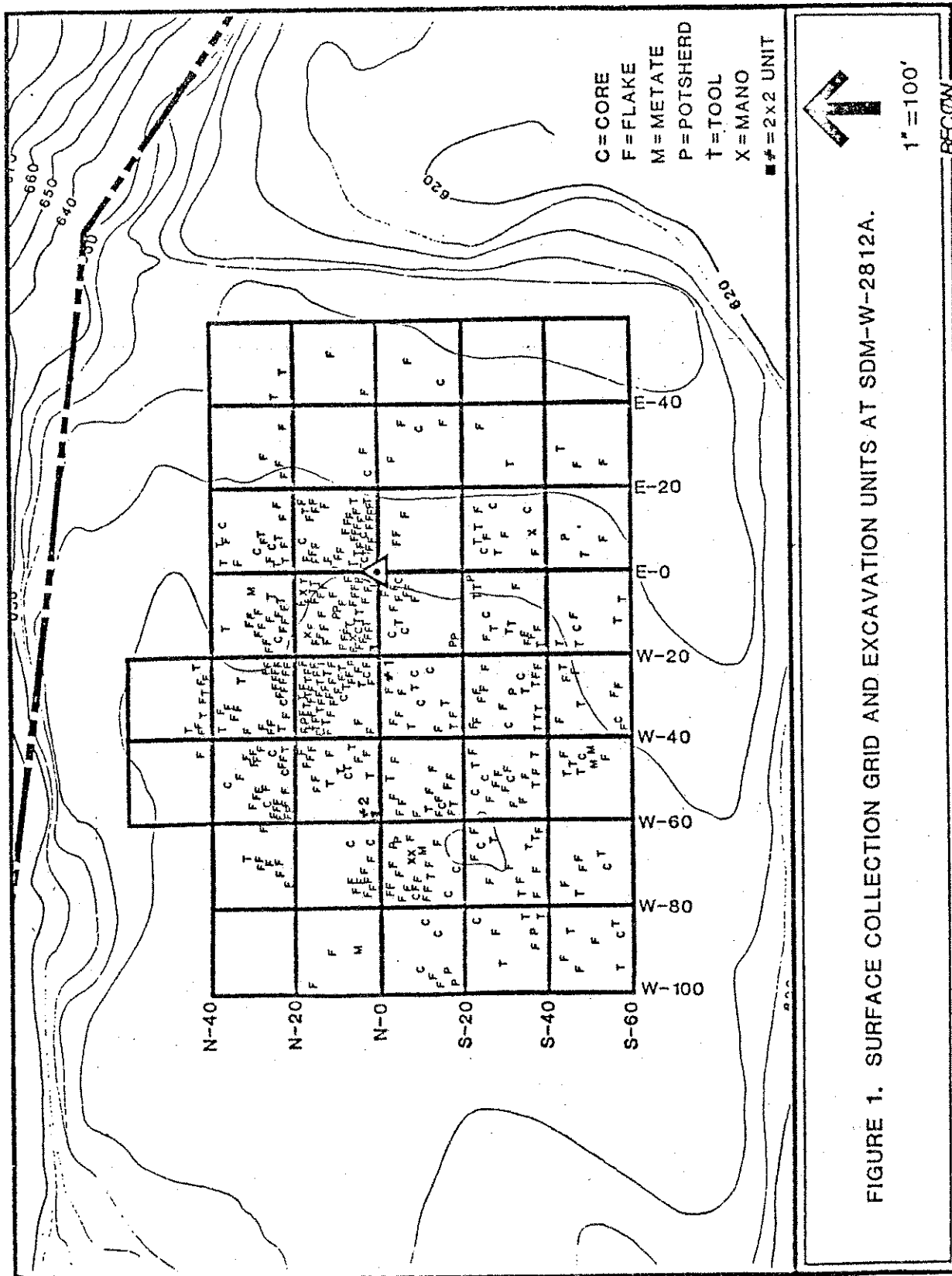
Eleven of the 21 projectile points that were found had diagnostic features. True's (1966) typology was used to classify these. Five of the 11 classifiable points were True's (1966) Type 1 and 2, common late prehistoric types found in San Diego County. Four others were variants of the Desert Side-Notched type common in the Great Basin. The other two have not been explicitly described in San Diego County and are convex-based.

The knives recovered from the site are nondiagnostic but conform in general to True's (1966) descriptions. Knives were differentiated from projectile points on the basis of gross size.

Unifaces are defined as tools with edge damage (crushing, rounding, abrasion) on one face. Most these fall into the descriptive category "scraper" but some are what have been called "utilized flakes."

Bifaces, of which relatively few were found, are artifacts with edge damage on two sides, with the exception of knives and projectile points (segregated on the basis of size).

Several artifacts with battering and crushing damage only were found. These tools were typically used to produce percussion-flaked stone tools. Since so few were found, it can be suggested that "hammerstones" may have actually been made from antler or wood.



Cores compose the most abundant artifact category and are a by-product of stone tool manufacture. Most of the cores were porphyritic metavolcanic. In contrast, most of the flakes were fine-grained metavolcanic. This was probably because the fine-grained metavolcanic material was used to produce pressure-flaked artifacts resulting in flakes that are small and numerous. The porphyritic material produces larger, relatively fewer flakes per artifact and core. Most of the flakes lacked cortex, which would indicate that finished or much-reduced stone tool manufacture was occurring at the site.

In comparison to the number of cores and flakes recovered, relatively few finished tools were found. This suggests that tools or reduced blanks were manufactured at the site for export to other areas. Since the porphyritic metavolcanic and fine-grained metavolcanic stone types were available in the immediate vicinity, abundant raw material for such trade would be available.

Although only a small amount of shell was found, species present included Argopecten aequisulcatus (pecten), Donax gouldii (bean clam), Chione sp., Haliotis sp. (abalone) and Septifer bifurcatus. These types represent both sandy and rocky shore habitats and indicate exploitation of a diverse resource base by the site inhabitants.

Of the great amount of ceramic sherds recovered, only five were identified as Lower Colorado River Buff Ware. The remainder were Tizon Brown Ware. A high proportion of the rim sherds were from jars. This would indicate that large-scale storage was occurring at the site, necessary for year-round occupation. A fragment from a funnel-shaped pipe (True's [1966] Type 2) was found during posthole testing. Part of the stem and bowl, representing the midsection of the pipe, were recovered. An incised ceramic rim was also found. The pattern was a cross-hatch incision diagonal to the rim line.

Six metate fragments were found at the site, representing five metates. All were basin types made from granitic, coarse-grained rock. Since there are no boulder outcrops along the creek near the site, it would be expected that portable rather than bedrock milling stones were used at the site. Site occupants probably had milling stations in other areas to procure specific off-site resources. Manos found at the site were shouldered, and most were bifacial. One mano had faceting on the ends. The manos were made from fine-grained granitic stone and quartzite (originating as stream cobbles). A total of ten manos were found during the field tests.

A large amount of bone was found during test unit excavation. Much of this represents rabbit and small mammal, but deer-sized pieces were also found. Bone preservation was excellent. No bone tools were identified.

The presence of a great amount of bone, stone tools, and ceramics indicated that a population may have lived at the site throughout the year and intensively exploited the surrounding environment for stone tool material and plant and animal foods.

The site boundaries are shown in Figure 2. Surface and subsurface test results indicated that specialized activity areas existed within the site as a whole. As shown in the surface collection map (See Figure 1), the lithic tools are concentrated in the eastern portion of the site. The deep midden, with the majority of bone, shell, and ceramics, is concentrated in the western part of the site. This type of horizontal patterning is expected in a large village where many activities are occurring in different areas, but is rarely identified in prehistoric sites in southern California.

Based on the results of the tests described above, a doctoral dissertation proposal was developed for research on the activity areas, seasonality, and trade. A major part of this research project will be a reconstruction of the annual round of the site's occupants using environmental and cultural data. An environmental analysis will provide a list of available plant and animal species and the times of year during which they were abundant. This concept was pioneered by Flannery (1968) for early Mesoamerican prehistory, combining ethnobotanical information and plant community reconstructions to make a seasonal schedule.

Cultural material, such as animal bone, shell, and floral materials, from column samples can be used to reconstruct a seasonal schedule. For example, the presence of immature rabbit bone would indicate a springtime occupation. The culmination of all this seasonality data would be a determination of the time, or times, of occupation at the site.

Another interesting aspect to this project will be to evaluate the existence of activity areas within the site. There are two kinds of internal site structure for villages or camps. One kind, recorded and discussed by Steward (1938) for Shoshone groups in the Great Basin, consists of a duplication of all maintenance and subsistence activities by each family group. This structure occurs because the family groups live apart for the most of the year. When they come together, no special, group-wide exclusive activity areas are established.

The other kind of structure is that recorded by Yellen (1977) for the Bushmen, where specific activities occur in particular areas. Binford's (1978, 1980) ethnoarchaeological studies of Eskimo hunting camps have also provided information on this type of site structure.

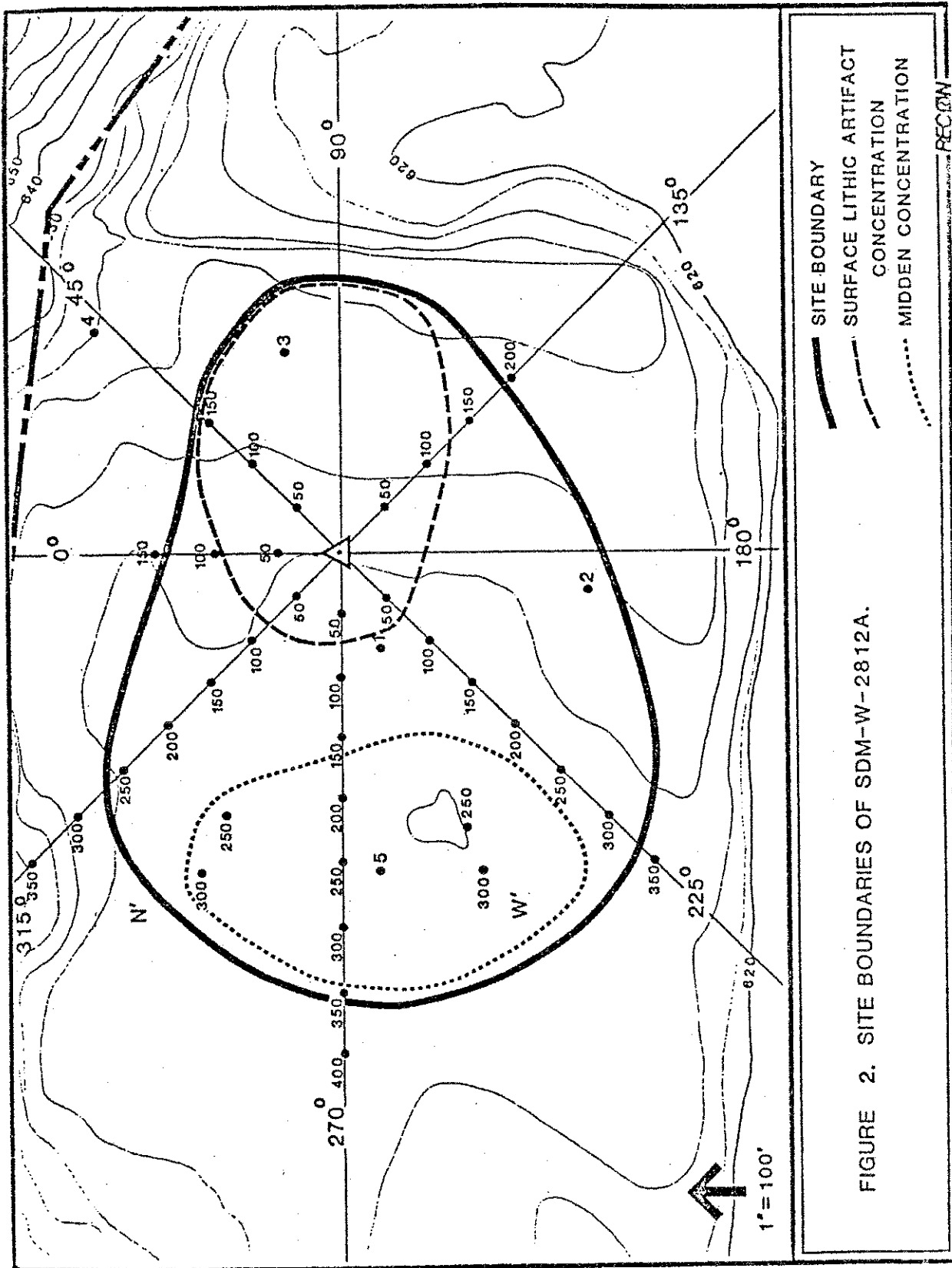


FIGURE 2. SITE BOUNDARIES OF SDM-W-2812A.

To determine which of these kinds of settlement occurred, the site was divided into two strata, based on the surface collection and posthole tests. One stratum appeared to contain primarily ceramics, animal bone, and shell, while the other stratum had abundant stone tools and flakes. Each stratum is 40 x 30 meters in size, and they are contiguous. Four 2-x-2-meter units were selected systematically for each stratum, with the widest possible horizontal distribution.

The analysis stage of the research will compare artifacts and stone tool attributes between the two areas. Comparisons will be made both horizontally and vertically, so analysis can control for both space and, theoretically, time.

If the site conforms to the Yellen (1977) model, stone tool production, represented by the presence of all stages of lithic reduction from raw material through complete tools, will be present in just one stratum. And, if one area was used primarily for food production, there should be a higher frequency of ceramics, shell, and animal in that area. If lithic reduction, ceramics, shell and animal bone are present in both strata, Steward's model will be supported.

These comparisons will be made by levels as well as between the two "activity" strata. The site is next to what was a perennial creek. In the unit excavated nearest the creek, thin, sterile alluvial layers were found, with midden above and below. In addition, the site does not extend all the way to the creek. The tentative explanation for these observations is that the site had to be abandoned at least once due to flooding, the creek having changed its course from the directions of the site towards its present location. Therefore, any differences that could be found between pre- and post-alluvium layers would indicate changes through time; perhaps earlier occupations of the site were not year-round.

Another topic that will be addressed during the course of the project is trade. Exotic materials, such as chert, obsidian, chalcedony, and jasper, have been found at the site. Expanding on Ericson's (1977) study, obsidian hydration analyses and sourcing will be done to see how far obsidian was traded during the late prehistoric period.

As part of the description of trading activities that took place at the site, a detailed ceramic analysis will be done. Both brownware and buffware have been found at the site. By comparing the relative frequencies of each, we should be able to determine what quantities of buffware are present. Also, since it is unlikely that large quantities of complete vessels were traded into the area, production of some buffware ceramics may have been local from imported clays; this will be addressed by examining rim decorations and forms.



The goal of these analyses, currently in progress, will be to describe the kinds of activities that occurred at villages. Byproducts will include information about sedentary behavior, use of the environment, and trade.

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THE IMPORTANCE OF BONES AND FLAKES  
IN ARCHAEOLOGICAL ANALYSES

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This article attempts to demonstrate the importance of collecting and saving all classes of archaeological remains excavated from a site for CRM purposes, in particular, debitage and faunal remains.

Problem oriented archaeology, developed during the 1960s, sought to refocus archeologists' attentions away from laundry lists of artifacts and ecofacts recovered, to answering questions related specifically to the behavioral aspects of the people who occupied the site. The methodology used should be the scientific method of hypothesis testing and model development. Approaching a site excavation with a particular or series of particular problems in mind theoretically allows you to concentrate on the recovery of those materials important to your problem(s). When conducting such an excavation for research purposes, the site in question will theoretically still be there if other archaeologists want to come back and test your procedures or collect data relevant to another research question. This is almost never the case in CRM oriented archaeology. While all excavation projects should be focused toward specific research goals, the site excavated for data retrieval (mitigation) will likely be destroyed and any remaining data lost forever. For this reason, all classes of artifacts and ecofacts must be saved for posterity, so they are available for present and future analysis and interpretation. This discussion does not include those artifacts and human remains which retain special cultural importance to living Native Americans, where it is the State's policy to return such items to the appropriate local representatives for reburial. This discussion focuses on two classes of materials common to most archaeological excavations: faunal remains and debitage (ro flakes).

Faunal Analysis

Faunal remains are the bone remnants of the animals consumed for food by the site inhabitants. Bones are generally preserved in all but the most acidic open-air sites. All classes of vertebrates have osseous remains durable enough to be preserved in archaeological contexts, including the bones from fishes, amphibians, reptiles, birds and mammals. Many of the bone fragments can be extremely small. For this reason, no excavation should propose to use screen mesh sizes larger than 1/8 inch, unless

special provisions are made; i.e., taking column samples, or screening a percentage of the units with 1/8 inch mesh. Column samples are especially appropriate for those sites which have a high density of faunal remains. Column samples are valuable for the information they contain on small animal bones, carbonaceous seeds, pollen, shellfish, etc. While this discussion focuses on faunal remains, one should remember that all classes of ecofacts are equally important for the information they provide about prehistoric subsistence patterns.

It has been estimated that the local hunter-gatherer diet consisted of 5-60% game, fish and other maritime animals (Bean 1978; White 1963), 20-25% probably being the average amount. Since the remainder of the diet was represented by various forms of plant seeds, greens, bulbs, roots and fruits, which are generally not preserved in the open-air archaeological deposit, faunal remains represent one of the most easily obtained and identifiable classes of information regarding the prehistoric subsistence system.

The professional literature is replete with discussions of the significance of faunal remains (cf., Casteel 1976; Chaplin 1965, 1971; Cornwall 1956; Daly 1969; Gilmore 1949; Olsen 1971; Reed 1963; Ryder 1968; Ziegler 1965, 1972); while the impetus in the literature today is the technical and methodological aspects of faunal analysis (cf., Lyman 1979, in particular pp. 171-228). The faunal analyst must do more than simply prepare a laundry list of the species present within a site. Any study should commence with the compilation of a list of species which would have been available for Native American exploitation within the territory surrounding the site. This exploitation territory has come to be called the "site catchment" (Vita-Finzi and Higgs 1970; Jarman 1972; Flannery 1976; Hastorf 1978; Findlow and Ericson 1980). From this data base the researcher progresses with a rigorous and systematic analysis of the faunal remains to ascertain:

- (1) which species were exploited;
- (2) where and how the animals were butchered;
- (3) how the bones were subsequently treated after meat removal;
- (4) trade networks or long-range settlement systems; and
- (5) the season of the year the site was occupied.

Once a list of utilized animals is compiled, this is compared with the catchment list of the site to determine:

- (6) which sectors of the environment were being exploited and conversely, which were not;
- (7) if the Native Americans were dependent on a specific food source;
- (8) the dietary preferences of the site inhabitants; and
- (9) the hunting techniques utilized.

By examining the above data in conjunction with ethnographic and ethnohistoric documentation, the faunal analyst can factor out:

- (10) changes in the subsistence base over time;
- (11) changes in the hunting patterns and procurement technologies;
- (12) changes in dietary habits, and why those changes occurred; and
- (13) which bone elements of each species could be predicted to occur within the site deposit.

Ideally, analysis should proceed with an examination of the tool assemblage from the site. The knowledge of which tools were actually used by the site inhabitants can greatly enhance the analyst's insight into procurement strategies. The zoologist could surely identify the faunal remains, but only a trained anthropologist/archaeologist is able to extract from the fauna the cultural inferences necessary for the reconstruction of aboriginal economic systems.

#### Lithic Debitage Analysis

Debitage, as used in this context, is the waste by-products of prehistoric stone tool manufacturing and use. The debitage or flakes were generally not the intended final product of tool manufacturing in the mind of the prehistoric flintknapper. But flakes were used as tools when their form or size was ideally suited to the tasks the user had in mind.

While lithic studies have been going on for more than a century (cf., Johnson 1978), it is only within the last decade that flakes have received any appreciable attention by archaeologists (Camilli 1981; Collins 1975; Feder 1981; Jackson 1981; Jelinek, Bradley and Huckell 1971; Matson 1981; Muto 1971; Phagan 1976; Rondeau 1982; Speth 1972). This is certainly odd since flakes are invariably the largest class of artifacts recovered from any archaeological excavation, with the possible exception of pot sherds in archaeological sites of the American Southwest.

There are two approaches to lithic analysis. The "functional" approach strives to place formal type names to a lithic artifact, stating that something was a scraper, chopper, knife, burin, etc. The "descriptive" approach attempts to class lithic objects by the degree of modification they have received without placing an arbitrary classification on them. In this latter approach, an artifact might be labeled unifacially modified, bifacially modified, pecked and/or ground, with additional descriptive data provided. Both approaches rely heavily on ethnographic analogy to provide crosscultural comparative data on tool function or use, but it is the descriptive approach which places the greatest emphasis on the debitage as an integral part of the entire site's lithic assemblage, to the almost total exclusion of the subject by the functional lithic analyst. If the functional analyst assumes he/she knows what an object was used for, there is no need to know how that object came to be. The descriptive analyst views the functional approach as ethnocentric, since it superimposes our own cultural values over that of the prehistoric. This is in effect stating that since a prehistoric tool may look like a steak knife, then why not call that tool a knife. One cannot assume similar uses because of similar forms when comparing dissimilar societies, and comparing artifacts (steak knife and prehistoric tool) which may have been used thousands of years apart. The descriptive approach uses replicative experimentation and statistical analysis to assist in the recreation of how tools came to be and, therefore, looks at the actual behavioral decisions being made by the tool worker. A whole array of technical machinery exists to help the analyst determine the types of surfaces tools were being worked on and the motions a tool was being put through. The debitage is of paramount importance in the recreation of prehistoric technological systems through this descriptive approach.

Analysis of the debitage can provide data on many aspects of prehistoric behavior, technological production, trade, change through time, chronology, subsistence and other activities. The potential exists to develop regional chronologies based on the size, quantity, material type and attributes of debitage. This would enable the temporal placement of sites in the absence of diagnostic artifacts. Significant shifts in attributal analysis might indicate cultural changes related to major population movements in San Diego County (e.g., La Jolla to Yuman).

The flake analysis can provide data on Native American lithic technology. Studies can be designed to analyze spatial artifact distributions, sourcing, trade, techniques of flint knapping, control of the flint knappers, types of tools being made or readied for final manufacture, changes in lithic technology over time, and intrasite activity areas. These are significant aspects of prehistoric human behavior which have been overlooked by many archaeological studies.

The techniques employed in a report should ideally be applicable to all studies of prehistoric Native American archaeological assemblages. Information on settlement patterns can be gleaned from such a study even when aspects of the lithic technology (i.e., final tools) are missing. For example:

...in a complex village it might be observed that all residents reduplicated the entire manufacturing process, and therefore, that the specialization was not practiced in the production of stone tools. Or through time, changes in patterns of exploiting material sources may reflect responses to changes in the physical environment. Some assemblages may include evidence of activities which are dependent upon earlier manufacturing steps that are not evidenced. The interpretive framework allows identification of these missing activities and leads to the inference that they must have been accomplished outside of the area sampled by the assemblage at hand (Collins 1975:16).

This interpretive framework, as developed by Collins (1975) and Bradley (1975), assumes that flint knapping was not a haphazard art, but rather was a well disciplined technological process. The stages involved include: (1) the acquisition of the raw materials; (2) core preparation and initial reduction; (3) (optional) primary trimming; (4) (optional) secondary trimming and shaping; (5) (optional) maintenance/modification; and (6) deposition into the archaeological context. "Each of these steps is composed of one or more activity sets and each activity set results in a product group of chipped stone artifacts. An activity set may include one or more specific activities and each product group (except the first) consists of two kinds of materials--waste by-products and objects destined for further reduction or for use" (Collins 1975:17). Product groups can be technologically described and inferences made regarding the activities employed to produce the particular manufacturing process. "The waste, debitage, is particularly amenable to this technological analysis" because those objects intended "for use rather than for discard or further reduction...are subject to relocation with use" (Collins 1975:17, 19). "Debitage remains at the locus of manufacture" and "in evaluating the spatial relationships between manufacturing activities" it is "the more reliable source of data" (Collins 1975:19).

Other researchers also have recognized the value of debitage for reconstruction of aboriginal manufacturing processes and the inadequacy of the implements by themselves to reveal the complete history of the manufacture of the final tool form. Flakes have been used to distinguish chronologically different but similar tool assemblages. It should be recognized that flakes in some cases "are the best artifacts from which to discover prehistoric behavior patterns" (Phagan 1976:35). Binford (1972:249) has stated that archaeologists need to develop "a set of expectations as to relevance" so

that significant selection "can be made from the infinity of characteristics potentially present in the body of empirical material being studied." Debitage studies can be designed to provide such a "set of explanations" of lithic fracture control within which specific flake attributes can be seen to have specific behavioral relevance in production technology.

In conclusion, no aspect of problem oriented archaeology can be successful without the proper analytical framework. The above discussion was presented for this reason and to show how the development of a good research design must begin with the proper descriptive approaches.



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CERAMIC ANALYSIS IN RESEARCH DESIGNS FOR  
THE PREHISTORY OF SOUTHERN CALIFORNIA

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In the prehistoric archaeology of Southern California, ceramic evidence has played only a modest role, at least in comparison with its importance in the nearby Southwest and in several other archaeological regions. This paper will attempt to review and assess that role, looking at what sorts of use have been made of ceramic evidence in research designs, with what success this use has been met, and in what ways the potential value of the evidence might be more fully realized.

Southern California archaeology had its first florescence in the 1870's, when museum expeditions from outside of the area came and made extensive collections, particularly from the large sites of the Santa Barbara Channel area (Warren 1973). These expeditions in turn stimulated the interest of local amateurs, commercial collectors, and eventually local museums (D. B. Rogers 1929). For prehistoric archaeology in this early period, the research design, if it can properly be called that, was relatively simple: archaeology was used to illustrate ethnography, with little serious concern about chronology or cultural processes. The prime center of this activity was the Chumash area, where prehistoric ceramics were lacking. However, one of the more conscientious of the 1870's collectors, Paul Schumacher, did publish a short ethnographic account of Cahuilla pottery making techniques (Schumacher 1880), which had archaeological implications. Toward the later end of this collecting-ethnographic period, and illustrating many of the strengths as well as the weaknesses of archaeology done in this mode, was George G. Heye's (1919) description and discussion of ceramic finds from the Diegueno area. Primarily archaeological, Heye's article also mixed in relevant ethnographic information and used the two sources of data to cross-check and supplement one another. Nonetheless, the result lacked historical depth and did little to elucidate the cultural processes implicated by the ceramic remains.

The name of Alfred L. Kroeber of the University of California is rightly associated with a florescence and professionalization of anthropology in California. His ethnographic work in the state had a strongly culture-historical bent, and his early seriation of Zuni potsherds (1916) was a landmark in the use of archaeological ceramic evidence to reconstruct culture history. Nonetheless, his influence on Southern California archaeology was ambiguous. Perhaps his commitment to salvage ethnography caused

him to downplay the potential role of archaeology, which might compete for funds and personnel. Acknowledging that the time depth of North American prehistory might prove to be great, he nevertheless generally perceived or anticipated little discernible evidence of cultural change through time in the archaeological record of California. Ceramic technology in the southern portion of the state was a minor exception to the general pattern of uniformity and simplicity of material remains. The areal distribution of pottery included two distinct regions in California, according to Kroeber's information (1925): the San Joaquin Valley and the southern Sierra Nevada, where the Yokuts, Tubatulabal, and western Mono practiced the craft in a crude form, and Southern California from the Colorado River to the San Diego coast, excluding the Chumash and Gabrielino areas and the desert north of the San Bernardino Mountains. Ceramics in the southern of the two regions had clear affinities with pottery traditions in the Southwest, and Kroeber inferred that the northern tradition probably received its stimulus ultimately from the same source (1925:538). Chronologically, Kroeber suggested on the basis of very limited evidence from the San Diego coast that the introduction of ceramic technology into the area was "not altogether recent" but dated from "no very remote period" (1925:702). The vagueness and occasional inaccuracy of Kroeber's conclusions on ceramics are easily accounted for by the poor state of development of archaeological information on Southern California when he wrote. More importantly, he had at least implicitly recognized the potential for ceramic evidence to distinguish relative chronological periods and cultural movements in Southern California.

In the 1920's and early 1930's, Southern California prehistoric archaeology experienced a paradigm shift, from collecting artifacts for ethnographic illustration to the definition of time periods and the reconstruction of movements by peoples and ideas. Because of its geographical distance and the Kroeberian priority given to salvage ethnography, academic anthropology as represented by the University of California did not take the lead in this shift, which instead was led by three local museums: the Santa Barbara Museum of Natural History, the San Diego Museum of Man, and the Southwest Museum in Los Angeles. Of the pioneers in Southern California culture-historical archaeology, David Banks Rogers (1929) worked on the Santa Barbara coast and the channel islands, where ceramics were lacking, and his contributions need not be considered further here. The same consideration applies to Ronald Olson (1930), who made an anomalous foray from Berkeley into the periodization of Chumash-area prehistory. The Southwest Museum's contribution is seen in the work of Elizabeth W. Crozier Campbell, William H. Campbell, and Mark R. Harrington. The Campbells' early work (1931) contained considerable information on ceramics in the Twentynine Palms area, but it was done in the older tradition, collecting, describing and preserving artifacts but not inferring prehistory from the ceramics of the area. When the Campbells and Harrington later made seminal contributions to culture-historical archaeology in the Mojave Desert and adjoining Great Basin, their focus was on discerning very early occupations, to which ceramic analysis was not particularly germane.

The key culture-historical pioneer from the perspective of ceramic studies was Malcolm J. Rogers, who worked in the San Diego region, the Southern California deserts, and adjoining areas. His delineation of the periods and character of Southern California prehistory remains, for better or worse, highly influential in current perceptions (Hanna 1982). An advocate of a short but finely-subdivided chronology, Rogers made considerable use of ceramic evidence in various short publications mapping out his view of prehistoric culture change. Two published accounts provide the bulk of Rogers' contribution on ceramics (1936, 1945). The first, "Yuman Pottery Making" (1936), was ethnographic rather than archaeological, but it was ethnography done by an archaeologist, making the sorts of observations about the ceramic techniques of the various Yuman-speaking groups which an archaeologist would wish to know for comparative purposes, and therefore the work may properly be termed ethnoarchaeological. Anna O. Shepard, in her classic summation of Ceramics for the Archaeologist (1956), cited Rogers' work several times as embodying some of the ethnographic observations invaluable to archaeologists in interpreting their evidence. Rogers' other key publication, "An Outline of Yuman Prehistory" (1945), worked with archaeological evidence, among which pottery played the most prominent role. Intrusive ceramics from the Southwest provided a framework for dating the chronological phases which Rogers discerned. Moreover, vessel types, forms, and design elements of locally-produced ceramics were used to distinguish successive stages in the local pottery industry, primarily with reference to the lower Colorado River area but also by implication for the deserts and mountains of southernmost California. Rogers' conclusions were innovative and provocative, and they were manifestly based on a substantial body of field experience and careful observation. However, in this as in other aspects of Rogers' published work, the level of documentation was entirely insufficient to sustain the conclusions he proposed. That the local ceramic traits have the sequential and chronological significance which Rogers suggested remains an unsubstantiated possibility. Regrettably, more rigorous replication of many of Rogers' observations may no longer be possible, and much of Rogers' unpublished data has been lost.

By the early 1940's, California archaeology was becoming more respectable within the University of California, and researchers, notably Robert F. Heizer and Adan E. Treganza, were making contributions to Southern California prehistory. Treganza (1942), discussing surface observations in an area including portions of San Diego and Imperial Counties and northern Baja California, conservatively recognized pottery as a chronologically diagnostic element separating early and late prehistory, as well as distinguishing Mountain wares and Desert wares differing both in the character of their clay sources and in their characteristic forms.

Initiative in rigorous classification of Southern California ceramics, however, came to a large degree from outside the state, in Arizona. Harold S. Colton (1939), in particular, developed ceramic ware and type categories for

northern and western Arizona which were applicable as well to Southern California. Revised by Euler and Dobyns (1958) for Tizon Brown Ware and by Schroeder (1958) for Lower Colorado Buff Ware, this classification provided relatively detailed descriptions of the diagnostic and nondiagnostic characteristics of the pottery groups, as well as information of their geographical and chronological distributions. Although not arranged in key form, the classification offered some hope of being a basis for the repeatability needed to test and amend the proposed distributions.

From a more specifically Southern California perspective, Meighan (1959), taking information from the Molpa site on Palomar Mountain and from sites in the Anza-Borrego desert, described in detail a new pottery type, Palomar Brown. He discussed the contrasts between this Southern California pottery and the coil-scraped Owens Valley Brown Ware but gave little attention to the problems of relationships with western Arizona ceramics. Euler (1959) appended comments to Meighan's report, including the Southern California material within Tizon Brown Ware but cautioning against application of the more detailed western Arizona type structure to this material.

The 1960's, highly productive and expansive in other aspects of Southern California archaeology, saw relatively little attention given to ceramic analysis. Contrasting attitudes toward the subject may be illustrated by two papers dealing with the eastern Mojave Desert. Meister *et. al.* (1966) classified 233 sherds from surface collections in the New York and Providence Mountains. Most of these sherds were labelled as Palomar Brown, with Tizon Brown used as a contrasting type (as well as a ware). Seemingly, the distinctions were based primarily on temper. Geographical sources for the pottery or for its makers were inferred in areas distant by 100 miles or more from the sites, on the basis of previously reported distributions of the ceramic types. In general, the paper was a rather naive application of existing typologies to the new data rather than a testing or amplification of those typologies or their interpretations. On the other hand, Donnan (1964), also working in the Providence Mountains, flatly rejected the previous efforts at ceramic analysis and interpretation.

All attempts to make a satisfactory analysis on the basis of current information derived, for the most part, from the sherds themselves--paste differences, rim types, surface treatments and manufacturing methods--are of little avail. The pottery recovered from Southcott Cave...shows the fallacies of the typologies which are made on the basis of sherds. Since the pottery recovered was all from within the confines of a small cave, it is possible to partially reconstruct several vessels. Time and again, sherds which each fit a description of a different pottery type would reconstruct into a single vessel (Donnan 1964:12).

Notably, Donnan's treatment of this important problem was impressionistic; he did not specify the particular type which overlapped or the specific traits which were irregularly present in the sherds from a single vessel. Consequently, Donnan's rejection of the (unspecified) typology must be taken on authority; no assistance in refining any classificatory scheme was provided.

In the 1970's, contributions were made in several aspects of ceramic analysis. Distributions of non-vessel ceramic forms were explored. Received ideas about the chronological and geographical range of prehistoric pottery making in Southern California were challenged. The problem of defining adequate pottery typologies was again raised.

Non-vessel ceramics, such as pipes and figurines, have been among the better-reported artifacts throughout the history of investigations, no doubt because of their relevance to intuitive notions about prehistoric aesthetics and idea-systems. They have also been used occasionally as chronological or cultural-boundary indicators. A discussion by Hedges (1973) of figurine types, their iconic interpretations and functions, and their presence in two geographically-distinct traditions within Southern California, is especially notable for taking the subject away from a purely descriptive framework and into an interpretive one relevant to larger cultural-historical research designs.

Relatively minor extensions of the chronological and geographical ranges of Southern California ceramics were proposed by May (1976) and by Koerper *et. al.* (1978). May presented substantial if not totally conclusive evidence for an early ceramic threshold of about 1000 B.P. in San Diego County. Possibly more notable in this context was the continuing failure of other, substantially earlier, ceramic dates to reach print, where they could be critically evaluated. The common propagation of such claims by word-of-mouth and their slowness to achieve formal presentation suggested some reluctance to give such a particularistic issue as the chronology of pottery diffusion a high research priority. Koerper *et. al.* (1978) presented an argument for the presence of locally-made prehistoric plain pottery in the Newport Bay area of Orange County, within the Gabrielino linguistic area, contrary to some earlier claims of a more southerly coastal limit to the spread of this craft. More notable, perhaps, than this modest geographical claim were the methods of technical analysis used to support it, including neutron activation analysis of sherds and suspected clay sources, and thin section study of sherds. Unfortunately, the article was marred by inadequate presentation of the basic data obtained, so that it was not possible to evaluate critically the validity of the conclusions, nor could the technical results presented be easily extended to other Southern California pottery.



Three extended discussions of Southern California ceramics have been recently published, by May (1978), Van Camp (1979), and Waters (1982a, 1982b, 1982c). Van Camp's contribution is most easily dealt with. Kumeyaay Pottery contained some general ethnographic background information on the Kumeyaay, brief descriptions of "typical" archaeological sites belonging to the ceramic period, a synthesis of information on Kumeyaay ceramic technology drawn largely from Rogers (1936) and Shepard (1956), and some comments on typology. The inadequacies of Van Camp's presentation of archaeological data have been well pointed out in a review by True (1980). On the issue of pottery types, although Van Camp usefully presented some of Rogers' previously unpublished notes in an appendix, she found little merit in the received ceramic categories. Instead, Van Camp proposed to rechristen Tizon Brown Ware and Lower Colorado Buff Ware respectively as Southern California Brown Ware and Southern California Buff Ware. Within these wares, she recognized types on the basis of surface decoration and on the basis of occurrence within ethnographically-attested geographical boundaries, giving as types Kumeyaay Brown Plain, Kumeyaay Brown Incised, Kumeyaay Brown Painted, Northern Diegueño Brown Plain, Northern Diegueño Brown Incised, Luiseño Brown Plain, Luiseño Brown Incised, and so forth. The sterility of such a typology, based almost exclusively on area of occurrence and contributing almost no observational data toward elucidation of culture-historical issues, is patent.

May (1978) presented a synthesis of his own and Malcolm J. Rogers' observations in a ceramic typology which was substantially more stimulating than Van Camp's discussion but was still problematical. May raised, without resolving, the question of whether the clays and temper used in particular ceramic types reflect geographical variations in the resources available or variations in cultural choices. In any case, the differential distribution of these types geographically and stratigraphically would be suggestive of prehistoric patterns in population displacement, seasonal transhumance, and exchange. If it is accepted that the questions which May raised were significant and appropriate, it may nonetheless be uncertain that his analytical apparatus was adequate to address them. The Rogers-May structure of wares-series-types lacked any clear key. Individuals attempting to apply it to particular sherds have reported the results to be ambiguous and unsatisfying. Repeatability of the classification does not seem to be adequately guaranteed; by focusing on one particular trait or cluster of traits rather than another, it seems likely that different classifiers would produce different results. Generalizations about the ranges and dates of particular types and series, although expressed by May in a properly cautious manner, were inadequately documented. Unfortunately, because of its authoritative tone and the problems in its application, May's typology may have served more to discourage than to stimulate careful evaluation of the important issues it raised.

Waters (1982a, 1982b, 1982c) developed and put to use a typology for the buffware pottery of southeastern California and adjacent areas. Like May, Waters used as a starting point the unpublished notes and collections of Rogers. Waters included a useful (if partisan) discussion of the history of ceramic studies in this region. Among the most valuable features of his chapters were detailed information on the geographical distributions of the ceramic types (including proposed distinctions between the ranges of archaeological occurrence and the centers of manufacture), some quantification of the frequencies of different types within particular sites, and a fairly detailed discussion of the evidence for generalizations about chronology. Unfortunately, although the detail of the evidence presented was substantial by the region's usual standards, it was probably insufficient to sustain most or all of the conclusions which were proposed. Much of the difficulty lay in the typology. Waters' typology stressed distinctions of vessel shape, surface treatment, and rim shape, but a variety of other traits were also noted as diagnostic or important. Like May's typology, that of Waters was not rigorously keyed, and it would not be possible to infer what characteristics were shared in common by sherds grouped together in Waters' type counts, or what traits consistently contrasted in his distinctions. Also as with May's typology, problems have been reported with consistency or repeatability in the application of the typology. Because the chronological and geographical generalizations about Southern California ceramics were closely tied to the typology, the weaknesses of the latter undermined the credibility of the former. Moreover, some of Waters' conclusions seem to have been in conflict with the data presented. It is not unlikely that many of Waters' hypotheses will be found to be substantially correct, but because of the way the data were handled and reported, it will be difficult to build directly on his work to test or modify them. If they are not taken or rejected on authority alone, much of the fundamental observational and analytical work will probably have to be repeated in a more rigorous, confirmable manner.

The uses which have been made of ceramic analysis for Southern California prehistory have been reviewed, with some comment on the value and validity of those uses. From these considerations, it is possible to project some general observations, both on the sorts of research values which may be expected to be available and on the research techniques required to realize those values.

Relative and absolute chronology is one of the more obvious and better-realized uses of ceramics in Southern California prehistoric archaeology. In a region for which so few well-established, temporally-diagnostic features are available, the rough dating of sites or levels as pre-ceramic or ceramic is an important, standard concern. Because sherds tend to be relatively abundant, are well-preserved, are unlikely to be totally collected by pot-hunters, and are quite likely to occur at almost any major site of a ceramics-using society, they are an excellent diagnostic item. The

uncertainties about the precise chronology of the spread of ceramic technology from the Lower Colorado River area to various portions of Southern California are readily addressable. The main problem seems to be to achieve some concensus among archaeologists working in this area that the question is one of interest and that prompt reporting of relevant information in a form in which it can be critically evaluated is essential. Negative information--the absence of ceramics from well-dated deposits--needs to be reported and evaluated as well as positive claims. Use of thermoluminescence dating may become important if the factors involved in local variations under the method become better understood and controlled and if costs are not prohibitive. The claims by Drover of far earlier ceramics in at least portions of coastal Southern California merit close testing. If the claims are confirmed, they will provide a substantial stimulus for closer examination of the Millingstone and Intermediate horizons in this region, in light of the general anthropological implications of so early an adoption of ceramics and its subsequent failure to spread or sustain itself.

Stylistic studies of Southern California ceramics have been an important element in the past but may be decreasingly so in the future. The rarity and simplicity of decoration on most pottery vessels in this area have always been major limitations on ceramic analysis. Proposals for using vessel form were advanced by Rogers. Unfortunately, the supply of whole vessels with good provenience information is probably nearing exhaustion for much of the Southern California area. Reconstruction of vessel forms from sherds, when surface collection or excavation is sufficiently intensive, is possible but laborious and a source of only a fairly limited amount of vessel form information. Stylistic consideration of non-vessel ceramic items such as figurines and pipes is a continuing, productive concern, but the numbers of such items tend to be so small that rigorous comparative analyses and conclusions are often not possible. The cultural interpretation of stylistic variation is under the best of circumstances a complex and problematic issue which a thin data base may render nearly unmanageable. Nonetheless, where the evidence of ceramic style is available, appropriate questions of chronology, cultural tradition, aesthetic ideals, and site activities may be at least tentatively considered.

Exchange patterns are an important objective for ceramic analysis. Rogers (1929), Ruby and Blackburn (1965), and others have occasionally considered the intrusive pottery found at Southern California sites but originating outside that region and its implications for long-distance travel and trade. Finds of Southwestern pottery have generally been reported in a sporadic manner, however, without adequate synthesis, making it difficult to determine, for example, which pottery types, in which proportional frequencies, have been found in various parts of Southern California. The nature of Southwestern-Southern California interaction, its chronology,

and its extent may be better estimated when the ceramic evidence is more fully exploited. Intraregional exchange patterns are also addressable. Frequencies of Tizon Brown Ware and Lower Colorado Buff Ware in sites in various areas are one measure of this. If more detailed typologies, attribute analysis, or clay sourcing become effective, it should be possible to sketch the regional pattern in much more detail.

Manufacturing techniques used to produce archaeological ceramic specimens have been little studied in Southern California, beyond the basics of paddle-and-anvil smoothing, degree of oxidation in firing, and the techniques for surface treatment. Experimental determinations of firing temperatures should be possible. Surveys and excavations should also encounter kilns or firing areas. If, as Rogers (1936:5) reports for the Kumeyaay, firing was done in an area separate from the habitation site, it should be evidenced archaeologically by a pit, charcoal, and sherds from pieces lost in firing, in a locality relatively sheltered from the wind.

The nature and degree of site disturbance and the depositional and postdepositional processes involved are topics apparently rarely addressed by means of ceramic studies but are ones well-suited to such an approach. For resource management goals as well as for interpreting the research values of an archaeological site, it is generally important to establish to what extent the deposit retains depositional stratification; to a lesser degree, the integrity of horizontal distributions are also important. By intensively excavating a given portion of a ceramics-bearing site, it should often be possible to recover substantial percentages of the sherds from individual vessels and to match these sherds to reconstruct the vessels. The horizontal and vertical dispersion of the matched sherds provide minimum measurements of the disturbance of remains produced by depositional and postdepositional processes. This is an argument in favor of intensive, block sampling rather than reliance on widely-dispersed excavation units, even during exploratory, testing phases of work. It is also another argument against precipitate breaking of sherd edges to examine paste and temper characteristics, at the expense of being able to match the original breaks.

The question of typology, of sherd classification, and of standardized attribute recording, is central to the future of ceramic analysis in Southern California prehistory. A successful typology could be used, according to various claims which have been advanced, to throw important light on chronology, on the movements of peoples, on processes of diffusion, on exchange systems, on the seasonal transhumance of groups, on patterns of clay and temper resource exploitation, and even on the force and functioning of supernatural taboos (cf. May 1978:26 on the use of tourmaline as a tempering material). On the other hand, a successful typology evidently must have two qualities: it must map by its variations one or more cultural variable of significant archaeological interest, and it must be repeatably and fairly

economically applicable to the sherds being recovered from Southern California sites. As suggested above, the typologies available from Southern California ceramics have realized the indicated goals only to a very limited extent, apparently because of defects in those necessary qualities, although even those defects have yet to be rigorously demonstrated.

Several approaches to typology are possible. These include polythetic type definition, type samples, monothetic type definition, and key attribute recording. Several of these have been applied to Southern California ceramics, singly or in combination. Each has strengths and weaknesses.

Polythetic type definition seems to be the essence of the most commonly used approach, exemplified by the works of Schroeder, of Euler and Dobyms, of May, and of Waters. Such a polythetic approach has the two virtues of traditional sanction and of current theoretical fashionability (cf. Williams *et al.* 1973). A polythetic definition of a class may be taken as one based on several enumerated criteria, no single one or group of which is both necessary and sufficient to establish membership in the class, but which in some specified combinations are sufficient. Unfortunately, polythetic definitions, like other kinds of definitions, can be explicit and precise or vague and intuitive, and the latter is characteristic of the ceramic type definitions in question. The criteria are such matters of construction technique, color, rim shape, temper material type, temper size and angularity, presence or absence of carbon streak, wall thickness, fracture, and even geographical distribution. Rather than imposing a logically consistent polythetic definition which seems to have cultural significance, the authors have, apparently, taken the types as naturally-existent, discovered entities and have described the range of attributes which seem to characterize those entities. As a result, the typologies are rather unwieldy and do not seem to be susceptible to reasonably objective, consistent application. Rigorous polythetic definitions do not seem to have been attempted.

The method of type samples is closely related to nonrigorous polythetic definition and has been advanced by the same authors, using type sites in the case of Schroeder and of Euler and Dobyms, type specimen collections in the case of May, and both in the case of Waters. The type samples method has also had limited non-ceramic use in the archaeology of this region (cf. Warren 1966). The approach perhaps came to archaeology from stratigraphic geology, where its success was considerable. By defining fairly arbitrary type sections of sedimentary rock and studying their paleontological characteristics, geologists were able to get good control of relative chronology long before absolute radiometric methods were available; even now, radiometric methods are often not applicable, and when they are, the relative chronology based on type section is often able to discriminate finer slices of time than the absolute chronology. The type method has worked in geology because

fossil-bearing stata are "information-rich," containing complex patterns of varying faunal assemblages and varying individual taxon traits, and because biological evolution is an irreversible process over extended periods of time. In contrast, archaeological assemblages, including ceramics, are relatively "information-poor" under present analytical techniques, and patterns of irreversible cultural evolution may play only a small role. Ultimately, perhaps, more sophisticated technical methods may increase the amount of information routinely recovered from potsherds to the point that type samples are an important tool for organizing this information and for organizing the discovery of still further information, but that situation does not seem to apply at present. Irreversible evolutionary trends which would give such information direct chronological value are few in archaeology; perhaps some events such as the introduction of ceramic vessel technology or the use of bow and arrow might qualify, but these events, unlike the minutiae of biological evolution, are more efficiently handled by direct description rather than by definition of type sites or collections.

Monothetic type definitions have not clearly been seriously proposed for Southern California ceramics. However, some applications of the polythetic typologies seem to have treated these typologies as if they were monothetic, i.e., using color as necessary and sufficient for ware discriminations, or using temper mineralogy as necessary and sufficient for type discriminations. One disadvantage of this procedure is its confusion with the same classes defined polythetically or by comparison with type samples. Another is its elaboration of the naming process beyond practical advantage: sherds which are buff-colored or brown-colored, with no more than that implied, are more economically called buff or brown sherds rather than Lower Colorado River Buff Ware or Tizon Brown Ware; if tourmaline is present in the temper, it is as simple to say just that as to call it Sentenac Brown. Monothetically-defined typologies may become important when a multi-tiered hierarchy of significant attributes has been recognized and arranged, but that is not the case at present.

Finally, the definition of types per se may be dispensed with, and attributes of interest may be systematically recorded. This seems the approach most appropriate to Southern California ceramic analysis at present. Suspicions exist that certain observable attributes such as color, rim form, and the characteristics of inclusions correlate with factors of interest for reconstructing prehistory. Equally, suspicions exist among critics, such as Donnan and Van Camp, that these correlations do not exist. The issue is worth testing.

Two lines of approach seem warranted in the development of ceramic attribute studies. First, archaeological collections should be examined to determine the extent to which attributes proposed as culturally significant do in fact show significant temporal and geographical correlations. This

must be done in an explicitly organized and publicly reported manner if it is to supercede the intuitive insights of Malcolm Rogers and others. Second, replicative experiments should be undertaken, using clays from a variety of sources (including sedimentary clays from western lagoons and residual clays from low desert mountains, if available) and using various preparation techniques, not only to attempt to duplicate patterns but to understand the cultural choices represented by patterns which are possible but not present prehistorically. Such experiments should also do much to clarify the accidental or intentional nature of several sorts of attribute variability.

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COMMENT

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I read with interest "A Selected Bibliography of California Cultural Chronological Documents in San Diego County" by Steve Shackley (1983, Casual Papers, Cultural Resource Management, February, Volume 1, No. 2, CRM Center, San Diego State University) and focused upon the fact that I am one of those "people working in San Diego today (who) have an actual and documented interest in the local prehistory (in a research sense)" (True Ibid.:84). I experienced a burning need to direct the readers to my point of view on the chronology west of the Lower Colorado River in the face of Michael Waters' critical view of Albert H. Schroeder's hypotheses concerning ceramics and chronology.

Having tackled the problem of ceramics in southern California following training under Dr. Richard Shutler and adopting Harold Colton's 1953 Potsherds as a foundation for any research into the problem of ceramics, I invested one year in 1972 examining all the ceramic data at the San Diego Museum of Man and then drove to Prescott College in Arizona to examine examples of Tizon Brown Ware. I did not quantify my data in those years nor did I obtain permission to thin-section specimens or conduct spectrographic analyses of "type" samples. What I found at the San Diego Museum of Man was (a) a large collection of reconstructed vessels ordered by the late Malcolm J. Rogers, (b) a huge collection of "type" specimens in dozens of drawers as identified by M. J. Rogers (and summarily mixed by the U. S. Navy in World War II), and (c) a book of unpublished type descriptions compiled by Rogers (some of which Schroeder had published in 1958).

I also had the opportunity to examine over 1,300 sherds from a buried site at Kitchen Creek (SDi-80) which was to be destroyed by the construction of Interstate 8; who says CRM does not offer contributions to California archaeology (True 1983:88)? Taking Rogers' type collection and his collections from the Peninsular Mountains and the works of others (Euler 1959; McCown 1955; Meighan 1959; Townsend 1960), I was able to examine the distribution of Tizon Brown Ware and Lower Colorado River Buff Ware (May 1975a) to ascertain whether or not patterning or randomness exists. Indeed, patterning of types did exist and even prove that at SDi-80 Lower Colorado River Buff Ware appeared late in the archaeological record.

The quantifiable data on the ceramic analysis appeared in "An Archaeological Salvage Report on the Excavations at Kitchen Creek: An Investigation of a Subsidiary Hakataya Resource Camp and Associated Milling Stations," which lies in the CRM Center and District 11 of Caltrans. Since the report was compiled without funding, no radiocarbon dates exist to orient chronological interpretation.

Schroeder and I collaborated via correspondence in the mid-1970's to name the "un-named" Hakataya Branch (Shackley 1983:65) in 1978:

The Hakataya Folk Tradition can best be illustrated by a demonstration. The region west of the Salton Sea to the top of the Peninsular Mountains covers two distinct environmental zones. These are the mountain and low desert. Archaeological sites can be found at natural springs, along ancient shorelines, and major drainages. Usually, this can be found to coincide with sand dunes and populations of mesquite. The Patayan Stem encompasses the major drainages and meadows which cross the California Peninsular Mountains and upland northwestern Arizona. In these places, the archaeological sites are close to well-drained areas forested in oak with subsidiary resource camps radiating out from villages like spokes from a wheel.

Following this theoretical pattern, branches can be found within the stems. These branches are evidenced by regional ceramic types and regional series of types which seem to be quantitatively dominant....

Correspondence with Schroeder has established that a 'Salton Branch' has been suggested for the low desert areas surrounding the Salton Sea and the Lake Cahuilla drainage system. This researcher has assumed that the Salton Branch joins the Colorado Branch in the Laquish Stem. Furthermore, it is suggested that 'Salada Branch' be tentatively considered for the area south of the Salton Branch and that it may extend along the west shore of the Gulf of California as far south as Isla de Los Angeles. Logically, the ceramics found in 'Seri Land' around Kino Bay, Sonora, Mexico, would seem to be an eastern extension across the Gulf.

The problem of addressing the development of ceramics in the Peninsular Mountains and coastal area has been tentatively solved by naming the area 'Peninsular Branch'. It does appear that the Tizon Brown Ware in California is quite different from the types found in the Cohonino and Cerbat Branches of the Patayan Stem in northwestern Arizona (May 1978:7).

What I have attempted to do is propose a model for tackling the problem of ceramics and their makers in California as they related to peoples of Arizona and Nevada. I did not re-invent the wheel and, thus, assumed a role of student of Rogers, Colton, and Shutler. I recognize that my approach is to isolate or "split" and then test for randomness or patterning. Just because a person does not comprehend the meaning of a pattern, does not necessarily mean that behavioral patterning did not exist. Waters,

on the other hand, is severely critical of Schroeder and myself and insists on 'lumping' Rogers types. In my opinion, none of us know for certain what occurred back before A. D. 1900 and that persistent testing of all possible hypotheses will eventually prove out whether or not the lumpers or the splitters are on the right track.

One pattern which has emerged from CRM studies in the past 12 years is the fact that ceramics did not appear uniformly at sites west of the Peninsular Mountains. Judy Berryman found abundant Tizon Brown Ware at the lowest levels at Santee Greens at A. D. 730, while Brian Smith found none at Mother Grundy Estates at A. D. 600 and Russell Kaldenberg found none at Sweetwater Springs at A. D. 1200. Tizon Brown Ware appeared at Cotton wood Creek after A. D. 1000 and has been reported by Jan Townsend to appear after A. D. 1200 in the Bonita Miguel area. More peculiar is the very late appearance of Lower Colorado River Buff Ware types in buried sites in the mountains.

It is my hypothesis that Tizon Brown Ware originated outside of California and Arizona and the ceramic-makers entered the Colorado Desert region around A. D. 600-700. This has been suggested by Schroeder's data at Willow Beach, Arizona and Berryman's data at Santee Greens. The dynamics of the diffusion and its implications upon local population developments need to be better examined by sophisticated anthropological research in future years. "Types" developed as local innovations-which-became-traditions among clans of both Shoshonean and Yuman-speaking culture groups. Fluctuations in political territories and clan affiliations can be tested by dating type origins and distributions. Types can be distinguished by formulae involving paste improvements (crushed quartz, mica, potsherds, etc.), and firing techniques. With more data, rim variants might also assist in keying out regional types. The appearance of ceramics at staggered times over large regions will be a key to understanding prehistoric political gerrymandering; in that swift acceptance followed one political unit while late appearance in otherwise occupied sites between A. D. 700 and 1200 indicate at least one other unit.

Students reading the Casual Papers and searching for thesis topics really ought to consider using ceramics as a problem. Professionals do not even know how long pottery has been in common use by people in the San Diego area. Mary Lou Heuett is currently working on thin-section and spectrographic analysis of Tizon Brown Ware and Lower Colorado River Buff Ware types. While large collections from buried sites certainly exist, few have been typed and tested. The problem certainly is interesting and needs more attention.

## RESPONSE TO D. L. TRUE'S COMMENTS

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Dr. Delbert L. True's comments on San Diego County CRM in the last issue (Casual Papers 1(2):82-89) were certainly lively and provocative. Many of them--including his questioning of the desirability of mechanical approaches to standardization in field procedures and in site significance evaluation, and his concern for the adequacy of research motivation of some of those involved in CRM--are in my view very well taken.

On the other hand, some of his expressed attitudes, and others which seem to be implicit in his responses, appear to me to be poorly founded and ill-suited to serve those values which Dr. True and at least some CRM archaeologists share.

Dr. True suggests that 20 years ago, to be an archaeologist was to belong to a respected profession, and that not it is not; indeed, now it "could be downright dangerous." Perhaps, rather, 20 years ago being an archaeologist was to indulge a harmless but possibly interesting eccentricity, in the eyes of the general public. Today, it is increasingly recognized that being an archaeologist--being actively concerned with preserving and interpreting that dwindling remnant of the physical record which has not been destroyed in the past 20 years and the preceding decades--is to be involved with some difficult and potentially controversial social issues. One of these is involved with the relative importance to be given to preserving portions of our heritage as against allowing unrestricted exploitation of short-term land values. Another may be the conflict between scientific archaeological advances in understanding and some traditional cultural ideas and ideological vested interests--the same sort of cultural conflicts which, in a larger context, attended the emergence of modern astronomy, geology, and biology. These conflicts are real, and possibly even dangerous, but I think we can live with them. Archaeologists can increase their public respectability by improving their professionalism in handling these difficult issues and by trying to do a better job in communicating with the general public. They can only harm even their own self-respect by trying to fade back into a completely passive, innocuous role.

I think Dr. True puts too much emphasis on a supposed contrast in motivation between CRM "non-archaeologists" and (implicitly, academic) Real Archaeologists. Unquestionably, some who do CRM archaeology are inadequately motivated, more interested in making dollars than in making research contributions. The same thing can also be said of other individuals who combine archaeology with teaching rather than with land use

planning. I am sure Dr. True has heard reports of academic types faking data or otherwise acting irresponsibly in their research, for the same basic motivations of money and advancement which he sees as so corrupting in CRM. The real contrast is between sincere, responsible, productive archaeologists, whether in academia, public service, or private business, and those who cut corners and betray trusts, whatever their affiliation. If there is a difference in the frequency of the two types in different institutional settings (if there is such a difference), it is the responsibility of committed professionals to look for methods of institutional reform, not just to repose in a facile cynicism.

Dr. True criticizes the research contributions of CRM archaeologists as often characterized by "careless scholarship" and too little concern with real research issues. Unfortunately, there is more than a little truth in this criticism. However, the implicit contrast between this situation and the Golden Age of Academic Archaeology (or present-day academic archaeology, for that matter) seems unwarranted. It has always been relatively easy to dig holes and to walk over hills; it has always been difficult to take archaeological data and apply them in sound ways to real research problems. It seems to me that if Dr. True were to take another look at the "unbureaucratic" work performed and the reports which were written 20 years ago (and still more, the innumerable reports which were not written 20 years ago) he would find little reason for complacency and more reason, if not to excuse, at least to understand, the shortcomings of present-day CRM archaeology.

My point in all this is not to deny the faults of CRM archaeology or to denigrate non-CRM archaeology. The real problem, as I see it, is the conclusion which Dr. True (and others with him, of course) propounds: to write off cultural resource management, muddle along, and hope for better days to come. Such Olympian detachment, it seems to me, supports precisely those elements of the archaeological community which tend most towards bringing it into a sometimes-deserved disrepute. If the senior member of the profession, the Ph. D.s, the tenured professors, do not deign to draw any fine distinctions between mechanical, money-oriented CRM work and the efforts of sincere, competent archaeologists to conserve a part of this heritage, the competitive disadvantage of the latter group is only worsened. Such a strategy of detachment may help distance its users from the ill-will of some developers and bureaucrats, but it can hardly be considered evidence of a genuine commitment to the research values of the region.

In contrast to Dr. True's conclusions, I think there is something constructive we can do about the quality of archaeological work, both CRM and academic, in San Diego County. Casting aspersions on the sincerity, integrity, or competence of a whole category of archaeologists is not an answer. Nor is it reasonable to attempt to reduce the practice of



archaeology to a sterile by-the-numbers drill. The best hope for improving the professional standards of local archaeology is surely for those with genuine research commitments, like Dr. True, to become more actively involved in professional review and criticism of the work being done. Archaeology by "non-archaeologists" cannot be screened out by checklists in the hands of uninformed and uninterested bureaucrats, as Dr. True rightly insists, but it can be distinguished by sincere professionals, and it can be discouraged. The work of the Real Archaeologists, too, can be improved by a more active critical dialogue within the discipline.

Archaeology at this stage cannot afford to write off preservation efforts, the data collected in those efforts, and the opportunity for advancing research goals attendant upon them. Dr. True needs to make use of the actual and potential contributions of CRM archaeology, and CRM archaeology needs Dr. True's active, critical participation.

## EDITOR'S CORNER

Some concern has been expressed that the term "Casual"\* is used in the title of this periodical. At least one archaeologist has voiced an hesitation that the inclusion of such a term would not go far in enhancing one's resume. So, why then "Casual"? Why not, say "Profound" or "Important" or maybe "Serious"? In fact, the editors have no particular reason for selecting the term "Casual." Indeed, this lack of a reason may be the reason. The goal of this publication is simply to provide information related to cultural resources in San Diego County of potential interest in as timely a fashion as possible. In any case, to date, to our knowledge, there have not been any problems with anyone confusing this periodical with American Antiquity.

\* casual = occasional (Webster's)

