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BATIQUITOS LAGOON REVISITED

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In the early 1960s, Claude Warren, Robert Crabtree, D. L. True, Max G. Pavesic, Carl Hubbs, and Jacqueline Miller provided a series of reports on Batiquitos Lagoon. These reports focused on La Jolla to late period archaeological sites and the environmental setting for the past 8,000 years. As a result of these works, many statements were made which affect our understanding of southern California prehistory.

The purpose of this paper is to review some of these statements concerning environmental change, degradation and periods of habitation at Batiquitos Lagoon. These statements are addressed by the following topics and associated questions.

Environmental Change and Abandonment of Batiquitos Lagoon

Warren et al. (1961:25) present a case for environmental change and abandonment stating that "from about 2500 to 3000 years it appears that Batiquitos Lagoon could no longer support a supply of shellfish adequate to maintain a sizeable aboriginal population." Carl Hubbs (Hubbs et al. 1962:222) criticizes the environmental change and abandonment argument stating "the species composition indicates a warm temperate fauna, with no definite indication of temperature different from those now prevailing. The shell gathering culture seems to have persisted around the Lagoon, at least intermittently, for ca. 7 millenia, in response to adequate food supply and water at or near the surface...."

The discrepancies between Warren et al. (1961), and Hubbs et al. (1962) include the following:

1. Was there environmental change at Batiquitos Lagoon?

2. Was there abandonment of Batiquitos Lagoon?

3. If there was abandonment, when did it occur?

The question of environmental change was approached by Miller during her Batiquitos Lagoon core study (Miller 1966). Results of this core study are summarized by Masters (1983):

> The coring equipment consisted of a bailer dropped through 10 cm i.d. casing. A total of 37 sediment samples were recovered to a depth of 12.14 m. Radiocarbon dates were obtained on shell from five levels. Miller concluded that the presence of shell in all samples below 3.8 m, corresponding to C-14 dates

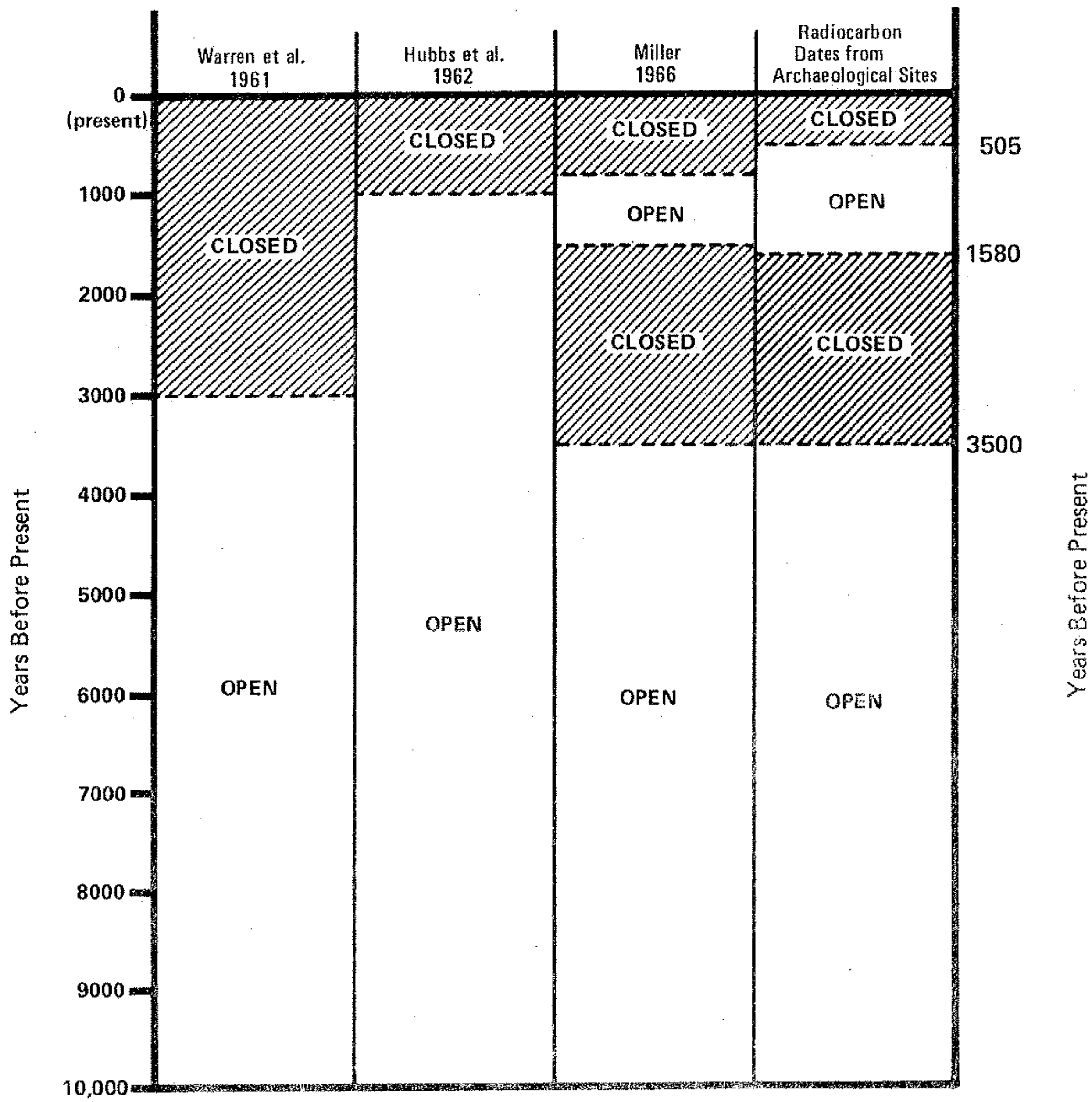
of ca. 3500 to 6500 years B.P., demonstrates that the Lagoon was open to the ocean during this time. The occasional presence of open coast species (Donax gouldi and Tivela stultorum) also indicate tidal exchange. The total absence of shell between 2.03 to 2.84 m, estimated 1500 to 3000 years B.P., she interpreted as evidence that the Lagoon was closed during this time. However, it was open and flushing ca. 1000 years B.P. based on the large sample of Argopecten aequisaculcatus obtained at 1.64 to 2.03 m in the core.... Miller cited the lack of shell above 1.64 m as evidence for closure from ca. 800 years B.P. into the historical period.

Miller plotted depth in the core against radiocarbon age and stated that the "uniform" rate 6500 to 4000 years B.P. was broken by rapid sedimentation between 4000 and 3000 years B.P. The rate decreased from ca. 3400 to 1000 years. She compared her plot with the sea level curve of Shepard (1961) and suggested that this area of the Lagoon was either very shallow or closed ca. 3500 years B.P.

It appears that all of these researchers agree that Batiquitos Lagoon closed at some point in time. The closing of Batiquitos Lagoon would have been a significant environmental change causing abandonment, as shellfish were a major food source. The scenario for these three researchers is presented in Figure 1. Warren and Pavesic (1963) approached the question of Batiquitos Lagoon's closure date by compiling 42 radiocarbon dates from coastal archaeological sites between Agua Hedionda and Sweetwater River, an area approximately 120 square miles in size. The results of C-14 data compilation suggested heavy occupation of the San Diego coast after about 3000 years B.P. and before 1500 years B.P. Light occupation was believed to occur between 1500 and 3000 years B.P.

At the time Warren and Pavesic (1963) conducted their study, only seven radiocarbon dates were reported for Batiquitos Lagoon. Presently 66 radiocarbon dates from 30 archaeological sites are available within 2 miles of Batiquitos Lagoon (Table 1). These dates are nearly continuous from 505 to 1580 years B.P. and from 3500 to 8280. Only one date (2640 B.P.) falls between 1580 and 3500 years B.P. and this date is questionable given the presence of pottery and the author's own comments concerning W-977 (Bull 1978).

On the basis of radiocarbon dates, it can be stated that there were two separate and distinct occupations of Batiquitos Lagoon. The radiocarbon dates support the findings of Warren and Pavesic (1963) as well as Miller (1966) with a lagoon closure taking place circa 3500 years B.P.; a reopening circa 1580 years B.P.; and a second closure within the past 500 years.



Batiquitos Lagoon Scenarios of Openings and Closures

FIGURE
1

Table 1

BATIQUITOS LAGOON DATES SORTED BY EARLIEST RADIOCARBON DATE

	SDi Number	W Number	Date (B.P.)	C-14 Number		SDi Number	W Number	Date (B.P.)	C-14 Number
1.	6144	1949	505±80	UGa-2878	34.	—	588	4520±250	UCR-406
2.	4394	49C	550±150	UCR-442	35.	8697	942	4940±70	LJ-3720
3.	6147	1951	580±65	UGa-2882	36.	—	181	5170±230	UCR-420
4.	6144	1949	625±85	UGa-2880	37.	763	106	5250±50	LJ-3484
5.	4392	49A	710±40	LJ-3159	38.	4358	108	5460±70	Beta-8318
6.	6144	1949	775±60	UGa-2884	39.	4358	108	5780±90	Beta-8317
7.	693	102	825±200	LJ-243	40.	4395	179	5880±125	Gak-4711
8.	2739	1952	830±70	Beta-8327	41.	4847	110	6000±70	LJ-5483
9.	5955	1318	835±75	UGa-2885	42.	4405	945	6140±80	Beta-11105
10.	693	102	870±200	LJ-242	43.	—	181	6210±280	UCR-421
11.	6142	1947	900±65	UGa-2879	44.	603	86	6250±150	LJ-256
12.	6142	1947	920±60	UGa-2883	45.	4847	110	6270±70	LJ-5299
13.	693	102	1075±150	LJ-245	46.	4847	110	6650±40	LJ-5485
14.	946	149	1100±50	LJ-3844	47.	4847	110	6670±50	LJ-5484
15.	6144	1949	1100±70	UGa-2881	48.	4410	951	6800±80	LJ-3719
16.	5415	1320	1125±55	UGa-2578	49.	—	915	6880±280	UCR-434
17.	946	149	1160±50	LJ-3845	50	4405	945	6900±120	Beta-11106
18.	763	106	1210±180	UCR-407	51.	4392	49A	6900±280	UCR-432
19.	603	86	1270±250	LJ-386	52.	—	147B	7060±110	J-4135
20.	4399	920	1275±60	UGa-5043	53.	4405	945	7070±100	Beta-11107
21.	6144	1949	1310±90	Beta-8325	54	—	147A	7120±150	J-4133
22.	5415	1320	1395±55	UGa-2577	55.	4395	179	7130±80	LJ-3717
23.	946	149	1430±60	LJ-3820	56.	603	86	7300±200	LJ-36
24.	946	149	1460±60	LJ-3821	57	4395	179	7430±80	LJ-3718
25.	4402	940	1500±150	LJ-3689	58	—	147A	7440±110	J-4134
26.	946	149	1580±70	LJ-3822	59.	4407	948	7780±90	LJ-3688
27.	4867	977	2640±60*	LJ-3824	60.	4392	49A	8010±80	LJ-3244
28.	604		3500±200	LJ-35	61.	4392	49A	8030±80	LJ-3160
29.	763	106	3640±60	UCLJ-3485	62.	4392	49A	8040±80	LJ-3243
30.	4863	973	3640±60	LJ-3823	63.	4392	49A	8060±50	LJ-3245
31.	763	106	3620±60	LJ-3485	64.	4392	49A	8110±80	LJ-3246
32.	603	86	3900±200	LJ-31	65.	4850	919	8160±360	UCR-436
33.	763	106	4370±250	UCR-405	66.	4392	49A	8280±80	LJ-3161

* Questionable date pottery present.

What caused the closure of Batiquitos Lagoon?

Miller (1966) believes that a rise in sea level producing an increased sediment rate, caused the degradation of Batiquitos Lagoon. A documented rise in sea level during this period (Shepard 1961) may have caused a slowing of fresh water into the lagoon and a rapid sediment build-up. Masters (1983) points out a methodological problem that Miller lacked any correction for present height above sea level in the locality of the core. Masters (1983) also points out that a single core will not elucidate the geological history of a body as large as Batiquitos Lagoon. Since sediment is deposited first in the head of lagoons and gradually builds towards the mouth, sets of cores spaced longitudinally from the mouth to head of lagoon would give a more complete picture of the lagoon's history and productivity (Masters 1983).

Other causal factors for the closure of the lagoon would include the following explanation by Warren and Pavesic (1963):

> A rocky foreshore was probably maintained by the rapidly rising sea level that would keep the beach free of sand. The rocks of the foreshore were probably derived from the sea cliffs, which were presumably retreating as the sea level rose. In fact, at present, the longest gravel beach in Southern California extends across the mouth of the Batiquitos Lagoon and along the adjacent shore (Emory 1960:184). Furthermore, the longshore movement at this time probably created a barrier bar across the mouths of the lagoons and these bars may also have been composed largely of gravels as is the present beach (bar) across the mouth of Batiquitos Lagoon. With the reduced rate of rising sea level, the coastal bays may have acted as sediment traps, particularly if a barrier was blocking or partially blocking the outlet.

Is there evidence of sediment build-up which could have caused the degradation of Batiquitos Lagoon?

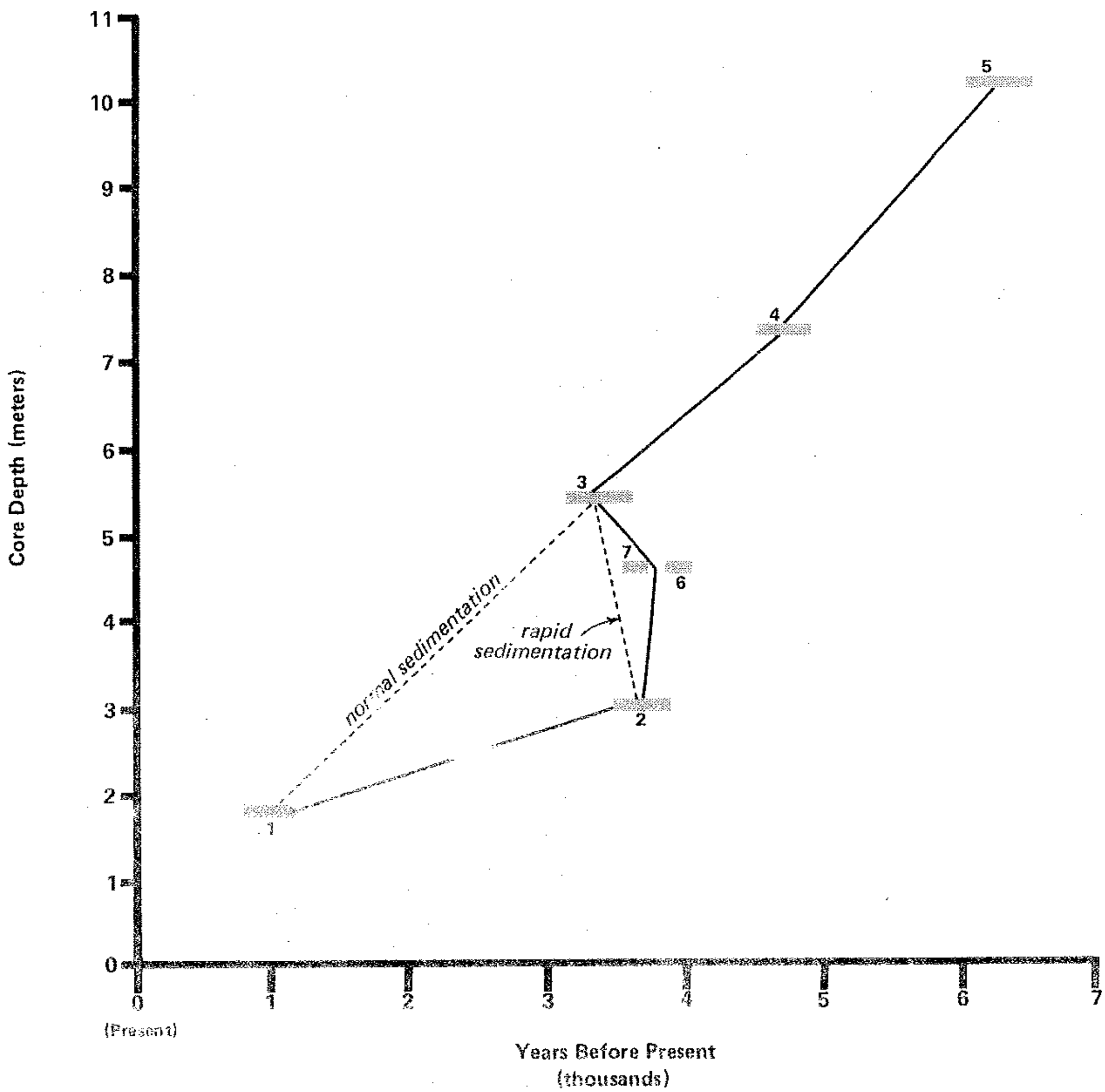
The only evidence is the one core study by Miller (1966). As a result of radiocarbon dating from selected levels, Miller was able to identify a 2 m build-up in sediment circa 3400 to 3700 years B.P. (Table 2). It should be noted that there is a reversal in the dates for levels 2.85 to 3.25 m and 5.25 to 5.60 m. This reversal can be explained in the plus or minus factor provided with the dates. As shown on Figure 2, the sediment rate could be considered constant if you threw out date number two (3700$\pm$ 200 years B.P.) 2.85 to 3.25 m depth. The result of discarding date number two allows for a constant rate of deposition which would support Hubbs in his contention that Batiquitos Lagoon did not close until circa 1000 years B.P. Miller could not throw out date number two; she explained this date as supporting a major sedimentation build-up circa 3400 to 3700 years B.P. Was it a constant sedimentation rate for Batiquitos Lagoon or rapid sedimentation circa 3500 years B. P. that degradaed the lagoon to the point that it could not provide abundant shellfish?

Table 2

CORE SAMPLE DATES

	Depth (m)	Date B.P.	Source	Material Dated
1.	1.65 - 1.95	1000±200	LJ-919	Argopecten
2.	2.85 - 3.25	3700±200	LJ-918	Ostrea
3.	5.25 - 5.60	3400±240	LJ-381	Chione
4.	7.25 - 7.50	4750±200	LJ-912	Chione
5.	10.00 - 10.45	6320±250	LJ-333	Argopecten
6.	4.09 - 5.14	3980±90	Beta 8467	Chione
7.	4.09 - 5.14	3650±80	Beta 8468	Argopecten

Notes: Dates 1-5 supplied by Miller (1966).
Dates 6 and 7 supplied by WESTEC Services, Inc. (1984).



Note: 1) dates shown with plus or minus factor
2) dates 1-5 taken from Miller (1966)
3) dates 6 and 7 supplied by WESTEC Services, Inc.

Batiquitos Lagoon Core Study, Sedimentation Based on Radiocarbon Dates

FIGURE 2

In 1983, the author, with the help of Betty Shore, Pat Masters, and Rose Tyson, relocated shell samples from Miller's core study. It was hypothesized that if shell between the levels 2.85 m and 5.60 m could be relocated and dated, then it could be determined if constant sedimentation for the past 10,000 years or rapid sedimentation circa 3500 years B.P. degraded the Lagoon's habitat. Should the dates fall on the same line as dates 1, 3, 4, and 5, then a normal sedimentation rate of 1 m of sediment was accumulating every 625 years. Should the dates fall in line with dates 2 and 3, then a period of rapid sedimentation circa 3500 years B.P. would be supported.

One Chione spp. shell sample and one Argopecten aequisaculcatus sample from the 4.09 to 5.14 m level were relocated and submitted by WESTEC Services to Beta Analytic for radiocarbon dating. The C-14 dates of 3980+90 B.P. (Beta 8467) and 3650±80 B.P. (Beta 8468) fell in line with dates 2 and 3 (Figure 2), thereby, supporting Miller's contention that, circa 3500 years B.P., Batiquitos Lagoon suffered rapid sedimentation build-up. The rapid sedimentation degraded the lagoon to the point that Batiquitos could no longer support a shellfish population and could be considered a closed lagoon.

The 66 radiocarbon dates from 30 archaeological sites presented earlier (Table 1) also support the closure of Batiquitos Lagoon circa 3500 years B.P. and a reopening circa 1500 years B.P.

Environmental Change and Shellfish Populations

Change in shellfish population was noted by Warren and Pavesic (1963). This change from a rocky foreshore to a sandy beach and mud flats, circa 10,000 to 6000 years B.P., "was more ecologically fit for Pecten (Argopecten aequisaculcatus) than for Mytilus," Warren and Pavesic (1963).

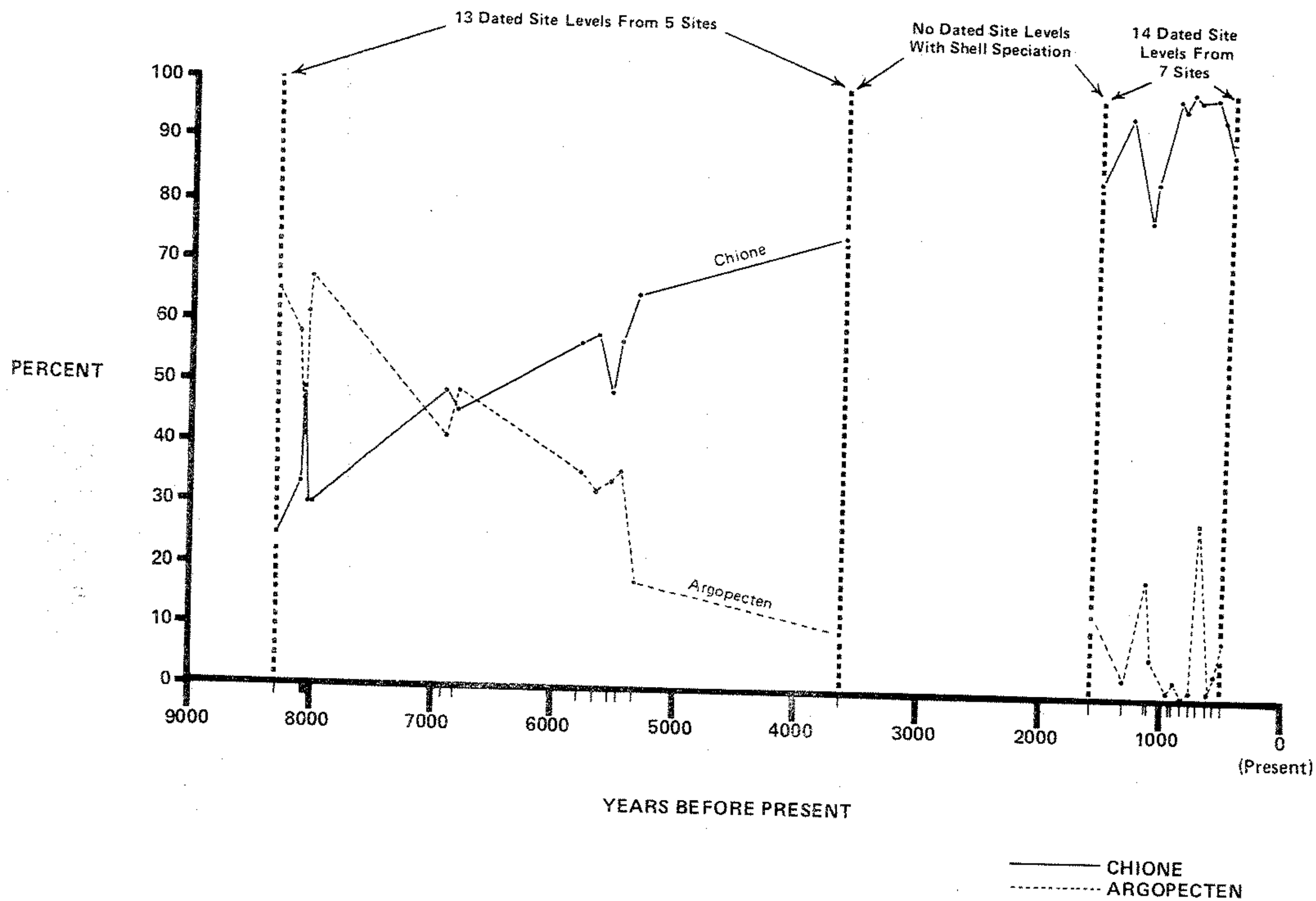
The period from 8000 to 505 years B.P. shows another change in shellfish ratio from Argopecten aequisaculcatus to Chione spp. dominance. This change was also noted by Bull and Kaldenberg (1976:34). Investigation of the ratio change from Argopecten sp. to Chione spp. was conducted by the author at 12 different archaeological sites at or near Batiquitos Lagoon. Twenty-seven radiocarbon dates from 8280 to 505 years B.P. have been produced for these twelve sites (Table 3). It was the intent of the author to use only radiocarbon dated and speciated levels from archaeological sites.

As can be seen on Table 3, sites dated circa 8000 years B.P. contain a dominance of Argopecten sp. shell to Chione spp. This ratio changes to a Chione spp. dominance of over 90 percent in the late period (Figure 3). The ratio change in Argopecten sp. to Chione spp. identifies an environmental change in the lagoon for the past 800 years. From 8280 B.P. to 3460 B.P., it appears that a somewhat regular rate of sedimentation was occurring in the lagoon. From 3640 to 1580, we have had no dated archaeological sites to infer etiher normal sedimentation or rapid sedimentation. And from 1580 to 505, a shallow lagoon or mud flat appears present.

Table 3

ARCHAEOLOGICAL SITES BY DATE, EXCAVATION LEVEL AND SHELL SPECIES

Site Number	C-14 Date (B.P.)	Source	Chione (%)	Argopecten (%)	Other (%)
SDi-6144	505±80	UGa-2878	90	10	
SDi-6147	580±65	UGa-2882	96	4	
SDi-6144	625±85	UGa-2880	99	1	
SDi-4392	710±40	LJ-3159	65	29	6
SDi-6144	775±60	UGa-2884	99	1	
SDi-2739	830±70	Beta-8327	100		
SDi-6142	900±65	UGa-2879	97	3	
SDi-6142	920±60	UGa-2883	99	1	
SDi-4867	1100±50	LJ-3844	83	4	13
SDi-6144	1100±70	UGa-2881	88	12	
SDi-946	1580±70	LJ-3822	85	14	1
SDi-763	3620±60	LJ-3484	76	10	14
SDi-4867	1100±50	LJ-3844	83	4	13
SDi-5415	1125±55	UGa-2578	79	20	1
SDi-6144	1310±90	Beta-8325	96	3	1
W-578	5310±250	UCR-363	65	18	12
SDi-4358	5460±70	Beta-8318	57	36	7
W-578	5525±90	UGa-361	49	34	7
W-578	5650±240	UCR-361	58	32	10
SDi-4358	5780±90	Beta-8317	57	36	7
SDi-4410	6800±80	LJ-3719	46	49	5
SDi-4392	6900±280	UCR-432	49	41	10
SDi-4392	8010±90	LJ-3244	30	67	3
SDi-4392	8030±80	LJ-3160	30	61	9
SDi-4392	8040±80	LJ-3243	49	41	10
SDi-4392	8060±90	LJ-3245	33	58	9
SDi-4392	8280±80	LJ-3161	25	65	10



Batiquitos Lagoon Chione to Argopecten Ratio by Radiocarbon Dates

(data taken from archaeological sites)

FIGURE 3

Summary

Early work by Warren and Pavesic (1963), and Miller (1966) appears well founded, given the data base present in the early 1960s. Miller (1966) identified two closures and one reopening of Batiquitos Lagoon within the past 10,000 years. The first closure she identifies is circa 3500 years B.P.; a reopening circa 1500 B.P.; and a second closure 800 years B.P. Recent work by the author suggests the second closure circa 500 years B.P. or earlier. Warren and Pavesic's (1966) seven radiocarbon dates from archaeological sites support Miller's (1966) findings and were supplemented with an additional 59 radiocarbon dates within this report. Two additional core radiocarbon dates provided by WESTEC Services also support Miller's original findings.

The work by Warren and Pavesic (1963) identified a change in shellfish species circa 10,000 to 6000 years B.P. from *Mytilus* sp. to *Argopecten aequisaculcatus*. This change in shell species was identified as resulting from the change of a rocky foreshore to a sandy beach and mud-flats.

Another shell species change occurred between 8000 and 1500 B.P. This change was from *Argopecten aequisaculcatus* to *Chione* spp. It is believed that this second shell species change is a result of siltation of Batiquitos Lagoon. The siltation is documented by Miller (1966) and is part of Warren and Pavesic's (1963) identification of an abandonment of Batiquitos Lagoon circa 3500 B.P. Evidence for abandonment of Batiquitos Lagoon is based on Miller's work and the near absence of dated archaeological sites between the period 1580 and 3500 B.P. The somewhat continuous dates from 1580 to 505 years B.P. suggest a reopening of Batiquitos Lagoon with a near dominance of *Chione* spp. shellfish.

Archaeological studies and environmental setting are closely interwoven. The archaeological data at Batiquitos Lagoon basically supports an early and late occupation of the Batiquitos Lagoon area. The early occupation as documented by radiocarbon dates is nearly continuous from 8280±80 B.P. (LJ-3161) to 3500±200 B.P. (LJ-351); and the late occupation from 1580±70 B.P. (LJ-3822) to historic contact.

The abandonment or depopulation of Batiquitos Lagoon can be based on increased sedimentation, circa 3500 years B.P., which degradaed the Lagoon to the point it could no longer support a viable shellfish population. Circa 1580 B.P., the Lagoon reopened, possibly caused by increased rainfall and lagoon flushing. Sometime between 500 B.P. and historic contact, Batiquitos Lagoon closed for the second time within the past 10,000 years.

Batiquitos Lagoon is one of our best studied areas and has provided archaeologists and environmental specialists with a wealth of information. Fortunately, the Batiquitos Lagoon area was subject to recent development after environmental laws were enacted and many of the cultural resources have been recorded, tested and excavated. Present and future work near Batiquitos Lagoon has the opportunity to recheck and evaluate previous findings, re-adjust interpretations and fill in the areas for which we have little information.

ACKNOWLEDGMENTS

I would like to thank Pat Masters for researching and locating Miller's core study and providing radiocarbon dates. Also to be thanked are Betty Shore and Rose Tyson for providing shell from Miller's (1966) study for radiocarbon dating. Dr. Ted Case and Colleen Kelly provided an understanding of shell size, variety and ratio change at Batiquitos Lagoon. Funds to process radiocarbon dates and to prepare the graphics were donated by WESTEC Services, Inc. Jay Thesken completed the technical illustrations; and comments from Richard Carrico, Charles Bull, Pat Masters, and Chris White were most helpful in completing this paper.

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SOME LINGUISTIC APPROACHES TO SOUTHERN CALIFORNIA'S PREHISTORY

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Introduction

In a recent article published here, Charles Bull (1983) has proposed revisions in some commonly accepted notions concerning the prehistory of San Diego County in particular and Southern California in general. Bull's article raises many potentially controversial points concerning the region's archaeological record. Additionally, linguistic evidence bearing on the same matters of regional prehistory is adduced.

The present article will not address Bull's archaeology. Neither will it deal specifically with the thorny methodological problems involved in attempting to coordinate archaeological and linguistic evidence. Instead, an attempt will be made to review and reevaluate the picture of the region's prehistory which seems to emerge from the linguistic evidence considered by itself. Insofar as archaeologists' interests in this region transcend the purely methodological, this linguistic evidence has an important bearing on some of the issues with which we are commonly dealing.

The linguistic evidence will be considered here only in terms of a specific problem, or a specific sort of prehistory: reconstructions of the expansion and contraction of linguistic areas--roughly but not without hazards equatable with population movements--their source areas, their timing, and their directions. This is the sort of linguistic prehistory most commonly considered by archaeologists, and it is the type specifically addressed by Bull. Nonetheless, it should be noted at least in passing that other potential contributions to prehistory from linguistic analysis do exist, notably the reconstruction of non-geographical aspects of past cultures from the semantic categories which were present in the reconstructed proto-languages. Indeed, reconstruction of earlier linguistic elements of all sorts, and the process of linguistic change, are also varieties of cultural prehistory in themselves (Haas 1969).

The Genetic Model

Fundamental to a consideration of linguistic prehistory is the genetic model of language change. In the genetic model, one or more daughter languages are considered to descend from an earlier mother language. Descent is branching only forward in time. Languages are grouped into families and other genetic units on the basis of common descent from a single ancestral language. Diffusion of linguistic elements into a language from other related or unrelated languages is not denied in the model, but such diffusion is assumed to have a limited character such that the true genetic source of the language is still a distinct entity, qualitatively distinguishable from mere diffusion sources.

A general continuum in the process of divergence of genetically-related languages is also envisioned. Within single languages there arise dialects--systematic geographical or social speech variants within a community which is still united by a large measure of mutual linguistic intelligibility. When, with the passage of time, mutual intelligibility has been lost, distinct languages are considered to have arisen. Languages whose genetic kinship is still close and quite evident are grouped into families and similar units. More remote or speculative genetic groupings may be termed stocks or phyla.

Linguistic divergence sometimes seems to be envisioned as a steady, inexorable process, but this is likely to be true only above a certain minimum socio-geographical scale. Linguistic change may indeed be steady and inexorable. However, within a speech community, there exist strong centripetal and well as strong centrifugal forces. The values of a shared medium of communication may equal or exceed the costs incurred in maintaining conformity. One may even envision a certain optimal size for a speech community, below which the burdens of bilingualism and imperfect communication which would follow separation become heavy, and above which local interests and entropy outweigh the benefits of unity, and take over. Such an optimum size should probably be envisioned as determining the direction toward which change will occur rather than as being a normally achieved condition; an overly large speech community will only gradually differentiate, and if an area of excessive linguistic fragmentation once forms, simplification must overcome a great deal of inertia. Optimal size for speech communities would probably also vary considerably with such factors as geographical obstacles, population sizes and densities, and a range of sociocultural variables such as endogamy and exogamy, intercommunity economic exchange, shared ritual activities, and so forth. Specifying an optimal speech community size may be difficult or impossible in a given case, but the concept of a spatial scale limit to linguistic diversification is useful.

An implication of the conjunction of the genetic model with considerations of scale is that the initial stages of linguistic diversification should occur primarily under certain specific conditions. One of these is speech-community expansion: the spread of a language over an area too large for linguistic unity to be maintained. The standard circumstance of this spread is a population migration into an new area, but it should be noted that a language may also spread by diffusion as a unit, without any necessary movement of peoples. Other initial causes of divergence might be (1) the emergence of communication barriers separating portions of a formerly-unified speech community, through the intrusive migration of other peoples or possibly through natural geographical changes, and (2) cultural changes reducing the optimal speech community size. It is fair to suppose that each instance of a mother language evolving into two or more daughter languages represents the operation of some such process as these.

The genetic model, as roughly summarized above, has proven its continuing usefulness in linguistic classification and in the reconstruction of linguistic prehistory. Nevertheless, it has some serious shortcomings, which need to be borne in mind.

For any three languages, A, B, and C, known to be genetically related, there are essentially only two patterns of relationships proposed by the genetic model (see Figure 1). One is that all three languages may be coordinate, that is, the separate ancestors of all three may have split off from a common parent language at a single point in the past. In this case, the genetic model predicts that in binary comparisons made among these languages, each of the three possible pairings--A-B, B-C, and A-C--will show an equally close or an equally remote relationship. The second possibility is that one of the ancestral lines, say that of A, split off first, and that the other two, B and C, separated from each other only later; that is, A is coordinate with proto-BC, and B and C are coordinate with each other but not with A. In this case, according to the model, valid binary comparisons should show the pairing B-C as being a closer one that either A-B or A-C, and they should also show the latter two pairs as being equally remote.

Figure 1. Models for Genetic Classification of Three Languages

In fact, available measures of linguistic closeness often contradict these expectations. The pairs A-B and B-C may be found to be equally close, while A-C is more remote, or each of the three pairings may show a different degree of relatedness. The problem in such cases may in part be the inadequacy of the methods of comparison. However, there is reason to believe that the fault often lies at least partly in the model itself. Linguistic divergence, from dialect formation to the erection of all barriers of mutual unintelligibility, seems typically to be a very drawn-out process. During that process, linguistic innovations emerge in a dialect and are diffused to some of the neighboring dialects but not to all of the more distant dialects. With multiple centers of innovation, a complex network of dialect variation arises; neighboring dialects are still mutually intelligible, and chains of intelligibility still may cross the entire area of the language, but widely separated dialects may already have too many unshared innovations to the mutually intelligible. Eventually, bridging dialects may die out, or the process of innovation may continue to the point that chains of intelligibility no longer hold the regions together;

several daughter languages have then emerged. These daughter languages may have innovated features which are shared in common with their nearby sisters yet which are not part of the common heritage of their geographically more remote, non-neighboring sisters. A centrally-located language B may thus share one set of innovations with A and another set with C, while A and C share none, thus frustrating an attempt to discover a consistent genetic subgrouping.

The Genetic Classification of Southern California Languages

The aboriginal languages of Southern California fall into three groups: Chumash of the Santa Barbara-Ventura area, Uto-Aztecan (Shoshonean) for much of the desert and the Los Angeles-Orange-northern San Diego county coasts, and Yuman in the southernmost and southeastern portions of the state (see Figure 2). The internal divisions of these three groups, their mutual interrelationships, and their relationships with other, non-Southern California groups, all provide clues to help interpret the region's prehistory. It is necessary to outline first what the various relationships appear to be, before attempting to interpret them.

The broadest genetic affiliations which have been proposed for these languages are continent-wide or even larger. Edward Sapir (1921) proposed a scheme uniting the languages of North America into just six superstocks. Of these, the Hokan-Siouan would include Chumash and Yuman, while Aztec-Tanoan, would include Uto-Aztecan. The second of these groupings has received more support than the first. Sapir's scheme was often uncritically adopted by scholars after its proposal, but more recently it has been strongly challenged (e.g., Campbell and Mithun 1979). Still more ambitious genetic lumpings were proposed by Morris Swadesh (e.g., 1964, 1967), who used lexicostatistical methods (discussed further below) to join most of the languages of aboriginal North and Middle America into a single Macro-Maya group, with still wider genetic links to the languages of South America and even Eurasia. Swadesh's proposals have never been generally accepted. Genetic relationships on this widest scale will be largely disregarded in the present paper, partly because of their still-questionable status, but mainly because such relationships, when and if they are established, are still not likely to have much bearing specifically on the prehistory of Southern California.

The next level of genetic relationship recognized for the central of the three language areas of Southern California is the Uto-Aztecan (Utaztecan, Yutonahua) family, with membership stretching from just beyond the northern Great Basin to Central America (see Figure 3). This family was recognized early (see Lamb 1964 for a historical discussion), and considerable progress has been made in confirming its validity through the comparative reconstruction of the proto-language. No controversy about its genetic validity exists currently.

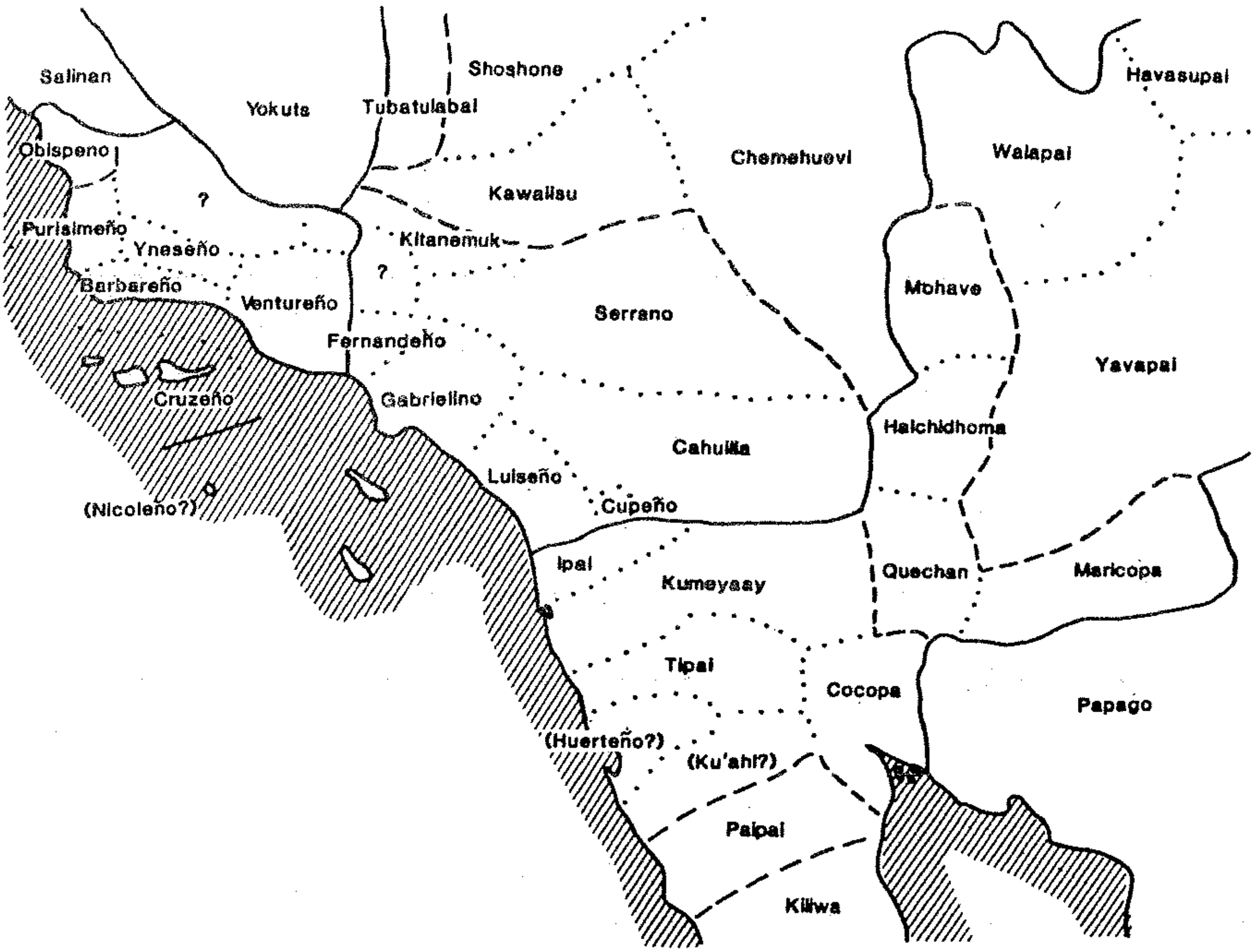

Figure 2. Map of the aboriginal languages of Southern California (modified from Kroeber 1925, 1939, and Hicks 1963).

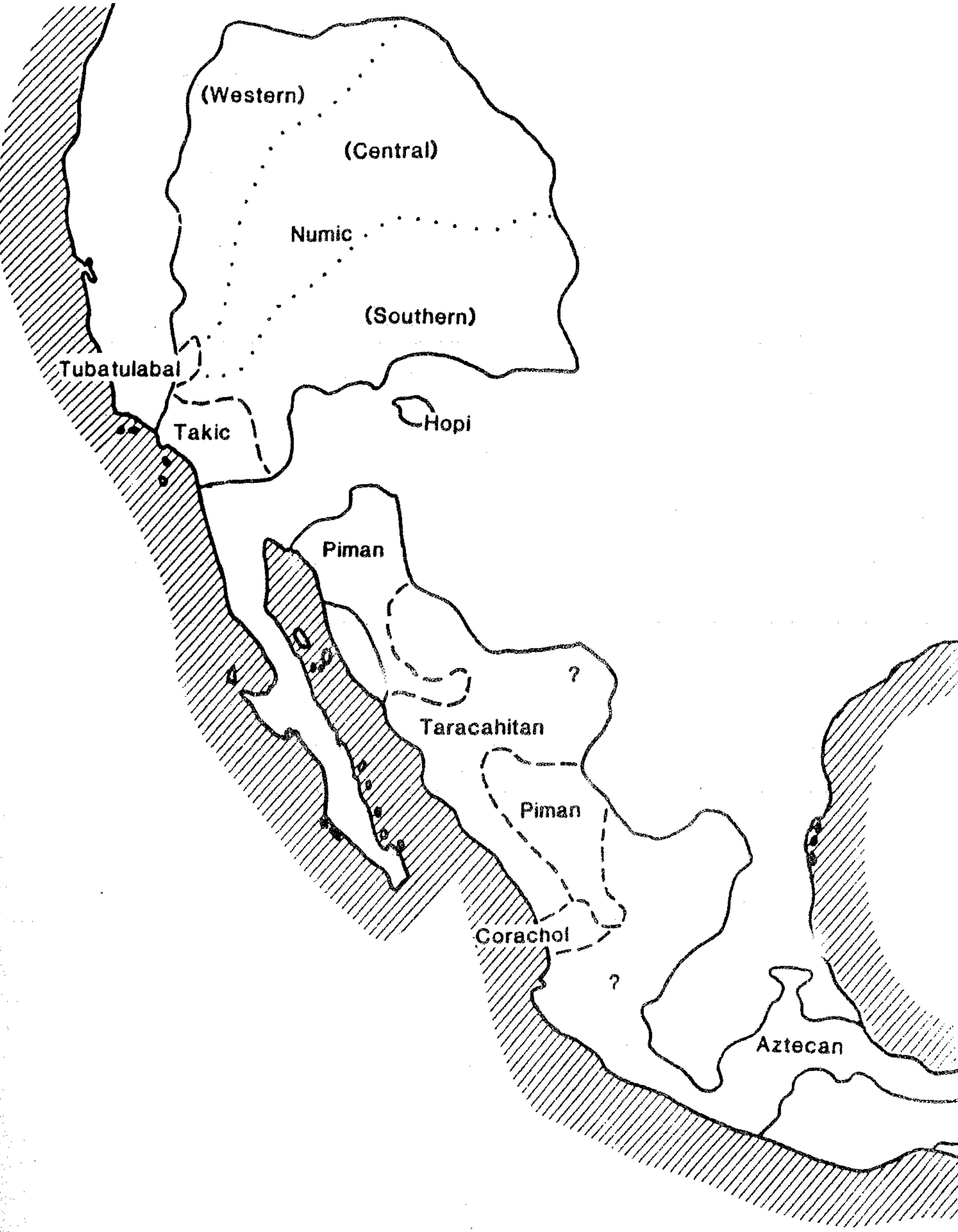

Figure 3. Map of the Uto-Aztecan linguistic family (modified from Kroeber 1934, 1939).

The genetic subgrouping of the Uto-Aztecan family has occasioned much more debate (see Figure 4). Early interpretations proposed three main Uto-Aztecan groups: Shoshonean (including all of the Southern California Uto-Aztecans), Sonoran, and Aztecan (see, for example, Hale 1958 for more recent support of this view). Some scholars have further grouped Sonoran with Shoshonean or with Aztecan to create only two divisions. Others, on the contrary, have rejected Shoshonean and/or Sonoran as genetic groups and have instead proposed eight or nine coordinate branches directly succeeding proto-Uto-Aztecan (e.g., Steele 1979). For the rejected Shoshonean division, these branches are Numic in the Great Basin, Tubatulabal in the southern Sierra Nevadas, Takic in Southern California, and Hopi in northern Arizona. In a recent review of the problem, Miller (1983) has retained these four branches as primary and coordinate divisions and has proposed a southern branch also coordinate with them, embracing all of the Sonoran and Aztecan languages. The matter is still clearly not settled, however.

Of the northern Uto-Aztecan branches, two--Tubatulabal and Hopi--are represented by single languages. Numic is more complex, with three subdivisions--labelled Western, Central, and Southern Numic--each of them represented in or near northeastern Southern California and each also extending out across the Great Basin.

The subdividing of Takic (also termed Luiseñic or Southern California Shoshonean) is more complex. Some have divided it into two groups: Cupan, including Luiseño, Cahuilla, and Cupeño; and Serran, containing Serrano and Kitanemuk. The position of the poorly-known Gabrielino language (or languages, with Fernandeno and perhaps Nicoleño as well) is uncertain. Bright (1974) has assigned it to a subgroup within Cupan, joined with Luiseño as against a Cahuilla-Cupeño subgroup. Shipley (1978:90) has given Gabrielino coordinate status in Cupan with Luiseño, Cahuilla, and Cupeño. Miller (1983:120-121), in contrast, has suggested that Gabrielino may either be coordinate as a group with Cupan and Serran, or that it may belong to a Serran-Gabrielino subgroup in which it is coordinate with Serran. The existence of Cahuilla and Cupeño as separate languages, closer to each other than they are to Luiseño (Bright and Hill 1967) seems to be fairly generally accepted, as does the status of Juaneño as a dialect of Luiseño.

The non-Uto-Aztecans of Southern California--the Chumash and the Yumans--have generally been grouped within a Hokan phylum or stock (see Figure 5). The geographical range which has been proposed for Hokan (or Hokan-Coahuiltecan) is as extensive as that of Uto-Aztecan, extending from Southern Oregon to Central America, but the southern and eastern portions of this range have been particularly controversial, the distribution is much less continuous, and the linguistic relationships involved are far more tenuous (see Figure 6). Conservative linguists have questioned the validity of Hokan as a genetic unit (Campbell and Mithun 1979), but it has most often been accepted at least provisionally (Langdon 1974).

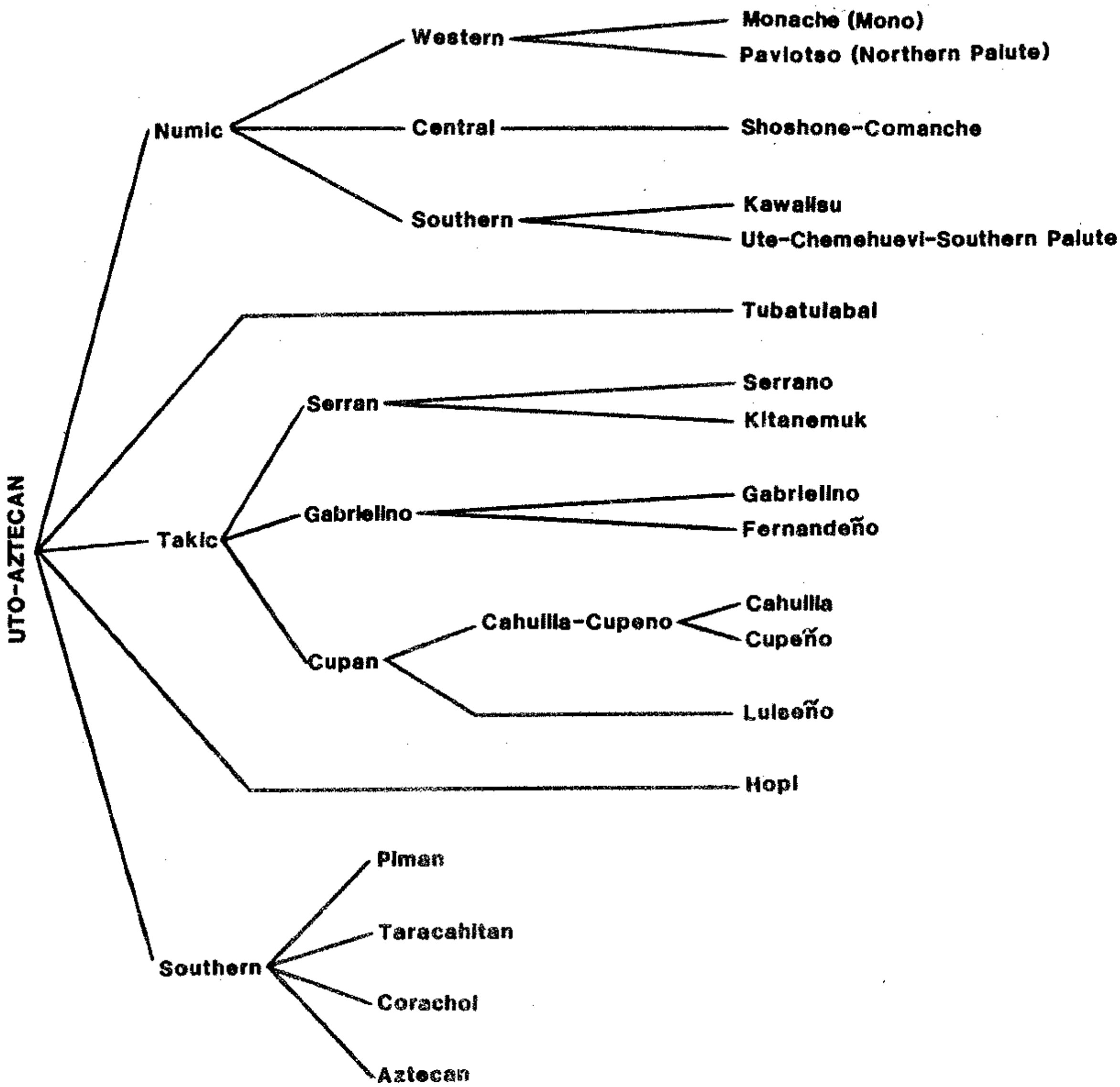

Figure 4. Genetic classification of the Uto-Aztecan family.

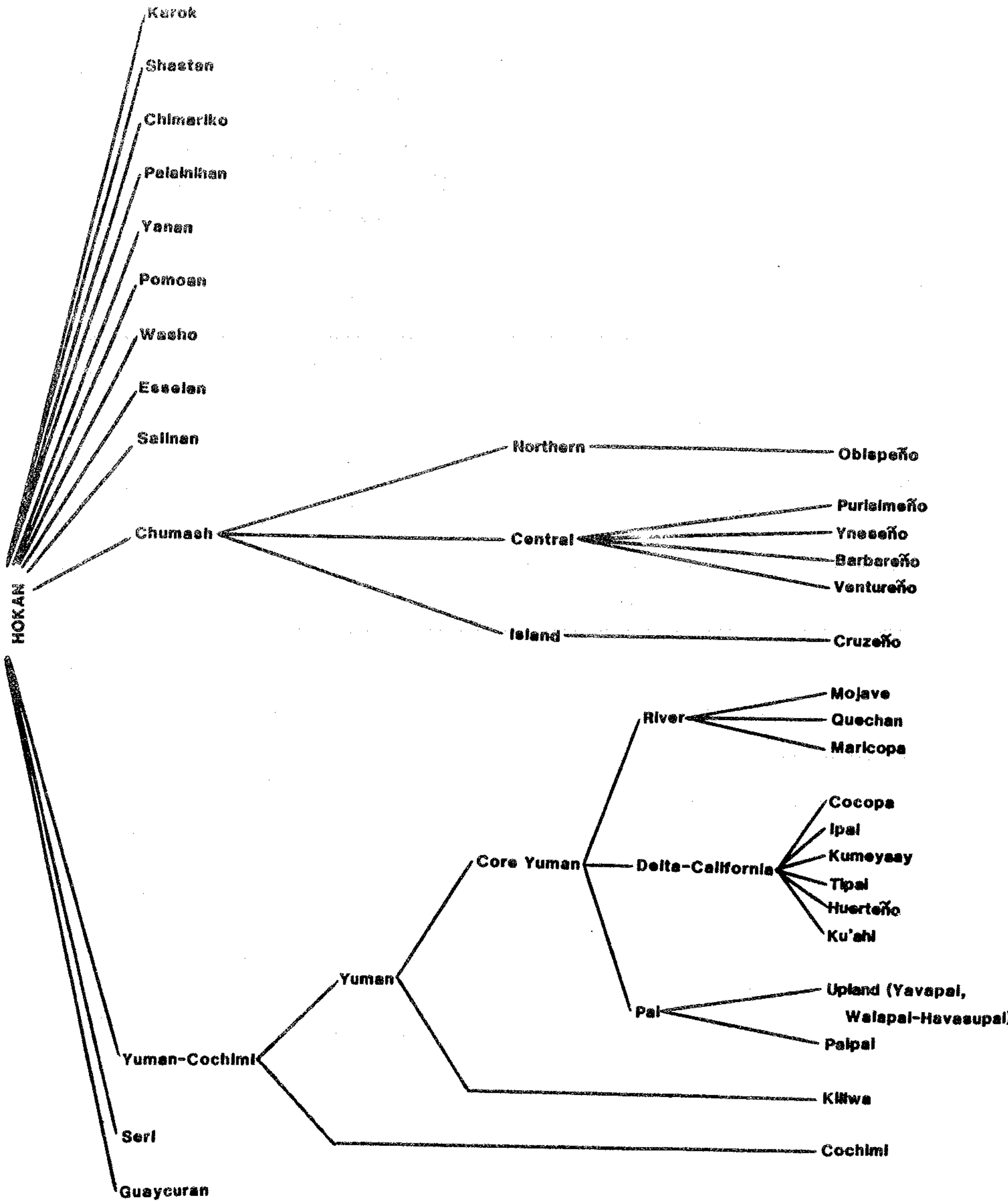

Figure 5. Genetic classification of the Hokan phylum.

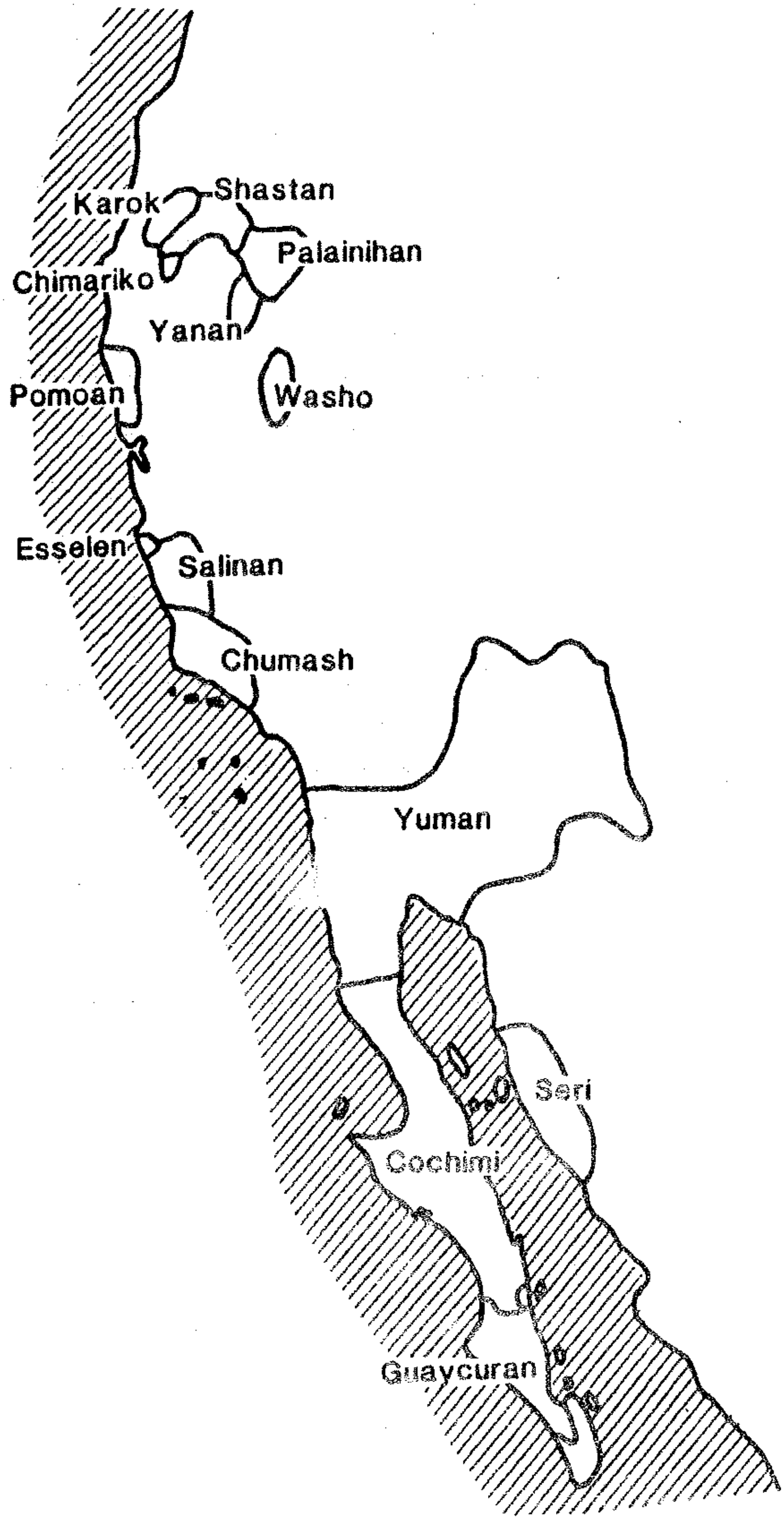

Figure 6. Map of the Hokan linguistic phylum (modified from Kroeber 1939).

Genetic subgroupings of families within the Hokan stock have been proposed only very tentatively. Because little has as yet been achieved in reconstructing the characteristics and content of the proto-Hokan language and demonstrating cognate forms in the daughter languages, little confidence can be given to proposed subgroupings; but for the same reason, any assumptions about the absence of such subgroupings (i.e., assumptions of the coordinate status of the various Hokan families) should be regarded as equally undemonstrated.

One subgrouping within Hokan which seems fairly secure and which has some relevance to Southern California's prehistory is the linking together of the Yuman family and Cochimi. The latter was a group of dialects or possible of close but distinct languages which were spoken throughout the central half of the Baja California peninsula. Cochimi is now extinct; it is known, very imperfectly, from accounts by eighteenth-century Jesuit missionaries and others. Cochimi has sometimes been classified within the Yuman family, as "Peninsular Yuman" (Massey 1949), but it is evidently more properly a separate family or isolate, with, however, a close genetic relationship to the Yuman family (Mixco 1978).

A Hokan language which is particularly important in respect to subgroupings and their implications for Southern California prehistory is Seri, spoken on the central coast of Sonora, Mexico, and on Tiburon Island in the Gulf of California. Seri evidently has undergone patterns of linguistic change which have made difficult the recognition of forms cognate with other Hokan languages (Langdon 1981). Some early linguists proposed a particular relationship between Seri and the Yuman family, perhaps primarily on the basis of geographical proximity and of cultural links. Later, Sapir proposed a more geographically-anomalous linking of Seri with Chumash and Salinan and of Yuman with Esselen. Bright (1956:48) also supported a closer link between Seri and Salinan than between Seri and the Yuman family. Bull (1977:53-56, 1983:50) has stressed this proposed grouping of Seri and Chumash in his interpretation of southern California prehistory, and has cited Langdon as a supporter of the grouping. However, more recently, Langdon (1981) has spoken in favor of a grouping within the Hokan phylum of Seri with the Yuman-Cochimi group. Evidently the matter needs further study; little security can be felt in interpretations based upon either grouping (Seri with Chumash or Seri with Yuman-Cochimi) at this stage, but the latter hypothesis may perhaps be somewhat favored.

Narrowing the focus, the relationships within the Chumash and Yuman families may next be considered. At this level, the data available are more nearly adequate to the task of demonstrating genetic connections. Nonetheless, uncertainties and controversies also exist.

Chumash is now extinct, but was studied by Merriam, J. P. Harrington, and Beeler, among others, and the data collected are now being analyzed by several students. Klar (1977) has proposed that an initial division spilt proto-Chumash into Southern and Northern groups, the latter giving rise to the Obispeño language. The Southern group in turn divided into Island and

Central groups. Island Chumash gave rise to the Cruzeño language and possibly a distinct Roseño language. Central Chumash produced Yneseño, Barbareño (including Emigdiano), Ventureño (including Alliklik), and Purisimeño. Yneseño and Barbareño may also constitute a subgroup within Central Chumash.

The Yuman family is well-represented by about a dozen surviving languages, with aboriginal territories in western Arizona and northern Baja California as well as in Southern California. A number of proposals and counter-proposals have been made as to the proper genetic subgroupings for this family (see Kendall 1983).

The River Yuman branch, encompassing Mohave, Quechan, and Maricopa, has been recognized by all authorities. Now-extinct Halchidhoma and Kavelchadom also evidently belonged to this branch, but their further relationships within the branch cannot be determined. A proposal (Biggs 1957) that Quechan and Maricopa be considered dialects of a single language has not won acceptance (Kendall 1983:9); there may be enough similarity between the two languages to unite them as a subgroup within River Yuman, however.

The Pai branch has occasioned a sustained debate of some importance for Southern California prehistory. An Upland Yuman speech group, including Havasupai, Walapi, and Yavapai communities, has long been recognized. Havasupai and Walapi are evidently dialects of a single language, and Yavapai is either another closely related language, with dialects of its own (Langdon 1977:94), or another subgroup of dialects within a single Upland Yuman language (Kendall 1983). Paipai--separated geographically from the other Pai groups--poses more difficult problems. Kroeber (1943) and Joël (1964) noted resemblances between Paipai and Upland Yuman but attributed them to a common linguistic conservatism rather than to a special genetic relationship. Kroeber tentatively grouped Paipai with Diegueño and Kiliwa in a California Yuman branch, a solution to the question generally rejected now that better linguistic data have become available. Joël proposed Paipai as an independent branch of Yuman. More recent work, however, has supported the Paipai-Upland Yuman genetic link. Winter (1967) went so far as to see Paipai as a dialect specifically of Yavaipai, resulting from a migration from western Arizona and a separation of "probably less than a century" (Winter 1967:376). If accepted, this interpretation would put Paipai entirely outside of the matter of prehistoric relationships. However, other investigators have supported the status of Paipai as a separate language and its coordinate standing with Upland Yuman within the Pai branch (Kendall 1983, Langdon 1978, Langdon and Munro 1980:122).

Cocopa and Diegueño are now generally recognized as forming a Delta-California branch of the Yuman family. Cocopa, together with now-extinct Kahwan and Halyikwamai, was classified as Delta Yuman by Kroeber and others. The meagre surviving evidence on Kahwan and Halyikwamai is not sufficient to establish their identities as distinct languages; it may be just as well to treat them merely as probable dialects or even as non-linguistic social units of the Cocopah. Diegueño has most commonly been split into two dialects, Ipai and Tipai (Luomala 1978), or into three dialects, Ipai, Kumeyaay (or Kamia or Campo), and Tipai (Langdon 1978:94, Kendall 1983). Winter (1957) suggested that Ipai and Campo may be distinct languages rather than dialects, but this view has not been generally followed. Ochoa Zazueta (1979, 1982a) has proposed a somewhat more complex division of Tipai dialects in Baja California into several distinct languages, as is discussed further below.

Finally, Kiliwa poses its particular problems in subgrouping. Kroeber's (1943) lumping of it with Paipai and Diegueño in a California Yuman branch has been generally rejected, and Kiliwa has been recognized as a separate branch in itself. One suggestion has been that Kiliwa is divergent enough from the other Yuman branches to justify a distinction between two daughter-lines descending from proto-Yuman: Kiliwa on the one hand, and Core Yuman, composed of the remaining Yuman branches, on the other (Joël 1964; Langdon 1975:148; Mixco 1975, 1977; Kendall 1983:4). Elsewhere, the Kiliwa branch has been treated as merely coordinate with the Delta-California branch, the River branch, and the Pai branch (Langdon 1978:94, Langdon and Munro 1980). Some other, alternative suggestions for super-branch groupings within Yuman have included setting the Pai branch off as distinct from the other three (Langdon 1978:122), or from the other two (River and Delta-California) after an initial Kiliwa separation (Langdon 1975:148); grouping together Pai and Kiliwa (Webb 1977, cited by Kendall 1983:12); or setting the Pai and River branches against the Delta-California and Kiliwa branches (Kendall 1983:11). Evidently, the matter of subgrouping within the Yuman family at this level is still very much an open issue.

Glottochronology and Lexicostatistics

A scheme subgrouping linguistic phyla and families creates by implication a relative chronology for the successive differentiation and branching off of successively lower-level subgroups. Glottochronology is an attempt to quantify this divergence and link it to an absolute time scale expressed in years before the present. Proponents have argued that empirical evidence from historically-datable linguistic separations shows that for basic vocabulary--semantic categories which are almost universally present in different languages--there is a remarkable constancy in the rate at which one lexical item replaces another, analogous to the constant rate at which carbon-14 decays radioactively. Procedures have been developed involving 100-item and 200-item basic vocabulary lists. In the method, the lists of these items for two languages are compared, and probable matches of cognate forms for a given item on the two lists are counted. With a decay constant derived from the historically-known language separations, the percentage of matches for the two lists is then translated into a date representing the minimum period of separation between the two languages in question. The date is considered a minimum in that it is recognized that divergence is a product not only of the time at which differentiation began but also of the amount of subsequent contact between the two languages; if the subsequent contact were prolonged and intense, glottochronology would be likely to give an overly recent estimate of the separation.

The validity of glottochronology has been intensely debated, and a number of potential problems have been identified (see, e.g., Hymes 1960, Dyen 1975). The existence of any real constancy in the replacement rate for all languages has been challenged. The ways in which the lexical items are collected may raise problems. When only incomplete vocabulary lists are available, inequalities in the replacement rates for individual items in the lists may produce erroneous results. The ways in which probable cognates are recognized on compared lists is not fully consistent or adequate. Such concerns have led some critics to reject glottochronology entirely. A more balanced view is that of Hymes (1960:3):

> It is tempting to think of reasons why glottochronology should not work, and some find it hard to accept the fact that it can. It is tempting for an anthropologist to use even provisional findings of linguistic relationship and time depth, and some find it hard not to accept them uncritically. Either may be unfortunate. Extreme skepticism delays the maturity of glottochronology....Rash use of provisional results may give way to rash disillusionment.

(Glottochronology is sometimes defined as one technique within a larger linguistic subfield of lexicostatistics. The latter, in practice, has been largely limited to the use of the same basic vocabulary lists and the same counting of possible cognates, but involves the use of those results for purposes other than determining absolute chronology. Lexicostatistical comparisons have been used as an alternative to the standard comparative method in proposing remote genetic relationships and in evaluating genetic subgrouping schemes.)

A number of glottochronological datings have been put forward which are relevant to Southern California's prehistory. Problems exist with these dates, particularly because the data on which they are based have often not been published and the techniques used in determining probable cognates have generally not been made explicit and have not been critically evaluated. Nonetheless, these results are worth considering, if viewed with a proper skepticism.

As noted above, Swadesh, who devised the modern methods of glottochronology, was particularly interested in relatively remote genetic relationships (Swadesh 1964, 1967). He also emphasized the "mesh" principle of gradual dialect divergence and borrowing which limits the applicability of a strict genetic model. Languages which could be linked by chains of relationships which Swadesh dated glottochronologically at less than 50 minimum centuries of separation have been grouped by him into eleven very large super-phyla worldwide. One such super-phylum, called Macro-Maya, would embrace the Uto-Aztecan and Hokan groups, as well as most of the other languages of aboriginal North and Middle America. Still other links, generally dated at between 50 and 70 minimum centuries, would complete the worldwide linkage. However, internal connections between some of the pairs of languages within a given super-phylum might be more remote than the closest links across phylum boundaries; thus, for instance, Macro-Maya had internal separations between member languages of up to 96 minimum centuries. It would seem that at such time depths as these, the problems of lexical borrowing and of false cognate recognition tend to swamp the true genetic signals. The method of glottochronology is not yet sufficiently well defined for dates of separation to be taken very seriously, even as provisional estimates, for very remote linguistic chronology.

The next level of chronological interest for Southern California's prehistory is that of the divergence of the Hokan phlylum. Figure 7 extracts some glottochronological figures from Swadesh (1967:104).

Washo						
36	Esselen					
96	36	Salinan				
63	36	35	Chumash			
50	32	43	88	Maricopa		
67	43	42	69	38	Seri	
163	124	47	49	64	102	Guaycura

Figure 7. Glottochronological Measures for Certain Languages (after Swadesh 1967)

(All glottochronological figures reproduced here are expressed in minimum centuries before the present, to facilitate comparisons. Some of the original sources express these statistics in years, to four decimal positions, a precision which seems clearly exaggerated.) These statistics are all based on incomplete lexical lists, particularly in the cases of Guaycura and Esselen. Moreover, very liberal standards for recognizing probable cognates have been applied. The results do not seem to be very consistent with any genetic model of subgrouping within the phylum; their value as chronological indicators is probably not great.

The use of glottochronology studies to date the stages of Uto-Aztecan divergence may be a more realistic goal; certainly, many more prehistorians have may use of these estimates. Hale (1958), Swadesh (1963, 1964), and Goss (1968) have all published results of glottochronological counts for various Uto-Aztecan languages.

Hale (1958) has made glottochronological counts for 17 Uto-Aztecan languages. The figures seem to suggest a minimum time depth for the family of about 5,000 years, although Hale himself favored a 4,000-year estimate. The primary division suggested by Hale's statistics is between the Aztecan languages and all the rest; a secondary division between Sonoran and Shoshonean groups could be put at about 4,000 years ago. Within the Shoshonean division, Numic, Tubatulabal, Takic, and Hopi would all be coordinate branches, with a time depth of about 3,000 years. Hale's figures for the northern (Shoshonean) Uto-Aztecans are summarized in Figure 8.

Northern Pauite							
14	Shoshone						
10	4	Comanche					
13	15	10	Ute				
17	12	11	6	Southern Paiute			
30	22	23	26	26	Tubatulabal		
32	30	26	34	28	22	Cahuilla	
30	27	25	29	27	29	29	Hopi

Figure 8. Glottochronological Measures for Northern Uto-Aztecans (after Hale 1958)

Swadesh (1962, 1964) has published the results of 435 binary lexicostatistical comparisons for 30 languages or dialects of the Uto-Aztecan and Kiowa-Tanoan families. Such an extensive series is particularly useful for its inevitable anomalies and apparent inconsistencies, which suggest something of the limitations on the precision and accuracy of the method. Swadesh's figures also show notable inconsistencies in detail with those of Hale. Beyond this, the results are also of value for their indications of genetic subgrouping and for their suggestions of an absolute chronology.

Within the Uto-Aztecan family, Swadesh's figures give some support to an initial division of five branches: Numic, Tubatulabal, Takic, Hopi, and Southern (including Sonoran and Aztecan). Glottochronological distances between languages of different branches are typically in the range of 30 to 50 minimum centuries. A reasonable estimate for the time of breakup of proto-Uto-Aztecan would be 5,000 years ago. Separations within the Southern branch commonly range up to about 30 minimum centuries, suggesting 3,000 years ago for the expansion of this branch. Takic shows a similar internal variation, also suggesting 3000 B.P. as the date of its expansion. Numic displays much less diversity, with only 14 minimum centuries of divergence.

Swadesh's counts for various Takic languages also suggest an insight into possible subgroupings. The inclusion of Gabrielino and Fernandeño within a Cupan group is not supported; instead, these languages would better fit either within the Serran group or as a separate group of their own, coordinate with Serran and with Cupan or even coordinate with Serran-plus-Cupan, acording to the lexicostatistical evidence.

(A portion of Swadesh's chart relating to the northern Uto-Aztecan languages is shown in Figure 9. Fernandeño, Gabrielino, and Cahuilla figures are based on word lists with only 51 to 75 of the 100 items recorded.)

Monache										
11	Shoshone									
10	2	Comanche								
12	14	13	Pauite							
33	30	30	28	Tubatulabal						
41	42	44	34	34	Fernandeño					
46	42	39	39	39	10	Gabrielino				
31	35	32	26	28	23	25	Serrano			
33	40	33	33	33	32	26	24	Cahuilla		
36	39	37	34	29	30	30	24	24	Luiseño	
31	35	30	30	32	41	44	27	33	31	Hopi

Figure 9. Glottochronological Measures for Northern Uto-Aztecans (after Swadesh 1962, 1964)

Goss' glottochronological work covers only the northern branches of Uto-Aztecan, with particular emphasis on the Numic branch. His results are partially summarized in Figure 10. These results suggest a minimum divergence time of 3,500 years for the northern Uto-Aztecans and a divergence of 1,500 to 2,000 years within the Numic branch. Goss' estimates agree more closely with those of Hale than with those of Swadesh.

Paviotso									
8	Mono								
10	10	Comanche							
14	11	3	Shoshone						
13	19	10	15	Southern Ute					
17	16	12	13	4	Kaibab				
30	34	26	26	29	29	Tubatulabal			
--	31	32	35	--	26	28	Serrano		
32	--	33	30	34	28	22	--	Cahuilla	
30	31	27	31	29	29	30	27	29	Hopi

Figure 10. Glottochronological Measures for Northern Uto-Aztecans (after Goss 1968)

Application of the technique of glottochronology to some Yuman languages has been attempted by Robles Uribe (1964) and by Ochoa Zazueta (1982a, 1982b). The value of both studies for understanding the overall chronology of Yuman divergence is somewhat limited by the fact that they are exclusively confined to those Yuman languages spoken in Mexico. On the other hand, both have the singular merit of presenting in some detail the linguistic data on which they are based, thus permitting some independent evaluation and re-analysis.

Robles' study is based on word lists for five Yuman languages (or languages and dialects): Kiliwa, Paipai, Cocopa, Ku'ahl, and "Cochimi." Ku'ahl is a language or dialect of the Diegueño group, spoken by Indians living in close association with the Paipai around Santa Catarina, Baja

California. The language or dialect which is termed "Cochimi" by Robles and by Ochoa, among others, also belongs to the Diegueño group and is spoken around La Huerta, near Ensenada. Ochoa has explicitly supposed 1982b:34-35) a linguistic relationship between the speech of La Huerta and the now-extinct Cochimi language family of central Baja California. In fact, there seems to be no evidence for such a relationship apart from the shared name of "Cochimi"; this name may recall an early historical link between the two groups, but that link is evidently not linguistic. To reduce possible confusion, the "Cochimi" of Robles and Ochoa will here be referred to as "Huerteño."

For the five languages or dialects, Robles collected the standard 200-word lists, but used only the items for the 100-word lists in his analysis. Moreover, he included only those 74 items which were available to him for all five languages. This procedure presumably has made the different language pairings more comparable in that all are related to the same 74 items, but on the other hand, it has also presumably reduced the value of the individual estimates of absolute chronology in that fewer of words were used for these estimates than were available. As Ochoa has also pointed out, Robles' work was somewhat marred by arithmetical carelessness. Robles' glottochronological results are shown in Figure 11.

Cocopa				
17	Ku'ahl			
16	11	Huerteño		
21	13	19	Paipai	
27	21	21	19	Kiliwa

Figure 11. Glottochronological Measures for Baja California Yumans (after Rob[illegible] Uribe 1964)

Ochoa has collected 200-item basic vocabulary lists for five Baja California languages also: Kiliwa, Paipai, Cocopa, Huerteño, and "K'miai." The last of these, spoken in the Ensenada and Tecate areas, will here be relabelled "Tipai," following what would appear to be the common practice of linguists north of the border and in order to keep open the question of a possible distinction between this speech community and the one sometimes labelled "Kumeyaay" farther north. Ochoa computed glottochronological dates based on both the 100-word and the 200-word list, shown in Figure 12. (The 200-word results shown here are those based on a retention constant of 80.5%.)

Cocopa				
20	Tipai			
25	14	Huerteño		
33	31	35	Paipai	
49	42	45	35	Kiliwa

Cocopa				
17	Tipai			
20	10	Huerteño		
28	27	29	Paipai	
42	39	42	48	Kiliwa

Figure 12. Glottochronological Measures for Baja California Yumans, Based on 100-word (left) and 200-word (right) Lists (after Ochoa Zazueta 1982a, 1982b)

In making an evaluation of Robles' and Ochoa's results, the usual problems with glottochronology and lexicostatistics must be kept in mind. In particular, the manner in which probable cognates have been recognized seems to be a problem. Neither writer is explicit about his methodology in this matter, but evidently both used criteria of phonetic similarity in the lexical items, without attempt to reconstruct proto-forms from which the presumed cognates would have been derived. This is indicated by the fact that for a single lexical item in three compared languages, form "A" may be counted as being cognate with form "B," and "B" as cognate "C," while "A" and "C" are counted as non-cognates. Inspection of Robles' and Ochoa's lists suggests that both scholars, and Ochoa in particular, were very conservative in recognizing cognates. Since the results of glottochronology are properly an estimate of "minimum centuries of separation," it would seem that an error over-estimating rather than under-estimating the number of cognates would be more appropriate. Also, some of Ochoa's decisions on recognizing or denying cognation appear unfounded and probably represent typographical errors.

With these considerations in mind, a new evaluation of this Yuman data has been attempted here. For convenience, consideration has been limited to the 100-item lists. For Kiliwa, Paipai, Cocopa, and Huerteño, both Robles and Ochoa's lists have been used; when the forms on the two different lists disagreed as to cognation, fractions of 1/2, 1/4, or 3/4 were scored for the item. Robles' Ku'ahl list and Ochoa's Tipai list were also used, as was a list published by Swadesh (1967:105-115) for Maricopa. The evaluation of potential cognates was done in a liberal manner, more likely to over-estimate than to under-estimate the percentage of cognates. Known patterns of phonetic alternation were considered (e.g., Wares 1968). However, no attempt was made to use phonetic shifts to weed out cases of later borrowing; here again a resulting over-estimate of cognates and therefore an under-estimation of time depth is likely. The results of this reevaluation are shown in Figure 13.

Maricopa						
15	Cocopa					
17	10	Ku'ahl				
14	11	9	Tipai			
15	12	12	8	Huerteño		
14	18	13	16	17	Paipai	
20	28	22	20	23	22	Kiliwa

Figure 13. Glottochronological Measures for Yumans

Some comments about genetic subgroupings are warranted by these lexicostatistical results. The separate status of Kiliwa as against the Core Yuman languages is generally supported. The coordinate status of the Pai, River, and Delta-California groups is also supported. The reality of the Delta-California group is strongly supported against Joël's (1964) hypothesis of separate status for Delta and Diegueño groups coordinate with Pai and with River Yuman. Within the Delta-California group, there is notably little support for a Diegueño group set off from Cocopa; internal Diegueño differences seem to be nearly as marked as Diegueño-Cocopa differences. The proposals of Robles and Ochoa for recognizing the variation within Diegueño as representing true languages rather than merely dialects is also generally supported.

On the matter of chronology, the available, conflicting figures do not justify any great precision, but they do suggest a framework of minimal time depths. Proto-Yuman evidently began its separation into pre-Kiliwa and proto-Core Yuman more than 2,500 years ago. The division of proto-Core Yuman into its branches may have begun some 1,700 to 2,000 years ago. Divisions within the Delta-California branch have been under way for at least 1,200 years.

Migration Theory and Language Spread

The linguistic "family trees" derived from the genetic model of linguistic relationships, combined with ethnographic information on the protohistoric locations of communities speaking the descendant languages, permit inferences about earlier linguistic distributions, including the "homelands" of proto-languages and movements from those homelands. Some comments on this problem have been offered by Sapir (1916:76-83) and Voeglin (1958), and aspects of it have been further elaborated and formalized by Dyen (1975:50-74) and summarized by Diebold (1960).

As developed most rigorously by Dyen, this subject has been named "Migration Theory." This theory will not be reviewed in detail here, but its application to the problems of Southern California's prehistory will be considered. The theory may perhaps be considered overly schematic, levelling a variety of complex factors into such simple categories as "moves" or "continuous" and "discontinuous" distributions. It has the merits, however, of clarity and objectivity and may provide a useful first approach to deciding difficult culture-historical issues.

Dyen's approach is probabilistic and is based primarily on a principle of parsimony in explanation, a "Postulate of Least Moves": "The probabilities of different reconstructed language migrations are in an inverse relation to the number of reconstructed language movements that each requires" (Dyen 1975:54). One conclusion derived from this postulate is "the principles of the highest order of diversity": "The principle is that the homeland of a family is probably in the area of the highest order of diversity" (Dyen 1975:72), meaning that a region containing languages representing more than one early branching of a family is more probable as a homeland for the family than a region having diversity only with respect to later sub-branchings.

A major limitation on the usefulness of Migration Theory for the present purposes is that it only addresses the interpretation of language distributions which are discontinuous, that is, in which contact between the territories of related languages is interrupted by a natural barrier, such as a stretch of ocean, or by the presence of territory occupied by other, unrelated or more distantly related languages. "Migration" in Dyen's terminology only refers to movements which produce such discontinuities, either by a language community moving across such a gap or else by an intrusion splitting previously contiguous groups. The case of continuous language spreading and of differentiation within unbroken chains of related languages is not addressed. Thus Migration Theory as formalized by Dyen is not directly relevant to the questions of the homelands of the Chumash, Takic, and Yuman groups, each of which groups occupied a continuous territory.

Partial exception to this generalization about territorial continuity exists in the cases of the islands off the Southern California coast. Unfortunately, linguistic data on the aboriginal inhabitants of these islands are rather meagre, but the usual classifications of the languages may still be considered. The Cruzeño Chumash language has been considered to be coordinate--equal in branching time depth--either with Central Chumash within a Southern division of the family, or else with both Central Chumash and Northern (Obispeño), as one of three primary divisions of the family. In either case, a mainland origin for the family is (not surprisingly) favored by the principle of highest order of diversity. If the primary split in proto-Chumash was twofold, between Obispeño and Southern Chumash, then both of these divisions are represented on the mainland and only one of them on the islands; a single "move" could account for a mainland-to-islands migration, but two "moves" would be needed for the reverse. If the primary division was threefold, again a single move from the mainland is favored over two moves from the islands. It should be noted that this hypothesis is not favored merely because there are more languages on the mainland than on the islands; had the migration occurred from the islands to the mainland, it is not improbable that the islands would have maintained their linguistic unity because of small territorial and population size, while the mainland offshoot would have been able to expand territorially and to diversify itself linguistically. The evidence for this would have been that the mainland languages should all form a coordinate group as opposed to Isleño--which, however, is not the generally accepted interpretation of the evidence available. In a similar case, Nicoleño, which was evidently Uto-Aztecan and possibly a dialect of Gabrielino or a sister language of it, would be favored as a product of later migration from the much more diversified mainland.

At the other extreme of scale and linguistic time depth, the applicability of Migration Theory to the Uto-Aztecan and Hokan groups may be briefly considered. Uto-Aztecan is nearly continuous in its distribution. One notable interruption occurs between the northern Uto-Aztecan territories and those in southern Arizona and Sonora. Without going into a formal analysis of the problem, it is clear that this discontinuity is most parsimoniously explained either by a northwest-to-southeast migration of Southern Uto-Aztecans (if these are a genetic unit) or else by intrusive Athapaskan migrations, and perhaps also by intrusive Yuman migrations. In the case of the discontinuous distribution of Aztecan languages in Middle America, migrations by the Aztecans themselves are favored, but this has no reference to Southern California's prehistory.

Hokan has a notably more complex and fragmented distribution, which would seem to be a favorable case for the application of Migration Theory. Unfortunately, the linguistic relationships between the families are so remote, as noted above, that it is difficult either to establish valid subgroupings of the families or else to establish their coordinate status. It is perhaps sufficient to say that the generally-accepted explanation for the disrupted Hokan distribution as being caused by Penutian and Uto-Aztecan intrusive migrations seems compatible with the likely conclusions of Migration Theory. Some of the Hokan subgroupings which have been proposed, however, would require more complex explanations. Subgroups of Seri-Chumash-Salinan on the one hand and Esselen-Yuman on the other would require one or more migration (in Dyen's sense) within the Hokan group. (The discontinuities produced by Uto-Aztecan "wedges" in Southern California and in northwestern Sonora can be taken as "given" in evaluating competing hypotheses, since they require separate "moves" on other grounds in any case.) If Seri, Chumash, and Salinan are all coordinate groups within one subphylum and Esselen and Yuman-Cochimi are similarly related within another, the most probable hypothesis according to Migration Theory would be a single migration of proto-Yuman-Cochimi speakers from north to south, losing their connection with Esselen and splitting Chumash and Seri. Other hypotheses are of course possible but would involve more than one move. While this is an interesting application of the theory, it should be pointed out, as has been discussed above, that the subgroupings on which it is based do not seem to be favored by current linguistic opinion.

Another application of Migration Theory concerns a spatial discontinuity within the proposed Pai branch of the Yuman family. The Upland Yumans of western Arizona (Havasupai, Walapai, Yavapai) are separated geographically from the Paipai of northern Baja California by other Yumans (Diegueño, Cocopa, Quechan). Three general hypotheses are available in Migration Theory to account for this distribution: a movement from western Arizona to northern Baja California by Paipai, a movement in the opposite direction by Upland Yuman, and an intrusive migration by the groups now separating the two areas. Evaluation of these hypotheses in turn depends on other proposed subgroupings within the Yuman family.

To take the intrusion hypothesis first, this would seem to require a minimum of two moves: by Delta (Cocopa) and River (Quechan) Yumans, by Diegueño and River Yuamns, or by Delta Yumaus and Uto-Aztecans (Papago), depending on the location of the original connecting corridor hypothesized between the Paipai and Yavapai. If River and either Delta or Diegueño were considered parts of a genetically coordinate language group as against the Pai group, a single move could be proposed to explain the intrusive pattern, followed by differentiation in place of the intruders. However, most linguisitic opinion does not currently support such a subgrouping, and the intrusion hypothesis must be assigned a low-probability rating of two moves. (Dyen 1975:63 treats all intrusions as single-move hypotheses, but adequate justification for such a rule is not offered, nor apparent.)

Deciding the probable direction of migration hinges on the classification of the Pai languages. If each of the three Upland groups has a separate language coordinate with Paipai, or if Yavapai is coordinate with

Paipai in a subgroup as against the other two, then a north-to-south movement is clearly favored, involving only one move as against two or three moves for the opposite direction. However, if Paipai is coordinate with the Upland group collectively, then neither hypothesis is preferred, each involving only a single move. The presumed greater internal diversification of the Upland group as compared with Paipai has no bearing on this issue if that diversity is only at a lower level of genetic branching, i.e., subsequent to the Paipai-Upland split. The Upland Yumans occupied a large territory in which linguistic fission would be expectable, while the small Paipai territory would probably not have permitted such divisions.

It is worth noting that Migration Theory makes explicit the fact that, given coordinate status for Paipai and the Upland group, no reason exists for preferring a north-to-south migration over a south-to-north migration. It has previously been more or less taken for granted that the former was more likely in this case. (Other, non-linguistic, ethnographic evidence possibly bearing on hypothetical Paipai movements exists in accounts of historic movements to and from the Colorado River Delta; see, for instance, Kroeber 1920 and Kelly 1977.)

As noted, much of the linguistic prehistory of Southern California is not addressable by Migration Theory as formalized by Dyen because the groups in question are not discontinuously distributed. Dyen evidently has good reason in restricting the scope of his theory, in that no such discrete, quantifiable units as "moves" are evident for the description of uninterrupted language distributions. Nevertheless, it would seem that culture-historical inferences can be at least tentatively advanced on the basis of considerations of orders of diversity, similar to those in Migration Theory. Sapir's method for the reconstruction of linguistic centers of dispersion was applied to uninterrupted distributions in the cases of Algonkin and Eskimo-Aleut languages, and others have implicitly applied similar criteria in other cases.

In the region bordering Southern California, the problem of language spread which has received the most scholarly attention is that of the Numic occupation of the Great Basin. Within the Numic group, three subgroups (Western, Central, and Southern) are present, and these subgroups show remarkably little internal differentiation, given their geographical scope. It has been noted that the territories of these three subgroups form a fan-like pattern, radiating out from a common center in southeastern California. At least two and perhaps all three of the subgroups of Numic have been divided into two languages, and in each case the territory of the language closer to the center in California is relatively compact, while that of the northern or eastern member of the pair of languages occupies a larger area in the Great Basin. Thus, linguistic diversity within these subgroups, as well as within the Numic family as a whole, is greatest in the southwestern corner, around the general Owens Valley area. At a still higher genetic level, this same area is a focus of diversity because of the meeting there of the Numic, Tubatulabal, and Takic branches of Uto-Aztecan. The apparent implications of this patterning for the location of the proto-Numic homeland in this area of maximum genetic diversity has been rejected by Kroeber (1925) and by Taylor (1961), but most other students of the problem have found the evidence convincing (Lamb 1958, Hopkins 1965, Miller 1966, Jacobsen 1966, Goss 1968). Evidence from the reconstructed proto-culture also supports this view, as will be discussed below.

The same principle of looking for the linguistic homeland of a group at its point of contact with coordinate groups and/or at the area of its greatest, highest-order internal differentiation, can also be applied to the other cases of uninterrupted language distributions. For the Takic branch, the patterns of contacts with coordinate groups points toward the previously-mentioned area where Takic, Tubatulabal, and Numic meet, whereas the Serran-Cupan or the Serran-Cupan-Gabrielino internal boundary is farther south, although still inland from the coast. The patterns of internal divisions for the Chumash and for lower-level Takic subgroups seem unilluminating; in any case, it is probable that the analytical principle being applied here would lose much of its force as the geographical scale becomes so small.

Yuman, on the other hand, is an interesting case. The most common assumption in the past has been that Yuman expansion occurred from a homeland on the lower Colorado River (see, for example, Bull 1977:52). This area is literally a "center of gravity" for the Yuman territories; it also seems to be an area of considerable linguistic diversity within the family.

Other, non-linguistic factors have probably also encouraged this view. The lower Colorado River area is the one of greatest Yuman prehistoric population density (Hicks 1963), which supports the notion that its "ample environment" provided "the surplus population necessary for so great an expansion" as that of the Yumans (Bull 1977:72). Archaeologically, it also seems evident that certain key technological traits, notably pottery-making and perhaps also agriculture, which were associated with the Yumans known historically in coastal Southern California and northern Baja California, should have reached those areas from the east, from the lower Colorado River area. It is natural to link the technological spreads and the linguistic spread together as a single population movement. However, these non-linguistic arguments may well be based on faulty chronologies and inappropriate environmental assumptions. The spread of ceramic technology and the possible spread of agriculture are likely to have occurred only within the last 1,000 to 1,500 years, whereas the internal diversity of the Yuman family suggests that the linguistic expansion occurred earlier, as was discussed above. Diffusion of these technological traits across other linguistic boundaries in this general region is a well-attested phenomenon. As for the environmental richness of the lower Colorado River area, that may also be an anachronism. The richness was based at least in considerable part on the potential of floodplain agriculture to complement other subsistence resources. In earlier, proto-Yuman times, before the agricultural option was available, it seems not unlikely that the closely-spaced, varied resources of coast, foothills, and mountains in northwestern Baja California and western San Diego County would have been more attractive and more productive than the resources of the lower Colorado River.

To return to the linguistic argument, it should be reiterated that, following Sapir's principles and their elaboration by Dyen, the criterion of diversity as an indicator of a likely homeland refers not to the density of different languages or dialects with respect to area but rather the proximity of highest-level subgroup boundaries. By this criterion, the likely homelands for both proto-Yuman-Cochimi and for proto-Yuman are clearly in northern Baja California rather than elsewhere. The boundaries between the Yuman and Cochimi families and between the major divisions of Yuman--Kiliwa and Core Yuman--are both within a short distance of each other. The situation is less clear for the differentiation of Core Yuman subsequent to the separation of Kiliwa, but diversity at this genetic level, too, is probably as high in this same region as it is in any other portion of the Yuman area.

On a lower level, the case of the Delta-California branch is more problematical. If coordinate status is accepted for Cocopa and the Diegueño group of languages or dialects, a boundary of maximum diversity is well to the east of the mountains, near the Colorado River Delta. If coordinate status with Cocopa is recognized for Ipai, Kumeyaay, Tipai, Huerteño, and Ku'ahl, a western homeland is perhaps more probable. However, as has been suggested above, it is questionable whether the method of inferring patterns of language spread can be legitimately extended to distinctions on such a small geographical scale. These doubts are reinforced by an awareness of the probable secondary ethnic dislocations which must have attended the unstable history of Lake Cahuilla subsequent to Delta-California differentiation.

Linguistic Borrowing

Non-genetic linguistic relationships are also important for the reconstruction of linguistic prehistory in general and of the direction and timing of language movements in particular. Diffusion of a linguistic item from one language to another implies some fairly strong direct or indirect connection between the two languages, usually including geographical proximity. Areal features in phonology and syntax have been recognized for the regions of North America (Sherzer 1976), but for the present purposes the diffusion of individual lexical items seems to offer a more useful index of prehistoric contacts. If a full set of proto-languages and a chronology of sound shifts were available, it would be possible to date lexical borrowings and hence previous contact situations. Even with the limited reconstructions actually available, indications of lexical diffusion may be helpful when they point to patterns of contact which differ notably from those known for the early historic period.

The problem of the relations between Paipai and Upland Yuman is a case which may potentially be clarified with this technique. Winter (1967) has argued for significantly greater lexical similarity between Paipai and Yavapai than between Paipai and Walapai-Havasupai, and he has suggested that this reflects a closer genetic relationship between the first two, which in turn supports the hypothesis of a migration of Paipai from Arizona to Baja California, as discussed above. If Winter's view of the genetic relationships is not upheld, however--if Paipai, Yavapai, and Walapai-Havasupai are all coordinate or if Paipai is coordinate with the Upland group--then particular lexical similarities for Paipai and Yavapai would suggest close contact between those two languages after the initial stage of differentiation within the Pai group. By the principle of parsimony in migrations, such contact would be more likely to have occurred in Arizona, before a Paipai migration to Baja California, rather than in the later region followed by multiple migrations to Arizona. Winter has also noted an instance of lexical similarity between Paipai and Maricopa, a similarity not shared with Havasupai, Walapai, or Maricopa's near relative Mohave (nor, it could be added, with Cocopa or Ipai). Clearly, a single lexical item is too meagre a basis for even provisional conclusions, but if a pattern of borrowing between Paipai and Maricopa were established, this would also strongly support the hypothesis of a Paipai migration from Arizona subsequent to the initial differentiation of the Pai branch.

Another important case of linguistic borrowing and its bearing of prehistoric linguistic geography has been reported by Klar (1977) and Shaul (1982). Klar, studying the Chumash languages, has found evidence of a "high degree of linguistic interaction" between Uto-Aztecan and Chumash (Klar 1977:164). The contacts are judged to be relatively old and to have included Obispeño, the Chumash language farthest removed from the Uto-Aztecan area at the time of European contact. Klar proposes that Uto-Aztecans once occupied at least part of the San Joaquin valley and were in contact with the northern Chumash there until a southward expansion of Yokuts split the two groups. Shaul has reviewed some of the structural features of Esselen, a Hokan language of the central California coast, and has found significant resemblances with Uto-Aztecan, from which it is inferred that these two groups too were "in long and intimate contact" (Shaul 1982:209). Shaul also cites work by Turner pointing toward early borrowing between Salinan and Uto-Aztecan, specifically involving the Numic and Takic branches of the latter. If these studies are valid, important consequences for interpreting earlier distributions of Hokan families and of Uto-Aztecan branches are indicated.

An ambitious use of lexical borrowing in the reconstruction of Southern California's prehistory has been made by William and Marcia Bright (Bright 1976). They have attempted to integrate archaeological evidence for coastal Southern California with lexical information on proto-Uto-Aztecan, Gabrielino, Luiseño, Chumash, and Ipai to show that the Southern California coastal area was probably occupied by non-Hokan (and also non-Uto-Aztecan) speakers during the period from about 5000 B.C. down to the arrival of the Takic speakers. The archaeological aspects of the argument need not be considered here, but an evaluation of the linguistic evidence is appropriate.

The Brights take as their starting point a set of 171 lexical items reconstructed for proto-Uto-Aztecan by Voegelin, Voegelin, and Hale (1962). This list is narrowed to 116 items for which there are available enough corresponding forms in the lexical data on southern California languages. The data are presented in six columns: the English meaning of the item, the proto-Uto-Aztecan form, and, when they are available, the forms for

Gabrielino (90 items), Luiseño (114 items), Chumash (mostly Ventureno; 112 items), and Ipai (113 items). Cognates are apparently proposed on the basis of general phonetic similarity, not rigorously established correspondences. Regrettably, in the article the forms considered to be cognates are not identified; however, statistical generalizations about them are offered. Forty eight percent of the Gabrielino forms (43 forms) are considered relatable to the proto-Uto-Aztecan forms, and 53% of the Luiseño forms (60 forms) are relatable to proto-Uto-Aztecan ones. The Brights then consider possible borrowings from Chumash and Ipai to account for the residue of Gabrielino and Luiseño forms apparently not derived from proto-Uto-Aztecan, and only six possible matches are found, amounting to about 4% of each of the two lists. They conclude:

> Since a large number of words in Gabriellino and Luiseno do not derive from either Proto-Uto-Aztecan or from Hokan borrowings, it seems likely that the Uto-Aztecan speakers encountered other Indians speaking a language (or languages) now presumably extinct, and that they lived in the same area long enough to take over a lot of vocabulary (Bright 1976:202).

Several aspects of the Brights' analysis may be challenged, however. First, the conclusion that the posited coastal predecessors of the Takic speakers would have to have been non-Hokans is not warranted by the data presented. The lexical forms for Chumash and Ipai show few resemblances to each other. If a third Hokan family had been the source of the proposed borrowings in the Takic languages, these borrowings would still likely not be identifiable as being from a Hokan language, because of the distance of the relationships involved and the meagre reconstruction of proto-Hokan achieved so far. At most, the Brights' data suggest that the source of the borrowings was non-Chumash and non-Yuman.

A potentially more serious problem with the Brights' analysis concerns the adequacy of their comparisons between proto-Uto-Aztecan and the Luiseno and Gabrielino forms. Evidently, only a single proto-Uto-Aztecan form is considered for each item on the lists, and possible semantic shifts are not considered. The Brights themselves note that the statistics "must be considered as only approximate, since the data are not complete" (Bright 1976: 199). Using a more extensive list of proto-Uto-Aztecan reconstructions which has become available (Miller 1967), and also looking for possible simple semantic shifts, the lists of the Brights have been reevaluated, and more Uto-Aztecan cognates for the Takic languages have been recognized. For instance, proto-Uto-Aztecan *a$_{\upsilon}$ŋa and Gabrielino -máaša-n for "wing" clearly do not match, but Miller (1967:63) also has reconstructed proto-Uto-Aztecan *masa for "wing," with cognates also in Serrano, Hopi, Tarahumara, Varahio, Mayo, Yaqui, and Cora. For "hill," proto-Uto-Aztecan *to$_{\upsilon}$no and Luiseño qawíi-ča do not match, but Miller also reconstructs "mountain" in proto-Uto-Aztecan as *kawi. These and similar instances clearly do not require explanation as external borrowings. Recalculating the statistics with this additional information, 53 out of 90 Gabrielino forms (61%) and 77 out of 114 Luiseño forms (68%) may be counted as derived from proto-Uto-Aztecan, or roughly two-thirds rather than roughly half of the lists. Of the six forms cited by the Brights as possible borrowings from Chumash or Ipai, two are also now better explainable as derived from proto-Uto-Aztecan.

After this re-analysis, there remain 37 forms each for Gabrielino and Luiseño which are not yet explainable as being derived from proto-Uto-Aztecan, and at least 34 which also cannot be explained as Chumash or Ipai. This residue, although reduced, still needs to be considered. It is not unlikely that further instances of forms with proto-Uto-Aztecan origins are still to be discovered. One way to evaluate this possibility would be to apply this same list of 116 items of a number of other Uto-Aztecan languages. If other Uto-Aztecan languages show unexplained residues of similar size, it may be either than the residues represent unreconstructed portions of the proto-Uto-Aztecan heritage, or else, less probably, that they represent uniform rates of borrowing from outside. If some of the languages show much smaller unexplained residues, it is likely that the Gabrielino and Luiseño residues do indeed represent lexical borrowing to a considerable degree.

Another way to evaluate the probable size of the externally borrowed and the proto-Uto-Aztecan-derived components of the unexplained residue is to try to estimate the rate at which semantic shifts and replacement by synonyms within the proto-Uto-Aztecan-derived vocabulary have been occurring. If this rate is high, then the whole residue may represent forms of proto-Uto-Aztecan dervlation; if low, borrowing is much more likely. To judge this rate by taking the percentage of the terms now identifiable as proto-Uto-Aztecan-derived which are not cognate with the terms of the Brights' list of proto-Uto-Aztecan terms would not be appropriate for two reasons. First, while the Gabrielino and Luiseño list may be taken to include the forms which are the most common or most exact equivalents available for the English list meanings, no such presumption applies to the proto-Uto-Aztecan forms, which have no doubt been reconstructed opportunistically and then given their English equivalents, even though the proto-Uto-Aztecan form may not represent the most standard counterpart of the English item; ignoring this fact could result in an overestimate of the amount of semantic change or synonym replacement since proto-Uto-Aztecan times. Second, the number of replacements of proto-Uto-Aztecan-derived forms by other proto-Uto-Aztecan-derived forms is not known, because presumably not all proto-Uto-Aztecan-derived forms have as yet been identified as such; ignoring this fact could result in an underestimate of the rate of change and replacement.

A more satisfactory estimate of the rate of change and replacement may be deduced for a later period only, the time between the period when the Takic branch was still unified and the modern period. The reasoning behind this estimate is as follows. Historically, the Gabrielino and Luiseño speakers occupied territories at a considerable distance from the territories occupied by any non-Takic but still Uto-Aztecan speaking peoples (e.g., Tubatulabals, Numic speakers, etc.). From this, it is fair to assume that most or all of the lexical forms in Gabrielino and Luiseño which are of proto-Uto-Aztecan origin were transmitted to these languages through the proto-Takic stage, i.e., that they are also proto-Takic forms. In the Brights' lists, some 87 of the English items have corresponding forms which are identifiable as proto-Uto-Aztecan-derived (and therefore also proto-Takic-derived) in the Gabrielino list (54 forms) or in the Luiseño list (78 forms) or in both lists (45 forms). Of the 78 forms in

Luiseño derived from proto-Uto-Aztecan, 61 are items for which some Gabrielino form is also recorded; of these 61 Gabrielino forms, 45 are proto-Uto-Aztecan-derived and 16 belong to the unexplained residue (or are Chumash). Of the 54 Gabrielino forms from proto-Uto-Aztecan, 53 are items for which a Luiseño form is also recorded; 45 of those Luiseno forms are proto-Uto-Aztecan-derived and 8 are unexplained or are from Ipai. These figures suggest that for the items known to have had proto-Uto-Aztecan-derived forms in proto-Takic, 15 to 25% have been replaced in one or the other of these two daughter languages. Of the 45 items having proto-Uto-Aztecan-derived (and proto-Takic-derived) forms in both languages, however, each member of the pair of forms is derived from the same proto-Uto-Aztecan cognate; none show shifts from one proto-Uto-Aztecan form, attested in one of the languages, to another proto-Uto-Aztecan form attested in the other language. This strongly suggests that the replacement of one proto-Takic-derived term by another has been very uncommon. If this is valid, then on the Brights' lists in the case of an item which is represented in either the Gabrielino or the Luiseño list by a proto-Uto-Aztecan-derived form but in the other list by a form of unknown derivation, this second form is unlikely to turn out to be proto-Uto-Aztecan-derived; if the forms on both lists are of unknown derivation, and if they are not cognates, it is unlikely that more than one of them will turn out to be proto-Uto-Aztecan-derived. This reasoning means that at least 18% of the forms on the two lists, and 50% of the unexplained residue, should genuinely not be proto-Uto-Aztecan-derived but rather, probably, borrowed from external sources.

If the residues do represent external borrowing, and if, as seems likely, there was no extensive borrowing from either Chumash or Ipai, other surviving sources may still be explored before appeal is made to some now-extinct sources. Some preliminary inspections of other potential lexical sources, however, seem to offer little hope of explaining many of the residual Gabrielino or Luiseño forms. Words lists of varying quality and completeness have been scanned from proto-Yuman and other Yuman languages, Cochimi, Guaycura, Seri, Salinan, Esselen, and Yokuts. A very few possible additional matches with Yuman languages and with Salinan were found. This informal inspection of lists is clearly not definitive, but it does suggest that if extensive lexical borrowing into Luiseño and Gabrielino did occur, the source family or families are probably now extinct, as the Brights hypothesized. As to the higher-level affiliation of those sources, it is not unlikely on geographical grounds that they belonged to the Hokan phylum, but specific lexical evidence for or against this hypothesis is not available.

One other aspect of this residue analysis offers promise: attempting to determine the cirucmstances and the timing of the Takic expansion into coastal Southern California. More specifically, if there has been substantial linguistic borrowing from the region's prior inhabitants, whose language or languages subsequently became extinct, it may be possible to infer whether that borrowing was done by still-unified Takic or Cupan groups, or whether the borrowing was done by groups already fragmented or fragmenting into their historic communities.

As has been argued above, in the Brights' lists for Gabrielino and Luiseño, there are no known instances of two different proto-Uto-Aztecan-derived forms for an item being represented on the two lists. It has been suggested that this is evidence for relative stability of the the vocabularies as against internal shifts since proto-Takic times, although evidence for instability as against presumably external sources is quite evident: 44% of the Luiseño-Gabrielino pairs are apparent non-cognates, indicating substantial change of some sort since proto-Takic times. Therefore, if much borrowing occurred into the proto-Takic language, one would expect to find a significant number of non-proto-Uto-Aztecan forms which are cognate in the two lists, since these borrowings into proto-Takic should show a stability comparable to the stability of the proto-Uto-Aztecan-derived forms in proto-Takic. In actual fact, of the items which have apparently non-Uto-Aztecan forms in both lists, in only about 25% of the cases do the Luiseño and Gabrielino forms appear to be cognate. Unrecognized forms from proto-Uto-Aztecan could easily account for all of these cognate pairs. Thus, there is no compelling evidence in these lists of any external borrowing into proto-Takic, but a strong suggestion of such borrowing into at least some of its daughter languages. This supports the hypothesis of the fragmentation of the Takic community before or during its expansion to coastal Southern California and a prolonged period of interaction between its daughter languages and the earlier non-Uto-Aztecan languages of that region.

The Evidence of Proto-Culture

Linguistic studies based on the genetic model, in addition to revealing broad patterns of family relationships among the languages studied, also reconstruct specific lexical items from the ancestral proto-languages. These reconstructions have semantic implications; if a form for a given item such as "pottery" can be reconstructed for the proto-language, this implies that this semantic category, or one close to it, was also present in the proto-language and in the experience of its speakers. This technique for reconstructing a proto-culture is particularly relevant to the present inquiry with respect to semantic categories which may be environmental or chronological indicators.

There are major potential pitfalls in this technique, however. Parallel semantic shifts are not unlikely in daughter languages which are exposed to new environmental or cultural circumstances; this may result in misleading semantic implications being given to otherwise properly reconstructed forms for the proto-language. Borrowing of later lexical innovations among daughter languages in these circumstances is also highly probable, and distinguishing such diffused items from true proto-language categories may be difficult.

An attempt to reconstruct the culture associated with proto-Yuman and to draw some culture-historical inferences from this reconstruction has been made by Howard W. Law (1961). Unfortunately, shortcomings in the data available and, above all, in Law's methods of analysis, have made the substantive conclusions in his article highly dubious. Nonetheless, it is worth following some of Law's analysis to review some of the pitfalls and potential merits of the method.

A first point to be noted is that Law did not have lexical data from Kiliwa to use in his reconstructions of proto-Yuman vocabulary. Therefore, in terms of the subgrouping scheme favored here, his efforts properly apply to a later stage of linguistic evolution, to proto-Core Yuman rather than to proto-Yuman. However, many of his reconstructions could also now be extended to proto-Yuman, using available Kiliwa data.

Law's article presents 106 lexical items with cognate forms in two or more of the Yuman languages which he considered, including Havasupai, Walapai, Yavapai, Mohave, Quechan, Maricopa, Cocopa, Ipai, and Huerteño. These languages are arranged in subgroupings corresponding to the usual classification of Pai, River, and Delta-California branches (although there is some confusion in language identifications indicated in Law 1961:46). The 106 sets of possible cognates are then ranked from "first order validity" through "fifth order validity"; evaluation depends on the number of different subgroups represented (all sets having at least two), the linguistic proximity of those subgroups, and whether or not regular sound shifts are in evidence in the sets. The details of this rating system need not be considered here, and in any case are not entirely clear in the article.

Law notes that some of the strictly linguistic data used in these ratings is suspect and that corrections should be made accordingly on the basis of external evidence. "Horse," for instance, earns first-order validity, with cognate forms in Yavapai, Mohave, Maricopa, Quechan, and Cocopa, yet it is certainly unlikely that this category belonged to proto-Core Yuman; as Law notes, the cognate forms may be related to forms for "deer" and/or "dog." (A similar case which might be adduced is "cow," with cognates in Havasupai, Walapai, Mohave, Maricopa, Cocopa, Huerteño, Paipai, and Kiliwa; see Wares 1968:81). Other items could receive upgraded ratings because of their semantic connection with higher-ranking items; "bow" is third-order and "cornmeal" is fourth-order, whereas "arrow" and "corn" are first-order.

Law's 106 items are categorized as referring to animals and birds (33 items), agriculture and food (17), wild plants (12), clothing and shelter (6), tools (7), weapons (4), religion (6), and miscellaneous items (21). Proto-Core Yuman forms are not reconstructed, although phonological correspondences are discussed.

The methods used to transform reconstructed lexical categories into inferences about prehistoric environment and culture are not made very explicit by Law, and are open to some question. These reconstructions may be worth considering in some detail. According to Law, "It is apparent that the Yuman-speaking peoples lived in much the same area as they do today." This seems a likely enough conclusion; it may be based on the fact that a sizeable vocabulary of fauna and flora is reconstructed. A more productive way of analyzing this data, however, would be to look at what regions may be excluded as potential homelands by reason of lacking these environmental characteristics.

Law says of the proto-Core Yumans that they "were probably concerned with hunting animals as well as with developing agriculture." The practice of hunting need not be questioned, since all of the ethnographically known

Yumans hunted to varying degrees. The question of agriculture in proto-Core Yuman times is more controversial, however. Cognate items relevant to this question are "bean," "corn," "cotton," and "squash," all of first order validity; "bean flour" and "cornmeal," of fourth order validity; and "blackeyed peas," of fifth order validity. The last of these, "blackeyed peas," is attested in six languages representing all three branches, but these forms show great phonetic uniformity, suggesting late diffusion, and in fact blackeyed peas were a Spanish-period introduction (Kelly 1977:30). "Bean flour" and "cornmeal," represented only in Havasupai and Maricopa, are easily analyzable as "bean" and "corn" plus a second cognate morpheme which does not correspond to "flour" but may be something similar; these two items do little to strengthen the case for proto-Core Yuman agriculture. Of the four items of first-order validity, three, "corn," "bean," and "squash," have been examined more closely by Joël (1978). "Bean" is traced to a probable borrowing from Hopi, "corn" is derived from "seed," and "squash" is unexplained. It is worth noting that "corn" and "squash" also have cognate forms in Kiliwa, implying a still earlier origin if they are indeed genuine retained cognates. These Kiliwa forms, and also forms for "corn," "bean," "squash," and "cotton" in Paipai and in the various Diegueño languages west of the mountains, represent agricultural terms among the Yuman peoples least likely to have been involved with such practices prehistorically. It seems evident that the possibility of very late diffusion rather than true proto-Yuman and proto-Core Yuman origins should not be discounted at present.

Law describes a hypothetical proto-Core Yuman homeland:

> The region was dry, dusty, and windy, but not without water. The natural phenomena of rain, sun, moon, stars, and constellations were important to the people. Snow and ice were also known to some degree. The center of the occupied area might have been closer to the ocean, and in lowlands as opposed to the highlands and plateau area, than the present day people are living. The main area was probably the lower Colorado River area rather than the northern Arizona area (Law 1961:55).

Some environmental characteristics might be inferred from reconstructed animal and plant terms--"jackrabbit" and "mesquite" perhaps suggesting dryness, for example. In using this method of reasoning, however, it must be kept in mind that categories with cognate forms present in the daughter languages describe not only the environment of the proto-language but also that of all those daughter languages which have these forms. This implies that individual cognate items which are found widely distributed in the daughter languages are very poorly suited to discriminate between the territories of those daughter languages as possible proto-language homelands. It is only when an item is reconstructable for the proto-language but is not present in some of the daughter languages because of environmental reasons that the area of possible homelands is narrowed. Law's geographical proposals do not appear to be based on any such limitations in distributions.

Another fallacy in Law's method seems to be the equation of the presence with the importance of reconstructed semantic categories. The reconstruction of proto-Core Yuman forms for "dirt (dust, earth)," "wind," "water," "rain," "sun," "moon," "star," and so forth do not establish for the proto-culture a dusty, windy environment, nor a special importance for rain, sun, moon, and stars; such categories are virtually universal and culture-free.

A proto-form for "ocean" would indicate an awareness of this phenomenon in the proto-culture, but since cognate forms are attested throughout the historic Yuman area, including in Walapai, no particular proximity of the homeland to the ocean is supported; no items for marine or littoral fauna or flora are reconstructed (not surprisingly, given the distance from shorelines of two of the three daughter branches--River Yuman, and Pai, which is represented in Law's sample basically by Upland dialects). Moreover, the forms for "sea (ocean)" which are presented have as their common, cognate element a morpheme analyzable as "water"; to this morpheme, several of the languages attach a second morpheme evidently cognate with "salt" or "salty" (Wares 1968:91). The reconstructed item itself, then, is questionable, as are Law's environmental conclusions drawn from that reconstruction.

In addition to supposed indicators of the proto-language's homeland, Law reconstructs items having some chronological implications. Agriculture has been discussed already. "Metate," "mortar," "pestle," "bow," and "arrow" are interesting, although caution is suggested by Law's insufficient screening for semantic shifts and for the diffusion of lexical forms, along with the physical items themselves, into the daughter languages.

The reconstruction of proto-culture as a clue to the location and dating of a proto-language has also been attempted for proto-Uto-Aztecan by A. K. Romney (1957) and Catherine S. Fowler (1983) and for proto-Numic by Fowler (1972). Romney's published paper is primarily a methodological prospectus and contains only a little specific evidence on proto-Uto-Aztecan. He argues on the basis of lexical reconstructions that in proto-Uto-Aztecan times the bow and arrow were used and agriculture was practiced, but that pottery was apparently absent. Romney's specific claims, however, have been effectively refuted by Miller (1966:94-102), who shows that some of the reconstructed items are more probably the result of later borrowing, and that others probably represent parallel semantic shifts in the daughter languages, as from "atlatl" to "bow."

Romney's advocacy of southern Arizona as the likely homeland for proto-Uto-Aztecan is important, because he seems to be the primary source of this hypothesis, which has enjoyed something of a presumption in its favor among later students of the problem (Lamb 1958, Goss 1966, Fowler 1972). Romney reconstructed some plant and animal terms which may have influenced his homeland selection, but this argument is not made explicit; Fowler's use of this material will be discussed below. The claim of agriculture for the proto-Uto-Aztecans was probably another factor in favor of the southern Arizona area; it now seeems that this reconstruction is poorly founded and chronologically improbable. Finally, Romney based his proposal on considerations of genetic subgrouping within the Uto-Aztecan family. He asserted that "linguistic evidence is clear in indicating that the Pima-Tepehuan language group is the most divergent within Uto-Aztecan" (Romney 1957:41), a claim which has received no support in more recent studies of Uto-Aztecan subgrouping.

Fowler's (1972, 1983) uses of lexical reconstruction in attempting to delimit probable homelands for proto-Uto-Aztecan and its successors are sounder than the similar efforts by Romney and Law. Fowler's most notable advance has been to consider in some detail the geographical ranges of the plants and animals for which terms are reconstructed in the proto-languages. Unfortunately, the results achieved are fairly modest. Most the ranges are too extensive to effectively narrow down the possible homelands, but some progress is made.

In her earlier article, Fowler (1972) addresses the problem of the homeland from the Numic branch, but draws heavily on evidence from Takic, Tubatulabal, and Hopi, and in effect reconstructs a homeland for proto-Northern Uto-Aztecan, if that is a true genetic unit. The later article (Fowler 1983) concentrates on the homeland for proto-Uto-Aztecan but also supports the existence of Northern and Southern divisions and the earlier conclusions about the homelands for the northern groups.

One restriction which Fowler is able to put on possible proto-Numic, proto-Northern Uto Aztecan, and proto-Uto-Aztecan homelands is that they would appear to have been located south of the northern limit of the "hot deserts," about 36°30' north latitude. In the first article, this conclusion is based on cognate forms for "turtle/tortoise," "chia," "lycium," and "cholla" being present in at least three of the four northern branches of Uto-Aztecan. (Of these items, however, "turtle/tortoise" is attested in Paviotso and in Shoshone, and "lycium" is in Shoshone; these two languages had ranges entirely north of 36°30', so the ranges of at least some of these species were evidently greater than Fowler has acknowledged.) In the second article, the same northern limit is supported by cognate forms for "agave" in Takic, Tubatulabal, Hopi, and a number of southern Uto-Aztecan languages. Generally, Fowler's argument against possible homelands for proto-Uto-Aztecan and its branches being as far north as the northern Great Basin seem persuasive.

Fowler further narrows down the potential homelands by noting the presence of a range of higher-elevation, woodland species as well as desert species in the reconstructed vocabularies. Oak and pinyon distributions are particularly noted. According to Fowler, this ecological range points in particular to two likely homeland areas: the southern Sierra Nevada foothills, and the mountains and foothills of southeastern Arizona and northern Mexico. The first of these is favored as the homeland for proto-Numic, as well as for proto-Takic, Tubatulabal, and Hopi.

In her first article, following Romney, Lamb, and others, Fowler favors a limited southern Arizona-northern Mexico homeland for proto-Uto-Aztecan, with a subsequent migration of the northern Uto-Aztecan branches northwest of the Sierra Nevadas. In the second article, a broader area for interacting proto-Uto-Aztecan dialects is suggested, covering most of Arizona and with a possible but doubtful extension westward into the deserts and mountains of Southern California. Both migration of the Northern Uto-Aztecans to the Sierra Nevadas and their isolation from the southern groups by expanding Yuman speakers are considered as possible mechanisms for the separation of the two groups.

Fowler's preference for Arizona and Sonora as a proto-Uto-Aztecan homeland, as against the southern Sierra Nevada area, seems to rest on a single lexical item, "turkey." Of the distribution of this genus, she reports,

> At present there is no archaeological basis for placing turkeys in the West north of roughly the same 36°30' north latitude boundary across southern California through Colorado.... Even southern California localities would be questionable, since the known natural range of the wild species does not definitely extend west of the San Francisco Peaks in northern Arizona.... However, a rather continuous band of woodland habitat does exist across the upper Mojave Desert to the southern Sierra Nevada near the parallel cited above--if some intervening low-lying basins are discounted (Fowler 1983:232-234).

An added footnote observes that recent research by Rea (1980) suggests that the "modern" turkey may have been introduced into the Southwest as part of a complex of domesticated species from Mesoamerica, and that an earlier species of turkey persisted in the Southwest "until roughly 3300-6600 B.P." (Fowler 1983:233). This poses a chronological problem; if there was a time gap between the periods of the two species of turkeys, one would have to suppose that a form derived from proto-Uto-Aztecan for "turkey" in the northern branches (specifically Numic and Hopi) must have shifted its meaning once to some other semantic category--another bird, or less plausibly a remembrance in myth of the turkey--and then shifted back again when the modern turkey was introduced. Given the time gap, it would seem to be about as simple to suppose instead either that the proto-Uto-Aztecan term had another meaning and was transferred to "turkey" independently in several daughter branches, or else that the cognate forms represent later borrowing rather than derivation from proto-Uto-Aztecan.

Aside from the chronological problem, there is a geographical problem with "turkey" as lexical evidence for the proto-Uto-Aztecan homeland. If turkeys were present uninterruptedly in Arizona and in the southern Great Basin but not in California, and if the forms for "turkey" are retained cognates, then Fowler's Sierra Nevada homeland for proto-Numic lacks plausibility. Cognates for the item are present in Shoshone, Chemehuevi, Southern Paiute, Ute, and Comanche, and clearly the item is to be reconstructed for proto-Numic. If, however, as Fowler hypothesizes, the linguistic ancestors of the Numic speakers moved west from Arizona to the Sierra Nevadas and lived there for some time before their expansion north and east again, and if turkeys were not present in California, the proto-Uto-Aztecan form for "turkey" could only have been preserved in proto-Numic and transmitted to the Great Basin Numic languages through an improbable double sequence of semantic shifts such as those suggested above for the chronological problem. From this argument, it seems likely either that the early range of turkeys did indeed include Southern California or that the terms for this bird are not true retained cognates from the proto-Uto-Aztecans. In either event, the case for preferring a proto-Uto-Aztecan homeland outside of Southern California is not well supported by this line of evidence.

Some Conclusions

The linguistic evidence examined above suggests some conclusions about Southern California's prehistory. As should be clear, any such conclusions are only provisional and probabilistic, at best representing the current balance of the evidence and subject to refinement or revision as more linguistic data and better analyses of that data become available. It is also evident that valid conclusions from linguistic evidence need to be weighed and evaluated in the context of conclusions drawn from other sources of evidence about prehistory, such as archaeology, comparative ethnology, and physical anthropology.

The Proto-Uto-Aztecan Homeland. The location of this homeland has not been definitively determined, but it is proposed here, primarily on the basis of subgrouping schemes and historic language distributions, that the northern fringe of Southern California should be regarded as the leading candidate at present. Proponents of the southern Arizona and northern Sonora region for this distinction have made only a weak case in its behalf. It is suggested here that proposals for a very broad homeland region, as outlined by Fowler (1983) and still more so by Goss (1977), would represent entities in linguistic disequilibrium even under early hunting-gathering conditions, and therefore, would more plausibly relate to the period of the breakup of proto-Uto-Aztecan rather than to the period of its unity.

The Proto-Yuman Homeland. The linguistic evidence, contrary to general past assumptions, seems to favor the mountains or coast of northern Baja California as the most probable homeland for proto-Yuman, rather than the Colorado River valley. The same is also true for the homelands for proto-Yuman-Cochimí and perhaps for proto-Core Yuman as well. The movement of the linguistic ancestors of the Ipai and Kumeyaay into southern San Diego County, if indeed such a movement occurred subsequent to proto-Yuman-Cochimí times, is more likely to have come from south to north, through a fairly homogeneous enviornment, rather than from east to west, from the desert to the coast. Bull's (1983:38) linguistic argument for a late Yuman intrusion in western San Diego County depends on the assumption of a Yuman homeland in the Colorado River valley, an assumption which does not seem to be warranted. True's (1966) thesis of a long-term cultural continuity in the Tipai-Ipai area, against which Bull argues, is still tenable linguistically.

The Shoshonean Wedge. The geographical separation of two Hokan-speaking groups, the Chumash and the Yumans, by a narrow area of Takic ("Southern California Shoshonean") speakers, which is linked to a much broader area of Uto-Aztecan speakers in the Great Basin, has given rise to the phrase "Shoshonean Wedge." The implications of this linguistic-geographical pattern have been variously evaluated.

In an extreme view, the Shoshonean Wedge has been taken to represent an intrusion which separated a previously unified linguistic community which was ancestral to both Chumash and Yuman families. The members of this community presumably spoke either proto-Hokan or a daughter language in a southern Hokan subgroup. Kroeber (1925:579) seems to espouse this view.

An alternative view of the Shoshonean Wedge is that it separated a previous Hokan continuum which, however, was already internally differentiated. Bull's suggestion of a Chumash-Seri continuum seems to be one hypothesis of this type; other possibilities would be a Chumash-Yuman continuum or a continuum of Chumash, Yuman, and another, now-extinct family between these two.

The issues involved in evaluating the first of these hypotheses are primarily chronological, while the issues in the second relate to borrowing, subgrouping, and Migration Theory. Bull (1983:38) argues against the first hypothesis by suggesting that:

> ...the concept of the disruption of a Southern California Hokan group by 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....
>
> The time depth between Yuman and Chumash is apparently of much greater antiquity than the difference between the Luiseñic (Takic) language family and its parent stock. This would, therefore, require a divergence of Yuman and Chumash languages well before the appearance of the Luiseño.

Bull's contrast between the high degree of differentiation between Chumash and Yuman on the one hand and the lesser differentiation between Takic and other Uto-Aztecan branches on the other hand, may be granted, although the geographical discontinuity in the former case as against the geographical continuity in the latter group could be proposed as a source of different rates of divergence. Bull's assumption that the Uto-Aztecan intrusion into coastal Southern California must correspond in time with the emergence of proto-Takic is certainly plausible but not necessary; the emergence of proto-Takic could well have been earlier, and it could also have been later. A possible (but perhaps unlikely) hypothesis would be that the "Shoshonean Wedge" was in fact a proto-Uto-Aztecan intrusion, and that eastward expansion and differentiation of that family occurred subsequently; with this hypothesis, the chronological problem of the early Chumash-Yuman divergence would disappear. The probability, however, seems to favor Bull's view.

Bright (1976) argues against the hypothesis of a continuum of Hokan daughter languages in coastal Southern California prior to the Uto-Aztecan intrusion there, on the basis of evidence for borrowing into Gabrielino and Luiseño. As has been proposed above, this body of evidence, insofar as it may be valid at all, has no force against the hypothesis of a Hokan continuum from Chumash to another Hokan group to Yuman-Cochimi, because the presumed borrowed elements cannot at present be shown to be non-Hokan. Even in the more restricted sense of an argument against Chumash-Yuman-Cochimi continuum, the interpretation is precariously dependent upon the assumption that the borrowings were made during and after the intrusion. The presence of possible Salinan cognates among the forms in question raises the possibility that borrowing might have occurred before the intrusion, perhaps into already-differentiating Takic languages in the southern San Joaquin valley.

Bull's hypothesis that the Uto-Aztecan intrusion separated Chumash and Seri groups poses serious problems in regard to the identity of the proto-Yuman and proto-Yuman-Cochimi homelands and in regard to the subsequent movements of the Seri. The main motivation from such a hypothesis--the supposed existence of a genetic subgroup within Hokan including Chumash and Seri but excluding Yuman--now appears dubious.

A Model for Southern California's Linguistic Prehistory. In light of the uncertainties in the evidence and in the conclusions, some may feel that it is best at this stage to describe Southern California's linguistic prehistory in only the most general terms, in order to avoid initiating or perpetuating potentially erroneous or misleading views. That approach to the subject is not favored here. Certainly, it is crucially important to be able to distinguish what is securely known, what is probable, and what is purely speculative. However, framing the discussion solely in terms of the first of these categories tends to make that discussion excessively inductive and relatively sterile. More concrete, detailed models or hypotheses provide a framework within which implications can be drawn out and tested and the models can be refuted or refined. In that spirit, one possible chronological-geographical model for Southern California's linguistic prehistory is outlined below and in Figure 14.

pre-4000 B.C. -- Hokan speakers settle the coastal and montane portions of central and southern California and northern Baja California. Divergence begins of Esselen, Salinan, Chumash, Yuman-Cochimi, Seri, and other groups. Proto-Uto-Aztecan speakers occupy a limited area in the vicinity of the southern Sierra Nevada mountains.

3000 B.C. -- Southern Uto-Aztecan speakers break off and move southeast toward Arizona and Sonora. Other Uto-Aztecan speakers expand their territory, including occupation of the southern San Joaquin valley, and begin diversification into Numic, Tubatulabal, Takic, and Hopi branches.

1000 B.C. -- Yokuts speakers expand southward in the San Joaquin valley. Takic speakers expand southward to coastal Southern California, displacing and absorbing unknown Hokan groups, and beginning internal differentiation. Yuman speakers in northern Baja California expand northward, dividing into Kiliwa and Core Yuman groups.

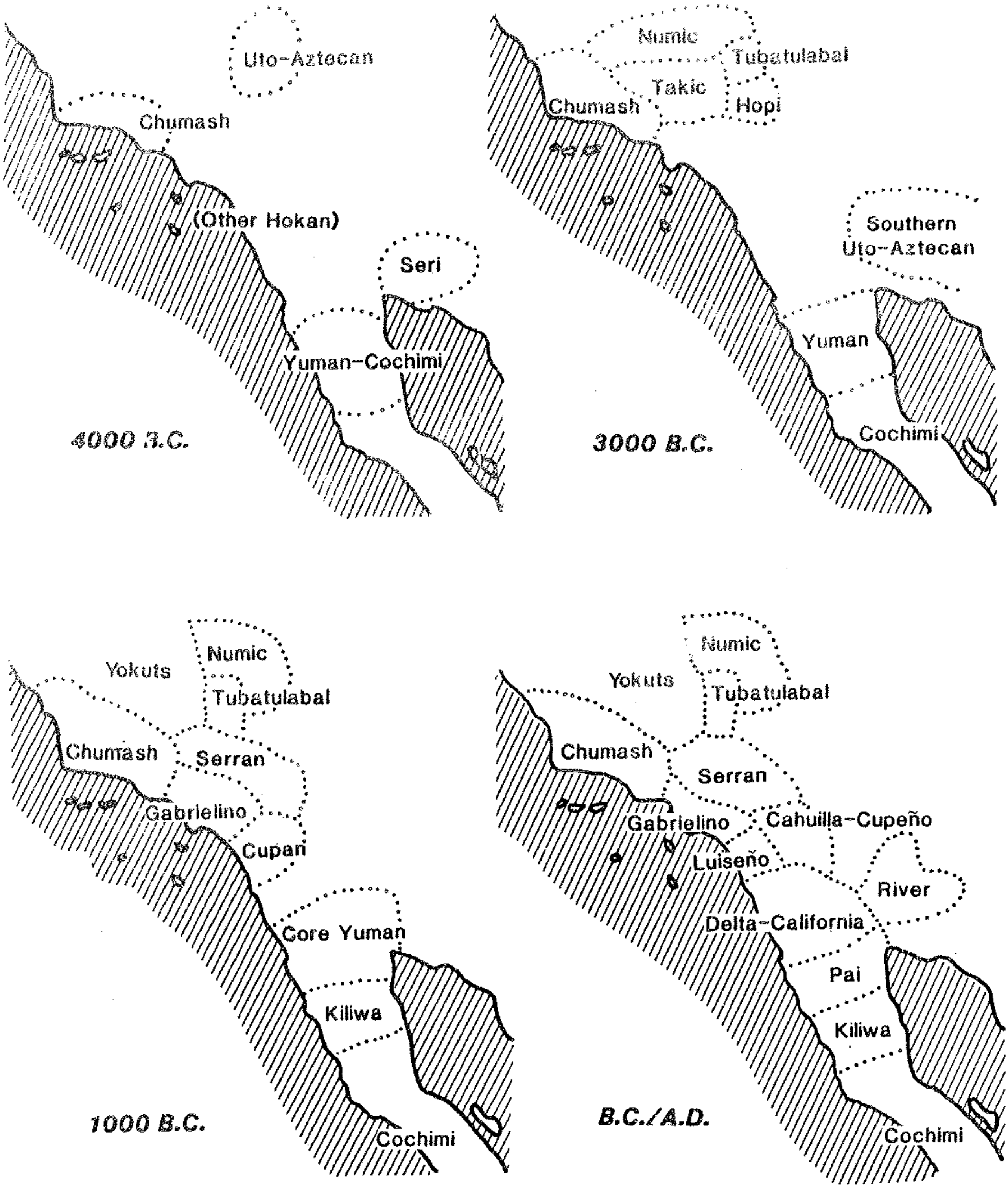

Figure 14. Map of hypothetical stages in Southern California's prehistoric linguistic evolution.

B.C./A.D. -- Further expansion of Core Yuman speakers northward, differentiating into Pai, Delta-Calfiornia, and River groups.

A.D. 500 -- Differentiation of Delta-California languages begins. Pai groups splits by the migration of Upland group to western Arizona. River Yumans expand up the lower Colorado River and Gila River. Expansion of western, central, and southern Numic divisions into the Great Basin begins.

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SUMMARY OF THE SDCAS INVESTIGATIONS AT CA-SDI-5589, BONSALL, CALIFORNIA

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INTRODUCTION

CA-SDi-5589 was subject to test and salvage excavations between 1973 and 1977, conducted by volunteers from the San Diego County Archaeological Society. As neither a preliminary nor final report was ever produced, the following discussion provides a summary of field techniques and results, based upon field notes and a partial catalogue archived by the Society. For purposes of this summary, estimates of total number of some classes of material (e.g., ceramics, flakes, bone, shell) were generated from a 30 percent sample of the units catalogued, stratified by spatial quadrant and depth.

FIELD METHODS

The initial phase of the SDCAS project was the excavation of 61 1 x 2 meter randomly and judgementally selected units at Locus A, the largest, western-most locus of CA-SDi-5589 (Table 1). The units were hand-excavated in 10 centimeter levels, screened with either 1/4 inch or 1/8 inch mesh, and photographed; any features encountered were photographed and mapped. The excavations were begun in November of 1973 and completed in February of 1974. This first phase was directed by Bob Booher and Dr. Paul Ezell.

A second phase was directed at establishing the boundaries of five loci of the site, and evaluating the degree of disturbance caused by pot-hunters. Site boundaries were established by testing each loci with a posthole digger in a uniform grid pattern. The excavated dirt was screened and samples of the soil at different depths glued to the excavation record sheets. The degree of disturbance from pothunting was tested by hand excavating and screening a 1 x 18 meter trench through a previously pothunted area of Locus B. Work on this phase was conducted in December of 1976 and January of 1977.

Materials recovered from the various test excavation phases were washed, sorted, and partially catalogued between 1974 and 1980. The level of descriptive or analytic treatment of the materials varies by locus and by artifact class: shell has been speciated, but faunal bone has not; flaked lithics from Locus A have been sorted by material and some metric or morphological attributes have been scaled, but flaked lithics from Locus B have not been catalogued. Most importantly, no final unit level summaries were ever produced; and the available documents such as the field notes, catalogue, and computer formatted data files yield slightly different totals or have artifacts or levels missing. Catalogue data for two units at Locus A are apparently missing.

TABLE 1

LISTING OF UNITS EXCAVATED AT LOCUS A

N.E. Quadrant	S.E. Quadrant	N.W. Quadrant	S.W. Quadrant
N_2E_2 0-70 cm.	S_0E_1 0-60 cm.	N_2W_0 0-80 cm.	S_0W_1 0-50 cm.
N_0E_{13} 0-70 cm.	S_0E_{10} 0-80 cm.	N_2W_9 0-70 cm.	S_0W_{12} 0-40 cm.
N_4E_1 0-70 cm.	S_2E_5 0-70 cm.	N_4W_4 0-80 cm.	S_2W_0 0-50 cm.
N_6E_4 0-60 cm.	S_4E_1 0-30 cm.	N_6W_0 0-80 cm.	S_4W_3 0-40 cm.
N_6E_9 0-70 cm.	S_6E_5 0-40 cm.	N_8W_4 0-70 cm.	S_4W_8 0-80 cm.
N_8E_5 0-70 cm.	S_8E_{10} 0-50 cm.	$N_{10}W_9$ 0-60 cm.	S_6W_4 0-70 cm.
$N_{10}E_{12}$ 0-30 cm.	$S_{10}E_5$ 0-40 cm.	$N_{14}W_0$ 0-60 cm.	$S_{10}W_0$ 0-70 cm.
$N_{12}E_{10}$ 0-40 cm.	$S_{12}E_1$ 0-70 cm.	$N_{16}W_4$ 0-50 cm.	$S_{10}W_9$ 0-30 cm.
$N_{12}E_{13}$ 0-30 cm.	$S_{14}E_5$ 0-40 cm.	$N_{18}W_9$ 0-50 cm.	$S_{10}W_{12}$ 0-50 cm.
$N_{14}E_5$ 0-40 cm.	$S_{16}E_{10}$ 0-60 cm.	$N_{22}W_0$ 0-40 cm.	$S_{12}W_9$ 0-70 cm.
$N_{14}E_8$ 0-50 cm.	$S_{18}E_5$ 0-40 cm.	$N_{24}W_4$ 0-40 cm.	$S_{14}W_1$ 0-50 cm.
$N_{14}E_{10}$ 0-30 cm.	$S_{20}E_1$ 0-40 cm.	$N_{26}W_9$ 0-40 cm.	$S_{14}W_4$ 0-70 cm.
$N_{16}E_2$ 0-50 cm.	$S_{22}E_5$ 0-60 cm.		$S_{14}W_7$ 0-50 cm.
$N_{16}E_8$ 0-30 cm.	$S_{24}E_1$ 0-50 cm.		$S_{16}W_7$ 0-60 cm.
$N_{18}E_8$ 0-40 cm.	$S_{24}E_{10}$ 0-60 cm.		$S_{24}W_6$ 0-40 cm.
$N_{18}E_8$ 0-40 cm.			
$N_{26}E_7$ 0-10 cm.			

Descriptive and analytical treatment of the collection are ongoing by SDCAS, who curates the materials. The following summary is preliminary with some totals estimated from samples of units due to gaps in the data base. Comparisons of the results from CA-SDi-5589 with other major excavations are provided as background.

RESULTS

Locus A

Features: Surface features consist of bedrock milling features and rock art. The bedrock milling is described with portable ground stone artifacts, below.

Two rock art features are attributed to this site. The first is a pictograph (located north and west of the midden loci) exhibiting red painted elements in the Rancho Bernardo style, i.e., large geometric designs in rectangular panels set upon large granite boulders. The Rancho Bernardo style may predate the San Luis Rey style, which is attributed to San Luis Rey II/Luiseno period sites (Hedges 1981:166; Ken Hedges personal communication 1984).

The second feature is a petroglyph set on a boulder north and east of the midden loci on the hilltop above. The design element is believed to represent a snake (Ken Hedges pesonal communication 1984).

No subsurface features are formally designated in the catalogue or fieldnotes. However, an areally widespread concentration (excavation units N10 W9, S2 W0, S4 W3, S1 E10, and N6 E9) of thermally altered rock, charcoal and bone occurring between 30 and 40 centimeters is noted in the unit level sheets. The only other occurrences of note are local concentrations of bones or tools, and a boulder with five small mortars found at 30 centimeters

Artifacts

Chipped Stone: Approximately 235 chipped stone tools and utilized flakes were recovered. Of these, about 70 percent are projectile points, knives, drills, blades and fragments; the remainder are cores, scrapers, choppers, hammerstones and utilized flakes. It is estimated that over 10,000 waste flakes and debitage were also recovered. The majority of projectile points are similar to types found at Molpa and Temeku: small, triangular arrow points weighing less than 2 grams, with straight or concave bases, which may be subsumed under the general class of Late Prehistoric Cottonwood Triangular (Lanning 1963), or True's types 1-3 (True et al. 1974:49). Variants include side-notched and serrated forms. One interesting variant is a large (fragmentary) corner-notched obsidian blade 5.5 x 3.5 x .5 centimeters. Corner-notched forms are rarely found in Late Prehistoric sites in the region, and obsidian artifacts this size (projected to be approximately 8 x 5 centimeters) are also rare.

One preliminary regional comparison that can be made with these data is the distribution of material types to finished artifacts. Table 2 compares the material types used for projective points and other tools at the Bonsall Site and Molpa (True et al. 1974) and Temeku (McCown 1955), two other excavated San Luis Rey II/Luiseno habitation sites. While the three samples are not representative in strict statistical terms, they are relatively large excavations each constituting over 50 cubic meters of deposit, and estimated to cover 3-5 percent of the total site area.

By inspection of the table, the Bonsall Site has a higher percentage of cryptocrystalline silicates than the other two, and a lower percentage of quartz. Interestingly, Molpa has a higher percentage of quartz utilizatin than either of the other sites, as do two smaller sites in the same vicinity: CA-SDi-593, 80 percent (Karst in True et al. 1974:14); and CA-SDi-543, 90 percent (Fulmer 1978:113-115). The other locally available materials, collapsed into the category of metavolcanic in the table and representing a variety of porphyritic and fine-grained rhyolites, andesites, and basalts, is equally represented at Bonsall and Temeku, but apparently less utilized at Molpa. Obsidian, which is not available locally, is found in consistent percentages at all three sites.

Obsidian is a trade item, while quartz and metavolcanics are available at various locations in the culture area. The silicates are more problematic, as they may be a) desert trade items; b) foraged locally from river gravels (McCown 1955:27); or c) foraged or traded from nearby coastal sources (Flower, Ike and Roth 1979:144-147). The source of silicates utilized at CA-SDi-5589 cannot be determined at this time. However, studies of the source materials utilized and their frequencies at different site locations within the culture area may provide important data for assessing the kinds and degree of inter- and intra-regional exchange.

Ground Stone: CA-SDi-5589 contains a variety of permanent and portable ground stone features and artifacts. Permanent features, slicks, oval basins, or mortars found on exposed bedrock are scattered across the site. While a complete inventory of these features is not available, the number and complexity of features is relatively modest in comparison to other habitation sites in the culture area. This may be due to a lack of suitable outcrops on site, or to the distribution of food resources and means of exploitation practiced. The most complex of the permanent milling features at the site, located at Locus C, has 11 mortars, 6 oval basins, and 11 slicks on two surface-level exposures (Figure 2). The remaining feature outcrops have either solitary or a few ground surfaces. One unusual feature exposed during the 1973 excavations at Locus A has 5 small (less than 10 centimeters diameter) mortars, which may have been used for pigments or plants for ceremonial occasions.

Portable ground stone artifacts include milling handstones and grinding surfaces (n = 31) of which (27) are fragmentary. All are made of locally available materials, and few exhibit any shaping or other modification other than grinding wear. A ground, shaped discoidal fragment, and a grooved steatite shaft straightener were also recovered.

TABLE 2

MATERIAL TYPES BY PROJECTILE POINTS (P.P.) AND OTHER TOOLS AT THREE MAJOR SAN LUIS REY/LUISENO EXCAVATED SITES

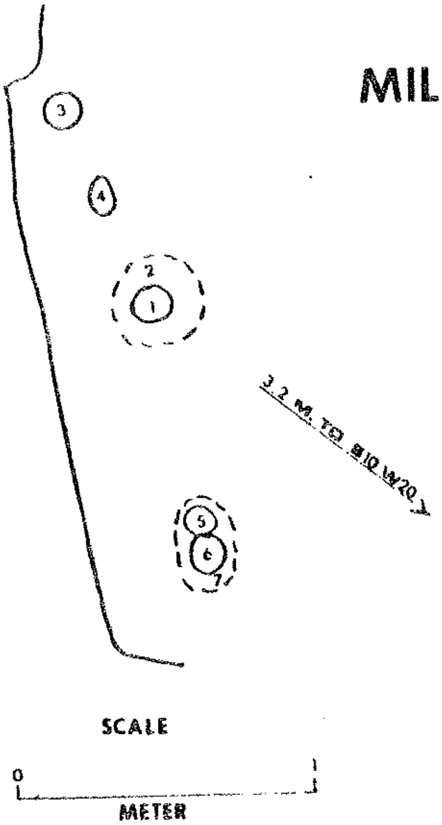
	BONSALL			MOLPA[2]			TEMEKU		
Material Type	Total % of Total	P.P.[1] % of P.P.	Tools % of Tools	Total % of Total	P.P. % of P.P.	Tools % of Tools	Total % of Total	P.P. % of P.P.	Tools[3] % of Tools
Quartz	84 36%	77 45%	7 11%	405 79%	344 81%	61 69%	452 55%	429 57%	23 32%
Meta-Volcanic	74 31%	36 21%	38 58%	45 9%	33 8%	12 13%	242 29%	199 26%	43 59%
Obsidian	11 5%	10 6%	1 0%	19 3%	13 3%	6 7%	27 3%	27 4%	—
Silicates	61 26%	44 26%	17 26%	13 3%	11 3%	2 2%	104 13%	98 13%	6 8%
Other	5 2%	2 1%	3 5%	27 5%	22 5%	5 6%	—	—	—
TOTAL	235	169 71%	66 29%	512	423 83%	89 17%	826	753 91%	73 9%

[1] May include knives and drills.

[2] Excludes knives and drills (addition to P.P. category does not alter % of materials).

[3] Includes only materials classed as "scrapers".

SDi 5589
LOCUS C
MILLING FEATURES



FEATURE A

FEATURE A

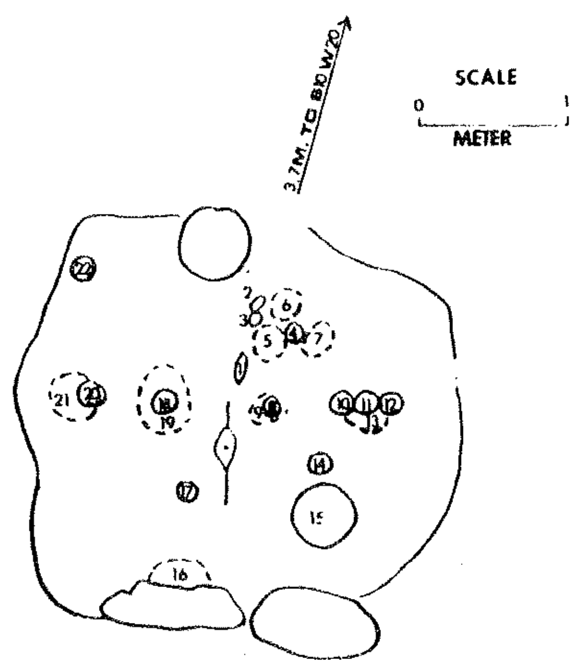
DIMENSIONS OF MILLING AREAS

Number		Length	Width (Centimeters)	Depth
1	Mortar	17	16	8
2	Slick	36	35	–
3	Mortar	20	16	11
4	Basin	19	10	5
5	Mortar	18	18	12
6	Basin	16	13	2
7	Slick	46	30	–

FEATURE B

DIMENSIONS OF MILLING AREAS

Number		Length	Width (Centimeters)	Depth
1	Basin	30	15	10.5
2	Mortar	16	14	7
3	Mortar	10	10	1.5
4	Basin	15	12	2.0
5	Slick	20	13	–
6	Slick	13	11	–
7	Slick	23	17	–
8	Basin	17	14	2.5
9	Slick	27	20	–
10	Mortar	17	16	9
11	Mortar	16	17	7
12	Basin	16	11	2.5
13	Slick	23	12	–
14	Mortar	14	14	3
15	Slick	35	33	–
16	Slick	30	16	–
17	Mortar	17	17	5
18	Mortar	16	16	4
19	Slick	46	27	
20	Mortar	17	15	9
21	Slick	26	24	–



FEATURE B

FIGURE 1

Ceramics: Approximately 2,000 sherds were recovered from Locus A. Of these, 64 or approximately 3 percent are exotic Colorado Buff Wares, 12 are bowpipe fragments, and 3 are effigy or pendant fragments. The remainder is the locally produced Tizon Brown Ware. An unusual orange paint decorated Tizon Brown Ware sherd was recovered. Over 90 percent of the sherds were recovered in the first four levels (0-40 cm.), which constitute only 70 percent of the volume excavated. In comparison 2,700 sherds were recovered at Molpa, for an approximately equal volume excavated; and 6,700 sherds were recovered at Temeku, for approximately twice the volume excavated. Approximately 4 percent of the sherds of Temeku were Colorado Buff Wares.

The extremely low rate of recovery of sherds in the lower portions of the deposit (1 or less per 2m. x 1m. x 10 cm. level) raises the possibility of an early San Luis Rey II or preceramic San Luis Rey I component being present at CA-SD-5589. There is a recorded Pauma site adjacent to Locus A; but no stratigraphic or typological evidence of an earlier component was found at Locus A. The rate of recovery of ceramics is similar to that predicted for San Luis Rey sites by Meighan (1954) and True (1966, 1970 or True et al. 1974), and postulated to represent a late introduction of ceramics by diffusion from Yuman groups to the south, and a slow adoption of their use.

Shell and Bone Artifacts: Thirty-nine shell beads were recovered. These are spire-lopped *Olivella biplicata* available from the Pacific shoreline 14 miles west. The beads were formed by grinding away the top portion of the spire of the shell. Similar forms occur in southern California prior to 2000 BC and persist into the post contact period (King bead chart in Elsasser 1978:48). Ten fragments of shell pendants, manufactured from *Haliotis* spp. are also noted in the catalogue, but not described.

Thirty-four bone awls and antler flakers were also recovered. Awls are piercing implements which were generally produced from the long bones of deer or from wood. They were used for shell and pottery working and in the production of coiled basketry (Drucker 1937:15 and 19). Bone and deer antler flakers were used for pressure retouching in stone tool manufacture (Drucker 1937:15). Sparkman (1908:206) specifies that a piece of deer antler was used in making projectile points. Deer antlers were also utilized as chisels or wedges (Sparkman 1908:210; Kroeber 1925:653; Curtis 1926:159).

Faunal Bone: Approximately 4,000 grams of faunal bone were recovered at Locus A. Only a small portion has been analyzed, and no firm data are available as to percentages of species, or range of species present. Large mammal (primarily deer), small mammal (rabbit, woodrat, and other rodents), avifauna and a few fish vertebrae are known to be present. There are no data for Molpa, and only 49 of 12,000 bone fragments at Temeku were identified; so there are no comparative data available. As noted at Temeku (McCown 1955:35), the recovery rate for bone is more consistent across the site than for other classes of material, although some concentrations were noted in association with thermally altered rock.

Shell: Approximately 300 grams of marine shell were recovered at Locus A. The estimated percentage breakdown of species is Chione 12 percent, Argopecten 12 percent, Donax 53 percent, and Ostrea, Mytilus, Tivela, Haliotis and others 23 percent. Eighty percent of the shell were found in the first four levels. There does not appear to be any significant changes in the ratio of species by depth in the deposit.

There are little comparative data available for inland sites. Neither Temeku nor Molpa have been analyzed or described in detail. Some data are available for smaller samples of other inland sites and coastal sites, however. These are presented in Table 3 along with data from Locus B.

The relatively low gram weight estimate of the total sample of shell from Locus A is a bit misleading in comparison to other sites because it is composed primarily of Donax, a small light shell (.2 grams average), as opposed to Chione, Argopecten or other larger and heavier species. Note that Locus B has over 50 percent Chione and Argopecten, while at Locus A Chione and Argopecten comprise only 22 percent.

There appears to be a pattern of dominance of either Chione and Argopecten on the one hand and Donax on the other at sites in the area. One explanation proposed follows Beaton's (n.d.) model of molluscan response to human predation pressures in arguing that Chione and Argopecten populations, while efficient to exploit, were slow to recover from predation and decreased in availability through time; Donax exploitation would increase as a substitute or supplement as Chione and Argopecten became less available. Hence, sites with Donax were from later time periods. An alternative argument follows from a similar focus on population dynamics of the molluscs: Donax, as it appears in large rapidly growing but transient colonies, may have appeared as a supplement or surplus in shellfish foraging strategies, and thus the rights to its exploitation may not have been as regulated as for other molluscan species. The sporadically appearing Donax colonies may have been exploited by groups that did not normally exploit shellfish to a large extent and, therefore, Donax would appear as a large percentage of the total shell consumed at those sites.

The shellfish component of CA-SDi-5589 exhibits variability that may relate to chronological or functional differences in occupation of the various loci, and provides an indication of the level of coastal foraging or exchange practiced by inland San Luis Rey groups. However, the variability between species percentages at Loci A and B needs to be explored further before summary statements of shellfish exploitation represented at CA-SDi-5589 can be made.

Locus B

Locus B was excavated with an 18 x 1 meter trench segregated into nine 2 x 1 meter units. Much of this area had been previously looted by pothunters, to a depth up to 70 centimeters. SDCAS screened the obviously disturbed portions, but not in separate 10 centimeter levels. Once undisturbed portions were reached, the remainder was divided into 10 centimeter levels. In either case, the deposit was screened with 1/8 inch mesh.

TABLE 3
Percentage Distribution of Shell from San Luis Rey II Sites

COASTAL SITES:	Chione	Argopecten	Donax	Other	Total Amount (in grams)	Volume Excavated
W-137 (Flower, Ike, Roth 1977)	11%	10%	61%	28%	1,836	$30m^3$
W-1256, 1257 (Flower, Ike, Roth 1979)	65%	28%	1%	6%	70,922	$60.6m^3$
INLAND SITES:						
W-1277 (Hatley 1979)	84%	10%	—	6%	383	$6m^3$
W-1278 (Hatley 1979)	56%	38%	—	6%	1,456	$13m^3$
SDi-5589 Locus B	25%	25%	21%	29%	132.3	$16m^3$

The material from Locus B has been only partially catalogued, and much of the data are only described in excavation notes. Many of these do not give quanitified estimates or descriptions of artifacts. Therefore only a brief outline of results is possible.

Features: The linear trench was laid out across a badly disturbed area of cremations. Human bone, primarily teeth, were found in concentrations of charcoal and shell beads, from approximately West 8 to West 18 meters (SDCAS Unit/Level File). The majority of beads were shell disc, with only a few spire-lopped examples. Surprisingly, litle pottery was found even in areas that had not been previously excavated.

Artifacts: The Locus B excavations recovered 5 projectile points, 4 manos and fragments, 3 metates and fragments, 1 hammerstone, 1 bone awl, 3 pipe fragments, human teeth, 150 disc shell beads, over 2,000 grams of faunal bone and human bone, over 1,000 flakes and debitage, over 200 sherds, and 132 grams of shell, primarily Chione, Argopecten, and Donax. Comparison of the recovery rate of small items such as bone, flakes, sherds, and shell between pothunted and undisturbed portions of the deposit did not reveal major differences, although the comparison is facile owing to the state of the data base. Most of the diagnostic artifacts were recovered from disturbed areas, although in lower frequency than at Locus A; the yield of projectile points appears to be especially low.

One interesting aspect of these results is that cremation areas were not found at Locus A. The Locus B sample is small and limited in area, but it does suggest a possible cremation area distinct from the habitation area at Locus A. True (1970:53) suggests that this is more typical of the Kumeyaay, and that such segregation of living and cremation areas is less typical for San Luis Rey/Luiseno groups. The excavation of CA-SDi-5589 is too limited and skewed to one locus to offer this as any more than a tentative observation though.

POSTHOLE TEST LOCI A-E

Five loci of the site were tested for the depth and areal extent of subsurface deposits by means of a rectilinear array of hand-excavated and screened postholes. The testing did not define the boundaries of the entire site nor of all of the loci tested. At Locus A, 35 postholes were excavated at 10 meter intervals in an area 40 x 60 meters; at Locus B, 83 postholes at 5 and 10 meter intervals in an area 45 x 65 meters; at Locus C, 27 postholes at 20 meter intervals in an area 40 x 45 meters; at Locus D, 24 postholes at 10 meter intervals in an area 50 x 50 meters; and Locus E, 20 postholes at 10 and 20 meter intervals at an area 40 x 50 meters. Quantities of shell, ceramics, faunal bone and chipped stone were recovered at each locus. The nature of the testing limits the utility of the specific artifactual counts per posthole or locus and they are not summarized here. Figures 2 to 6 delineate the test pattern and estimates of midden depth.

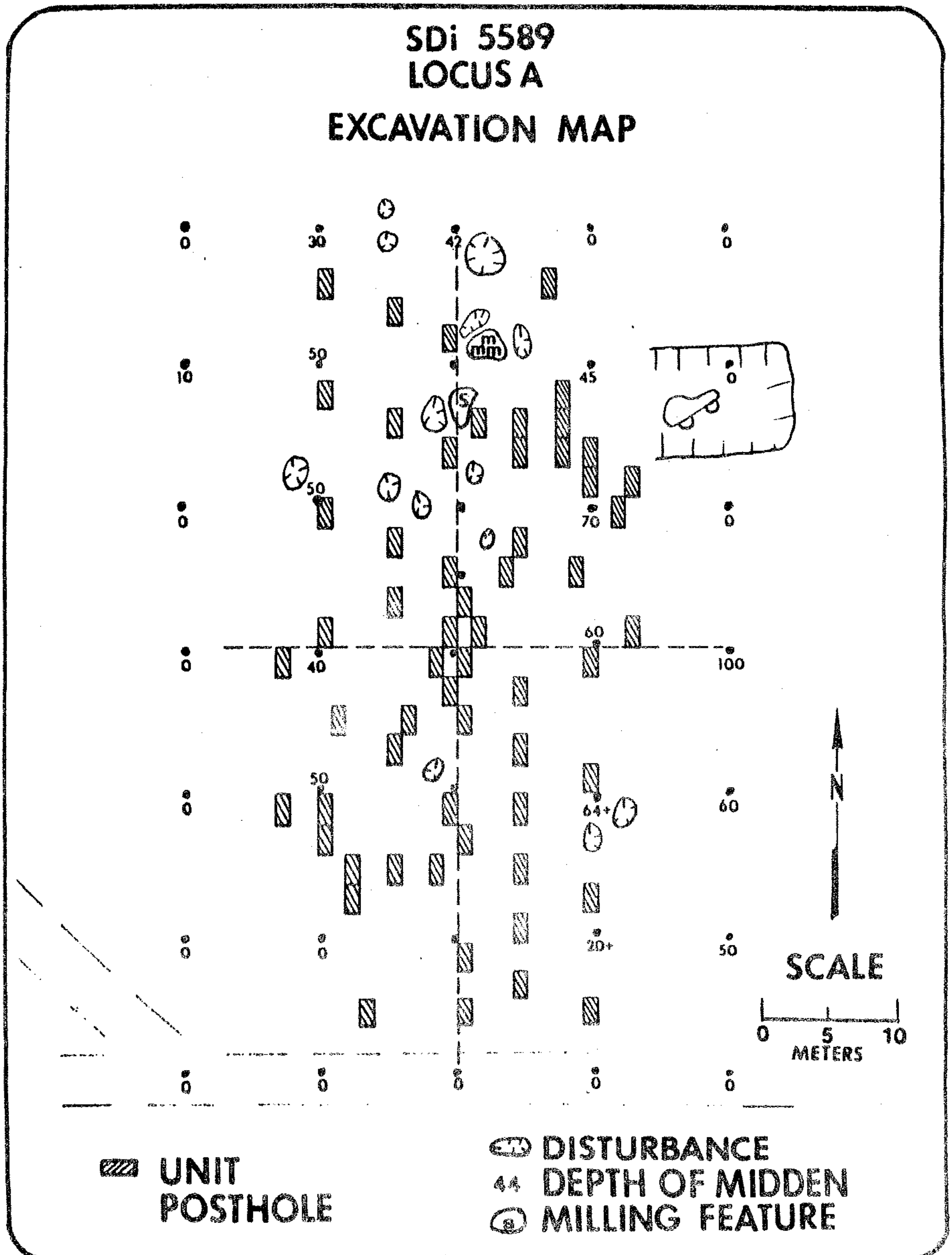
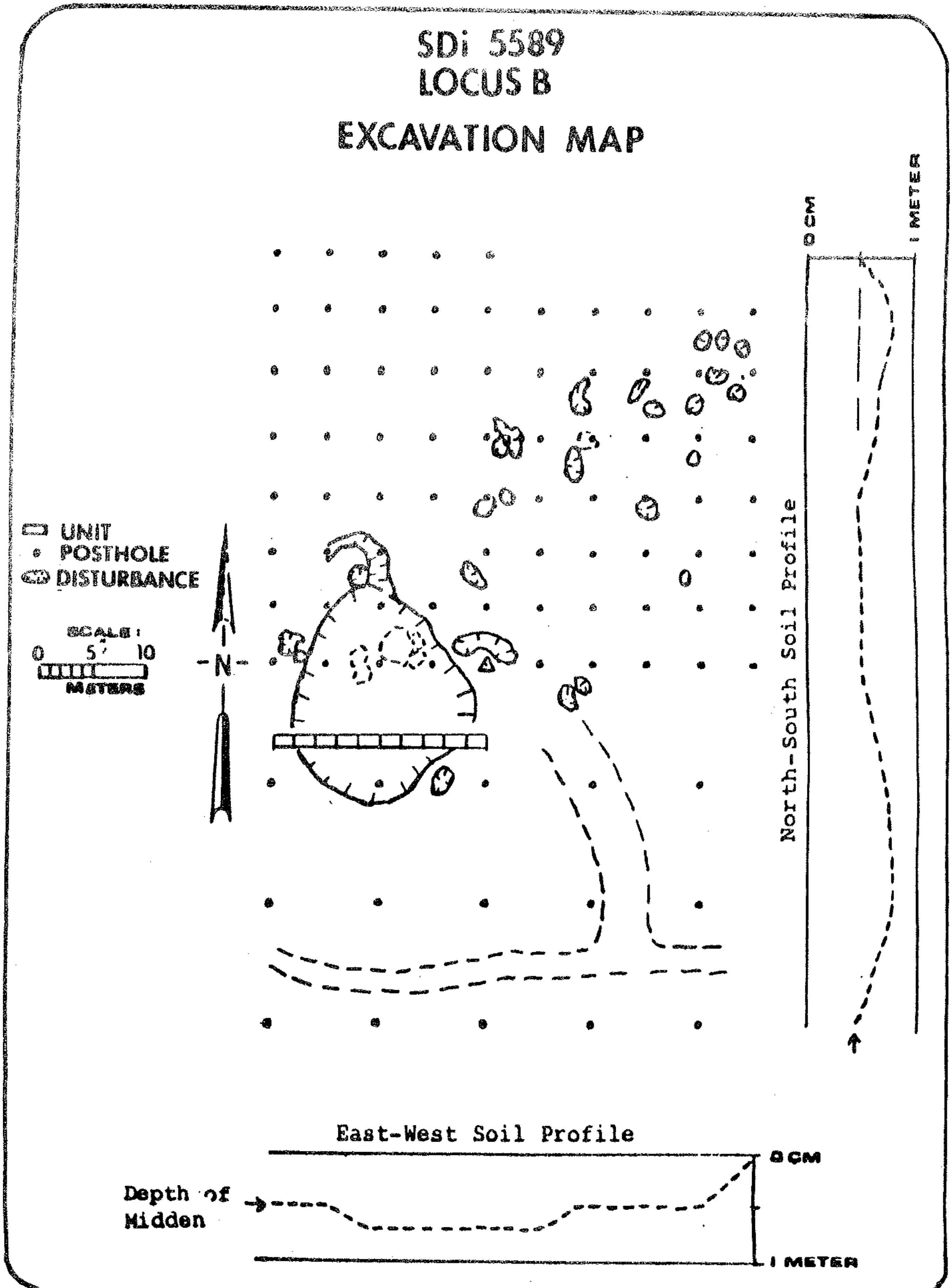

SDi 5589
LOCUS A
EXCAVATION MAP
SCALE
0 5 10
METERS
N
UNIT
POSTHOLE
DISTURBANCE
44 DEPTH OF MIDDEN
MILLING FEATURE

FIGURE 2


SDi 5589
LOCUS B
EXCAVATION MAP
UNIT
POSTHOLE
DISTURBANCE
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METERS
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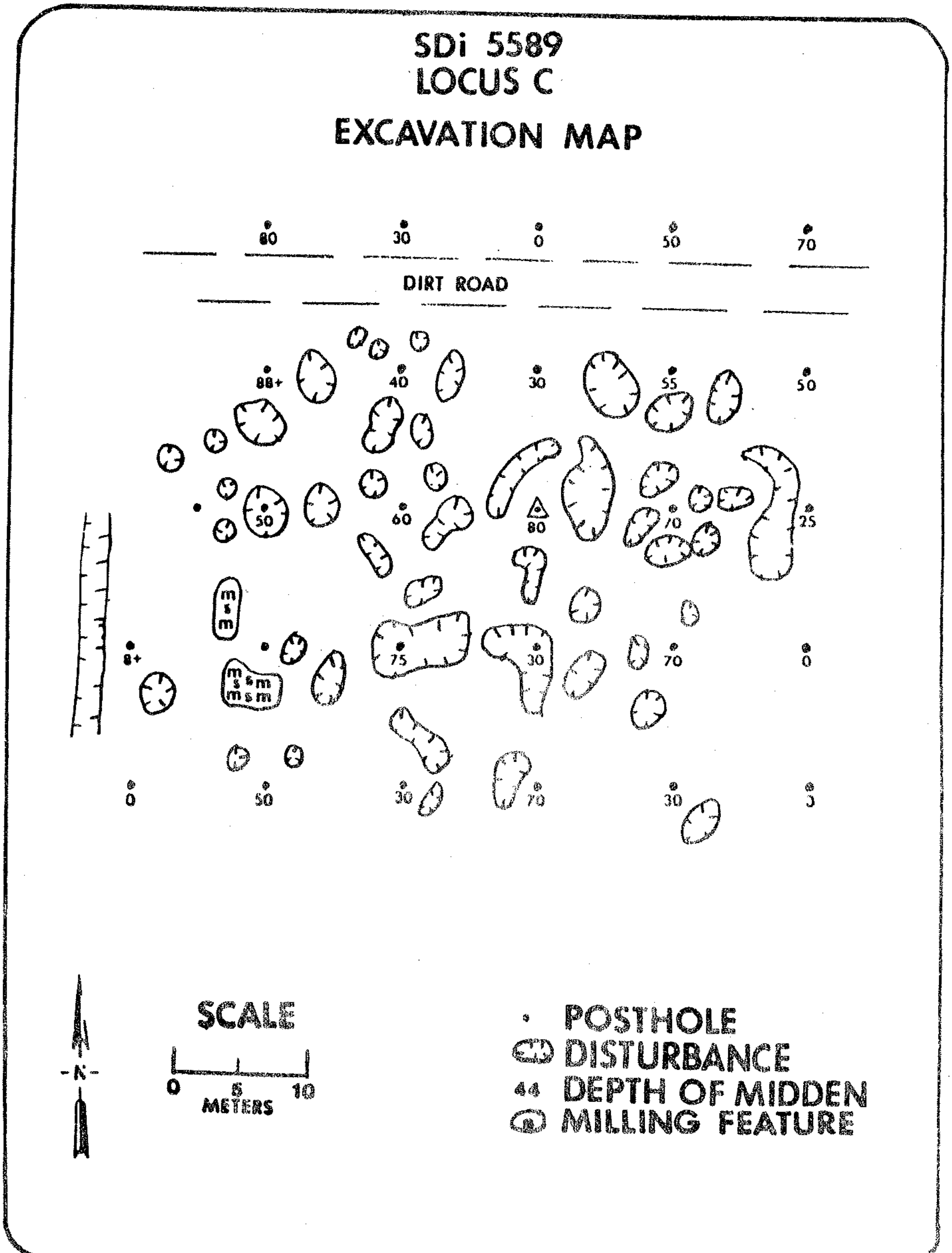
North-South Soil Profile
0 CM
1 METER
East-West Soil Profile
0 CM
Depth of Midden
1 METER

FIGURE 3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LOCUS C
EXCAVATION MAP
DIRT ROAD
SCALE
0 5 10
METERS
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POSTHOLE
DISTURBANCE
44 DEPTH OF MIDDEN
MILLING FEATURE

Figure 4

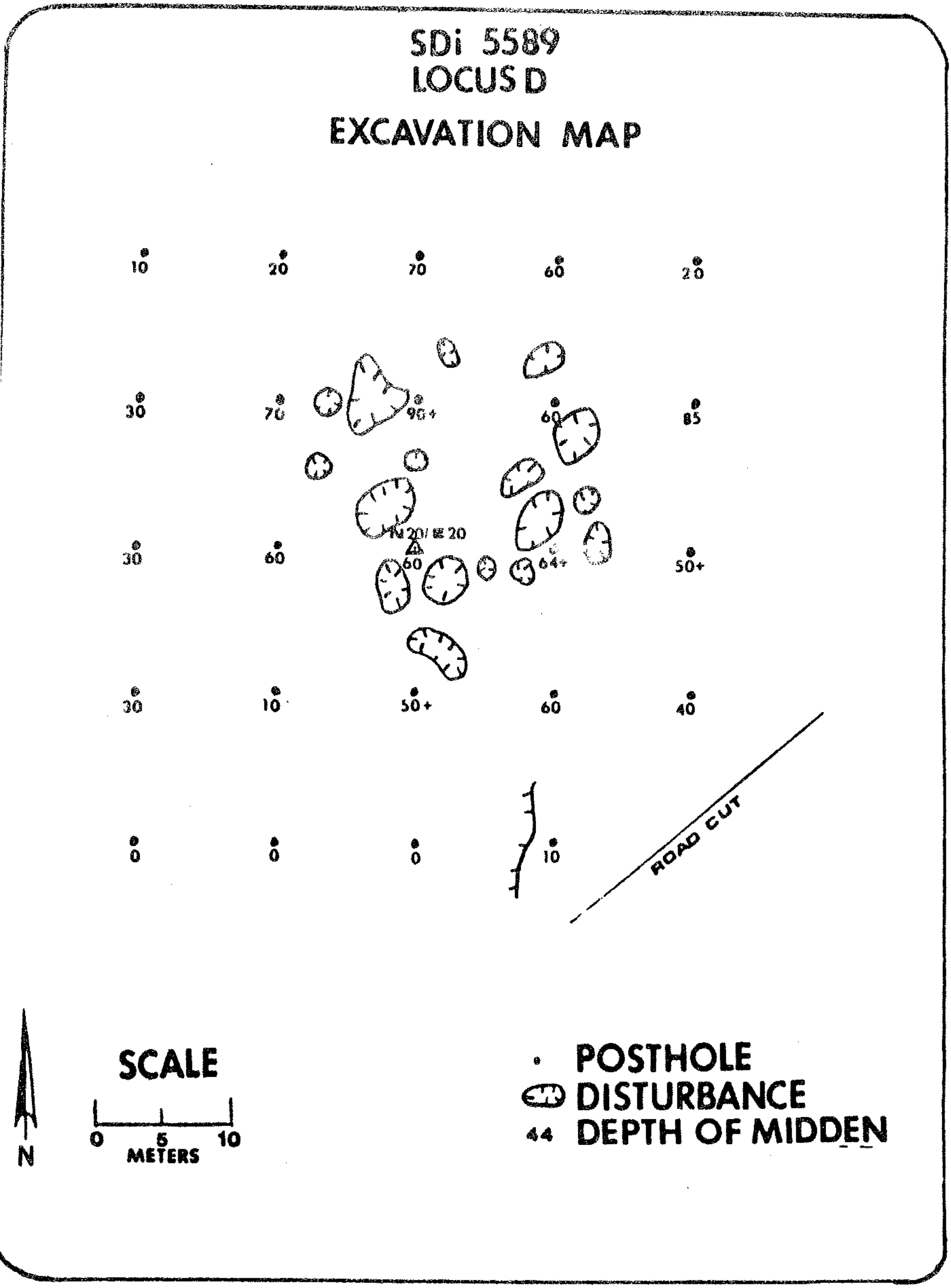
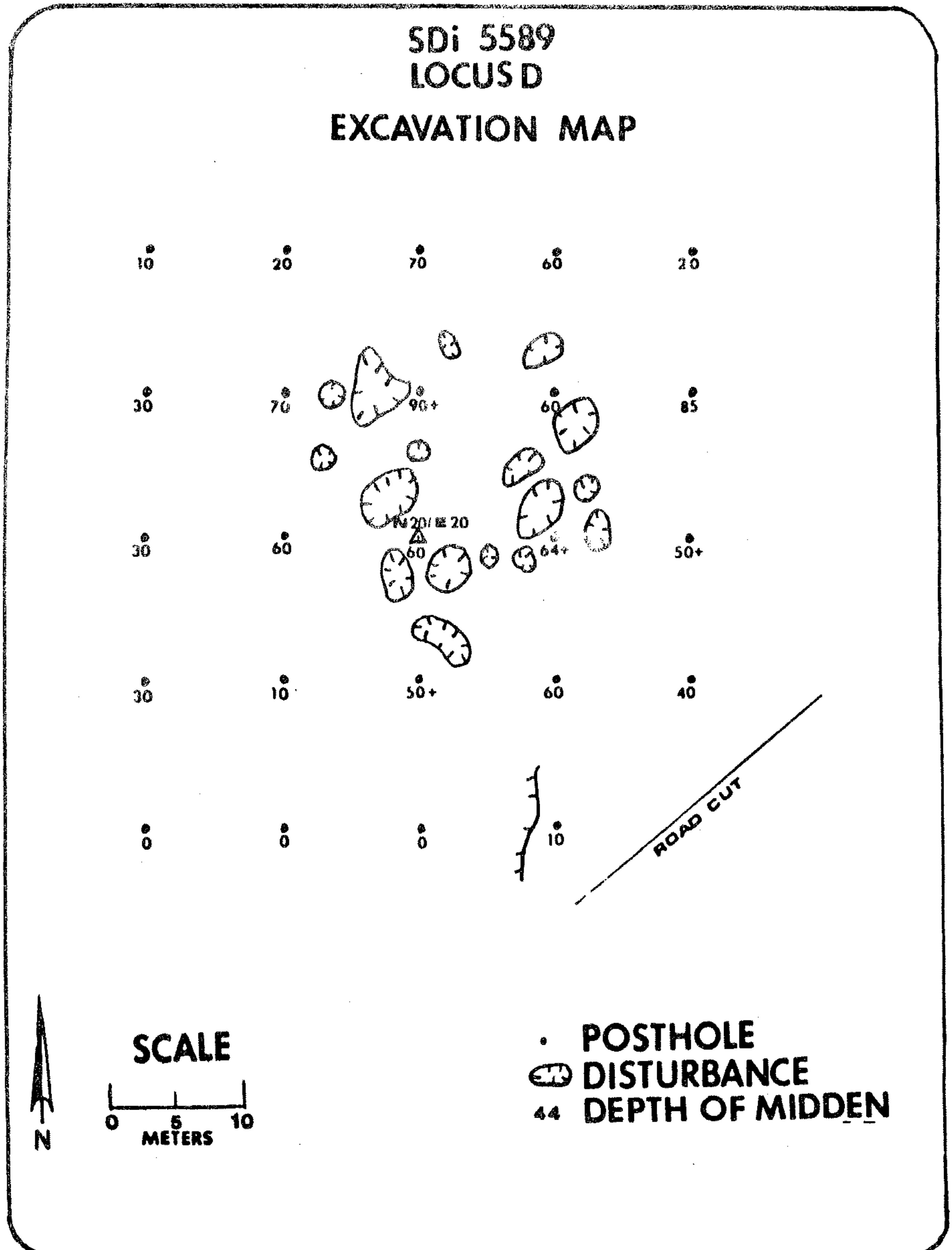

SDi 5589
LOCUS D
EXCAVATION MAP
10
20
70
60
20
30
70
90+
60
85
N20/E20
30
60
60
64+
50+
30
10
50+
60
40
0
0
0
10
ROAD CUT
SCALE
0
5
10
METERS
N
POSTHOLE
DISTURBANCE
44 DEPTH OF MIDDEN

Figure 5



SDi 5589
LOCUS D
EXCAVATION MAP
10
20
70
60
20
30
70
90+
60
85
N20/E20
30
60
60
64+
50+
30
10
50+
60
40
0
0
0
10
ROAD CUT
SCALE
0
5
10
METERS
N
POSTHOLE
DISTURBANCE
44 DEPTH OF MIDDEN

Figure 5

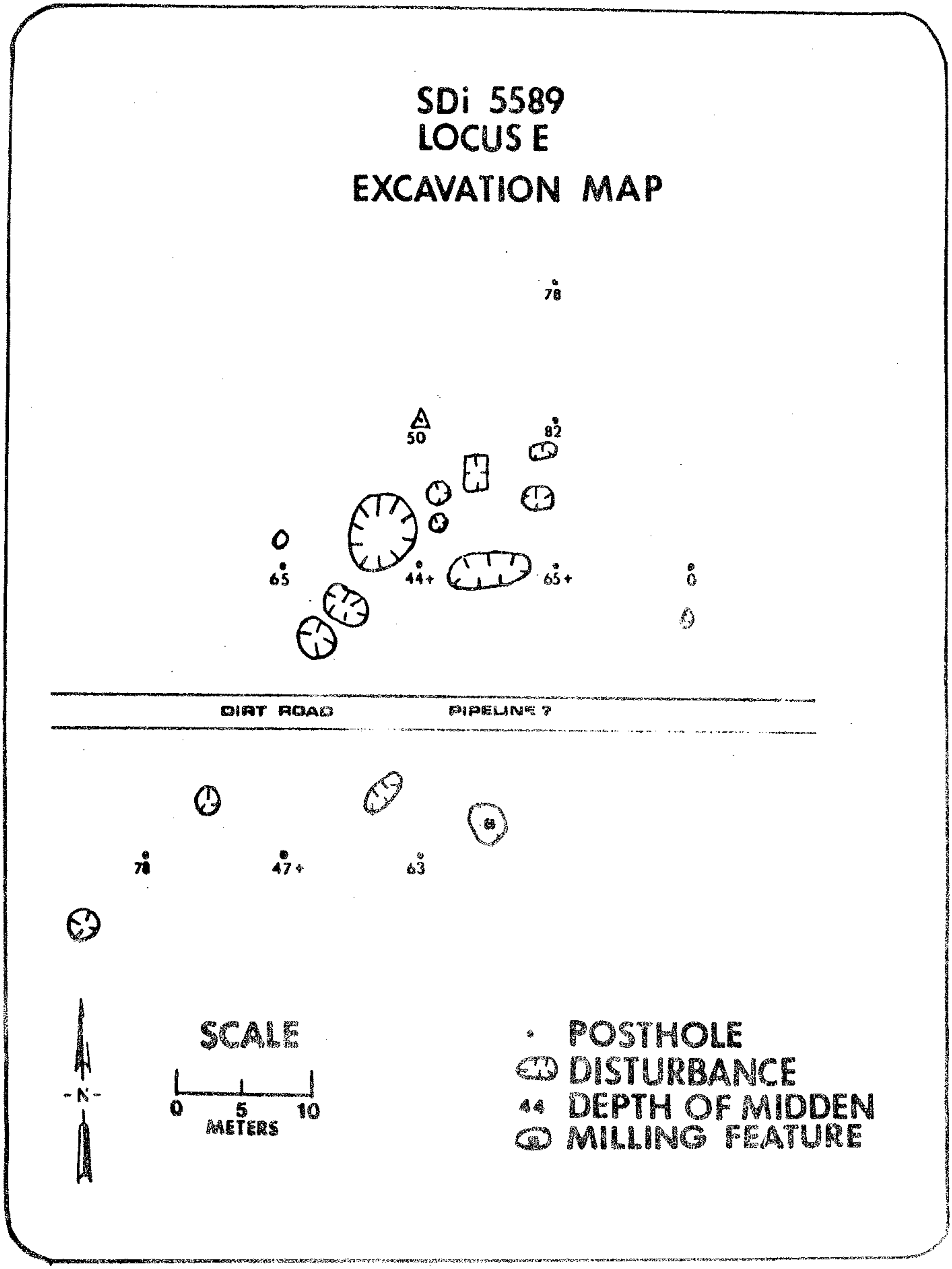

SDi 5589
LOCUS E
EXCAVATION MAP
78
50
82
65
44+
65+
0
DIRT ROAD
PIPELINE ?
78
47+
63
N
SCALE
0
5
10
METERS
POSTHOLE
DISTURBANCE
44 DEPTH OF MIDDEN
MILLING FEATURE

Figure 6

INTERPRETATION

Chronology: CA-SDi-5589 is a San Luis Rey II occupation site. There is a possibility that the lower levels of the site may be earlier San Luis Rey I, based upon the low recovery rate of ceramics, early Rancho Bernardo style pictographs, and that an area adjacent to the site had been occupied by earlier Pauma complex peoples. Materials that could provide absolute or relative dates have been recovered but not analyzed, and no dates can be given at this time. CA-SDi-5589 may have been occupied during the proto-historic/ethnohistoric period, and may be the village of Pamua or Pamame (Oxendine 1983:118-119).

Site Type: CA-SDi-5589 is a seasonal or permanent occupation site comprising six loci of midden and surface and subsurface features. Activities represented include: food processing and consumption, tool and crafts production and use, habitation, magic-religious or ritual behavior, and trade. Variability in the activities or times of occupation of the different loci is indicated but cannot be elaborated on at present.

Regional Research Values: The primary research values of CA-SDi-5589 stem from the size of the excavated sample, its location and physiographic and biotic setting, and the potential, through comparative analysis with other major excavations within the culture area to refine our knowledge of the San Luis Rey II cultural tradition. Specific research topics include: typological refinement and stylistic comparisons of chipped stone implements, ceramics, shell beads, and rock art; inter- and intra-reginal exchange of lithic materials, ceramics, beads, and food stuffs; and definition of settlement patterning, size and diversity of the subsistence catchments, or sources of raw materials exploited. While the deposit has been extensively impacted by looting, the data already collected constitute an important resource for future research, and the potential to collect additional useful data bearing upon some of the topics mentioned is still viable.

Native American Heritage Values: No efforts to asess the heritage values of CA-SDi-5589 to the Luiseno people have been made. However, as the site may be a named village location, and contains human, ceremonial and ancestral and remains according to the fieldnotes, it likely constitutes religious value to living Luiseno people.

POSTSCRIPT

Ca-SDi-5589 was determined to be eligible for inclusion on the National Register of Historic Places on May 18, 1984.

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OF GROUND SQUIRRELS AND ARCHAEOLOGISTS: DEALING WITH DISTURBED SITES

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George Borst and Rich Olmo (1983) have offered some interesting criticisms of the archaeological handling of stratigraphy and disturbance in the prehistoric sites of western San Diego County. According to Borst and Olmo, "midden" and "intact" are "the most abused words in the local archaeological literature." Genuinely stratified sites are "the null set" here, stratigraphy either having been destroyed, particularly by ground squirrel activity, or else never having been present. "The working paradigm 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." Not stated, but apparently concluded by Olmo, is the conclusion that excavating San Diego County sites by levels is a habit of gross inefficiency.

Several of Borst's and Olmo's points seem well taken and useful. "Midden" is indeed so abused a term in California archaeology as to be virtually meaningless. Assumptions about tidy "layer-cake" stratigraphy do seem to underlie too many analyses to which they are inappropriate. More attention to the processes of site deposit formation and alteration is certainly overdue.

However, other aspects of the views of Borst and Olmo are less satisfactory. The issue of "intactness" is the focus of these problems. The authors point out, probably correctly, that this is one of the badly abused terms in local archaeology. What seems to underlie Borst's and Olmo's understanding of that term, and what underlies the understanding which some other archaeologists bring to the term, however, is an absolute rather than a relative distinction. A site is not "fairly intact" or "relatively disturbed" in this view; it is intact or it is not intact, like a sort of archaeological virginity. Again, the fundamental problem is not really the choice of definitions for a term but the consequences of that choice, in the matter of how "non-intact" sites should be treated.

Taking "intact" to mean "totally undisturbed," I would agree that few if any archaeological sites in western San Diego County are "intact." I would extend the same generalization worldwide. Field archaeology may well be taken as the art of dealing with disturbed sites, of assessing the information which can be recovered from them despite their disturbance and of recovering that information. This is not to deny the great variability in the degrees and kinds of disturbance among sites or the sometimes-covarying differences in the value of their information content. But the name of the game should be "information value," not "intact/not intact."

The issue can perhaps be clarified by considering a number of dubious propositions about archaeological responses to site disturbance. Most of these are not to be attributed to Borst and Olmo, but all have been expressed or seem to have been implied by San Diego County archaeologists, and they arise out of issues raised by the paper of Borst and Olmo.

1. **Don't bother digging a "disturbed" site.** It has already been suggested that this proposition can properly be condensed to "Don't dig." An alternative proposition would be "Don't dig if digging won't produce valuable information." A site could be a complete jumble, lacking any internal vertical or horizontal integrity whatever, yet its excavatable contents could still yield valuable information about the activities which took place at that location at specific periods in the past. Disturbance destroys information or makes it harder to recover (or easier; cf. e.g., Binford 1972), but it does not necessarily or even usually destroy all information. Access the information remaining, not the information lost.

2. **If a site is in area known geographically to be erosional or stable rather than depositional, it's a surface site, so forget about digging.** Geologists and soil scientists generally work on a somewhat broader canvass that archaeologists use for site-specific studies, and they legitimately assume certain broad geographical regularities in natural processes. Archaeologists need to be concerned with very fine-scale variations in the ground surfaces they study, and those surfaces are likely to have been altered by artificial, human activities which a geological or soils survey will not have predicted. A non-depositional soils environment may contain natural or artificial depositional micro-environments of considerable interest to archaeology.

3. **Okay, but if the micro-environment is also non-depositional, it really is a surface site, so don't dig.** But as Borst and Olmo corrrectly emphasize, deposition is not the only way artifacts can get under the ground. Subsequently-buried artifacts from a "surface" site may be of great interest if the artifact sample remaining on the surface has been selectively depleted by natural processes or by pothunting, for instance, or if a larger artifact sample size is needed. Such a deposit may even have a perfectly valid sort of cultural "pseudo-stratigraphy" if the surface deposits accumulated over an extended period and if downward mixing was relatively slow; older materials from the surface may be found more frequently at greater depths than newer ones.

4. **Vertical provenience is meaningless in disturbed sites.** Again, this would really mean that vertical provenience is meaningless, period. If there has been some vertical disturbance, this obviously means that there is a risk that an artifact recovered from lower in the deposit may be more recent than one found higher up, and the more disturbance there is, the greater the risk. But this clearly does not mean that vertical provenience is meaningless; it means that its meaning is probabilistic. Patterns based

on comparatively numerous items will be more reliable than ones based on single, unique items. A site which originally had significant cultural stratification, even if it subsequently became quite heavily disturbed, is still likely not to have become perfectly mixed. Some "residual stratification," expressed in statistical frequencies of different items with depth, is likely to be detectable and may be of considerable archaeological interest. It may be detectable, that is, if and only if the deposit is excavated and analyzed in a manner appropriate to find that residual stratification.

5. **Any vertical patterning in disturbed sites is more likely to reflect the processes of disturbance rather than residual cultural stratification.** This is a real problem, but not an insoluble one. Clearly, the processes of disturbance such as ground squirrel activity operate in a patterned manner with respect to variables such as depth and the physical properties of the cultural materials being displaced; any pattern cannot automatically be assumed to be a cultural residue. It is sometimes possible to get some kind of a handle on the extent and character of disturbance, for instance by carefully recording crotovinas or by matching broken sherds and recording their relative displacements. A more direct approach would be to compare the distribution patterns of artifacts which are similar in the relevant physical parameters, which are likely to be size, shape, and density. If flakes within a certain size range which are made of comparable materials, such as basalt and felsite, or quartz and obsidian, show different patterns of vertical distribution, it is fair to consider the contrast to be a cultural one and not a product of site disturbance processes.

6. **To detect such residual cultural stratification statistically would take prohibitively large number of excavation samples.** This is not necessarily true; it all depends on how strong the residual patterning is. At one extreme, a single one-by-one meter unit might have 20 basalt flakes in the 0-10 cm. level and 30 feliste flakes in the 10-20 cm. level. The odds against such strong patterning occurring by chance in a thoroughly mixed deposit are astronomical. More subtle, blurred differences, repeatedly confirmed in several different excavation units, may be just as certainly non-random. A demonstration of non-randomness achieved with relatively numerous items can then be extended probabilistically to scarcer or unique items.

7. **Excavation by standard levels is grossly cost-inefficient in sites which probably have been disturbed.** Evaluating the cost-efficiency of different techniques is difficult and tends to be very subjective, because the different techniques recover different sorts of information, and the value of information in archaeology is not well-defined. As suggested above, the true degree of vertical disturbance is not likely to be clear until after a carefully

controlled excavation and analysis have taken place, so any decision is a gamble. In my experience, I would say that in the San Diego County area, on the average, field time in digging is equal to or a little less than field time for screening. Digging without vertical control (but with horizontal control) would perhaps cut digging time by 50%, so that a net savings of 25% or less on field time would be achieved by disregarding vertical provenience. Laboratory and analysis savings would perhaps be proportional. As a consumer of archaeological data, choosing between data from three units with vertical provenience or four units without it, from a site with uncertain amounts of vertical disturbance, I would rather have the provenienced data from three units. Sometimes that vertical provenience data will indeed be meaningless or unusable, but in most cases there will be no adequate way to know that unless it is collected first.

In conclusion, I would argue that the "working paradigms" of most San Diego County archaeologists should indeed be revised and refined, but that they should not be too hastily discarded. The absolute concept of "intactness" could well be discarded, but a relative notion of significant degrees of site integrity should not be. The issue should not be whether a site is in "mint" condition or not, but whether and how it can yield important information about prehistory.

Note: Thanks are due to Rich Olmo and Steve Apple for their patience in discussing and clarifying some of these issues. Obviously, they are not responsible for the conclusions expressed here.

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GARBAGE ABOUT THE FOUNDATIONS: A COMMENT ON BULL'S ASSERTIONS

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Charles Bull's (1983) article "Shaking the Foundations" contains so many unsubstantiated assertions, incomplete reporting of data, and errors, that it would require a response of greater length than the original article if all of the problems were to be dealt with. I have selected a few examples below in order to illustrate the lack of scholarship and biases displayed in his "analysis."

Throughout his discussion of the work conducted by True, Warren and other archaeologists in the late 1950s and early 1960s, Bull asserts that one of our basic premises was the acceptance of the chronological framework extant at the time, e.g., Rogers (1945), Wallace (1955). He states:

> The problem with its ["La Jolla pattern"] acceptance is illustrated in 1959 through the "placement" of the Scripps Estate site into a cultural chronology. The concept of placing of a site in a framework necessarily requires the acceptance of the framework.

The Scripps Estates site was identified as a La Jolla site by Rogers who had previously excavated at the site and who visited the site during the 1959 excavations. The artifact assemblage and burial patterns conformed to his limited descriptions of the La Jolla complex (Rogers 1929, 1945), as well as to those of Harding (1951) and Carter (1957). Furthermore, Harding's site (SDM-W316;SDi-531) was dated by radiocarbon at 6950±350 years B.P. (Grave and Griffin 1958); three radiocarbon dates, 5460±100, 6700±150 and 7370±100, had been obtained from the Scripps Estates site and a date of 6680 years B.P. was reported for the site in the parking lot at the Del Mar race track (Carter 1957:20; Warren and Thompson 1959:221). There was agreement that the assemblages from these sites were La Jollan and that the dates were of a similar age. However, this does not mean we accepted the earlier cultural chronology. There were, in fact, at least three different chronologies to consider. In 1959 I discussed them as follows (Warren 1959:211-212):

> The dating of the La Jolla Complex has varied from one extreme to the other. Carter (1957:369-73) would have the La Jollan industry extend back into the upper part of the last interglacial and preserve a cultural continuum of remarkable stability until the introduction of the Yuman Complex. Carter apparently recognized a division of the La Jolla Complex with the second phase of development marked by finer stoneworking technique. If I read him correctly, this innovation is attributed to contact with the San Dieguito Complex (presumably originating in the interior) at about 10,000 years ago.

> The other extreme in the dating has been advanced by Rogers, who would set the initial date for La Jolla I Complex at about the beginning of the Christian era [1945].

> Wallace (1955)...has placed the La Jolla Complex in Horizon II of his four broad temporal divisions for the Southern California Coast....Wallace estimates the milling stone assemblages [Horizon II] to be 2000 to 5000 years old.

My "placement" of the La Jolla Complex, interestingly, does not conform to any of the preexisting cultural chronologies, but is based on the radiocarbon dates.

> Giving a time range to the La Jolla Complex is still impossible to do with any certainty. We know that the La Jolla Complex was in existence between about 7000 and 5000 years ago (Warren 1959: 21).

> On the basis of known C-14 dates, it appears that the La Jolla Complex was in existence for some time prior to 7000 years ago and for 2000 or more years after that date (Warren 1959:212).

The chronology I presented in 1959 also questioned Rogers' La Jolla I and II division.

> If Rogers' division of La Jolla I and II is correct, then the burial pattern, well controlled stoneworking technique, shell beads and the doughnut-shaped stone would indicate that the Scripps Estates Site I belongs to the La Jolla II phase. If this is so, it may be assumed that La Jolla I phase predates the Scripps Estates site. However, the possiblity that La Jolla I and La Jolla II division is a reflection of seasonal economic activities rather than differences in time, or that it is the result of poor sampling, cannot yet be ruled out.... (Warren 1959:211).

Two chronologies resulted from the work of the late 50s and early 60s. Moriarty (1966) presented a series of cultural phases based on changes in artifact assemblages and associated radiocarbon dates; and Warren (1964) organized the cultural data by periods based on radiocarbon dates and environmental changes. Both of these exhibit major differences from the early chronologies of Rogers (1945), Wallace (1955) and Carter (1957). It should be clear to the reader that we were not simply "placing" cultural units in a preexisting chronological framework, we were testing the concepts of preexisting frameworks and modifying that framework as a result.

Bull's second assertion, although more subtly inserted, states or implies, that the cultural units we identified suffered from poor definition.

> Temporarily ignoring the problems with pattern definition.... (Bull 1983:28).
>
> Rather than attempting to 'strawman' all of the arguments presented for California prehistory on the basis of pattern definition...(Bull 1983:31).
>
> ...the terminology becomes more and more entrenched and the labels tend to take primacy over the patterns they describe (Bull 1983:34).

However, those articles of the late 50s and the 60s (Crabtree, Warren, and True 1963: Meighan 1954; Moriarty, Shumway and Warren 1959; Shumway, Hubbs and Moriarty 1961; True 1958, 1966, 1970; Warren and Pavesic 1963; Warren and True 1961; Warren, True and Eudey 1961: Warren 1966, 1967) contain more description and analysis of artifacts, artifact assemblages, sites and settlement patterns of San Diego County than any published article before or since. Bull (1983:28) is correct in stating that the La Jolla pattern had "only been tentatively identified until this time" (1959) by Malcolm Rogers (1929, 1945), Harding (1951) and Carter (1957). The significance of the Scripps Estates site report (Moriarty, Shumway and Warren 1959) was that it contained the first published description of a La Jolla artifact assemblage. Warren, True and Eudey (1961) described the artifact assemblages of five regions of western San Diego and discussed the artifact distribution and regional variation of artifact assemblages. Earlier, Meighan (1954) had defined the San Luis Rey I Complex, and True (1958) had defined the Pauma Complex. Crabtree, Warren and True described the artifact assemblages and temporal changes in those assemblages at SDi-603 and SDi-211. As part of the same research project, Warren and Pavesic (1963) discussed the evidence for changing environemntal conditions and the cultural adaptations identified at SDi-603. Warren and True (1961) described the San Dieguito artifact assemblage from the San Diego coast for the first time, and Warren (1966, 1967) presented additional information relating to that complex.

A large portion of these reports are devoted to the definition of cultural "patterns," especially the La Jolla and San Dieguito "patterns." Warren, True and Eudey (1961) devoted approximately one-half of the text to site and artifact description, supplemented by 22 pages of tables and graphs summarizing the distribution of artifact types. Warren, True and Eudey's (1961) article followed a format similar to True's 1958 description of the Pauma Complex. These were the first articles in which these prehistoric assemblages were described in such detail and typological comparisons made between sites and geographical areas. The more recent descriptive reports sometimes reflect the increased sophistication in sampling and use of statistics, but follow the same orientation and a similar taxonomy, if not the same typology. It was the papers of the late 50s and early 60s that define the La Jolla, San Dieguito, San Luis Rey and Cuyamaca Complex as entities that could be used in comparative studies.

Bull (1983:50) states:

> While it is quite reasonable to label certain assemblages, it is another thing to assign cultural significance to those constructions [sic]. 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.

It takes an equally "substantial step in logic" to equate those differences with a single people and single culture, a fact that Bull seems not to realize. And when we turn to Bull's discussion of prehistoric "patterns" we find a lack of consideration of artifact typology and artifact assemblages, e.g., poor pattern definition, an abundance of errors of fact, a display of ignorance of important data, and his, by now, standard unsubstantiated assertions.

> As described by reports summarized above, a San Dieguito Complex in the San Diego area has come to reflect an assemblage with a variety of patinated, flaked lithic material and a lack of milling implements. Because the pattern has been considered one in which the cultural system which created it lacks milling technology, it has been placed prior to the Milling Stone Horizon (Bull 1983:43-44).

"Patinated flaked lithic material and a lack of milling implements" is not the "pattern definition" for San Dieguito presented by Warren and True (1961, 1967). If Bull believes this to be the "pattern definition" for San Dieguito, then he and most other archaeologists are not talking about the same San Dieguito Complex.

Bull's assertion that because San Dieguito was considered to lack millingstones, it was placed prior to the Millingstone Horizon is not only unfounded, but clearly in error. In 1929 Rogers included the metate in his Scraper Maker (San Dieguito) assemblage and placed the "Scraper Maker Culture" later in the relative chronology than the "Shell Midden People" (La Jolla). However, Rogers (1940:178) later wrote:

> I included the metate in the [San Dieguito] pattern on the basis of surface evidence. Subsequent stratigraphic studies showed me to be in error and the correction was made in certain papers presented before scientific societies; papers, however, which never came into print.

It is clear that for Rogers the lack of metates did not determine the "stratigraphic" placement of the San Dieguito Complex. It does appear that the artifact assemblage and its stratigraphic placement at the C. W. Harris site led Rogers to exclude the millingstone from the San Dieguito Complex. [It should be unnecessary to point out that Rogers' quotes dates to at least 15 years before the Millingstone Horizon was defined by Wallace (1955)!] The physical stratigraphy at the Harris site also led Warren and

True (1966, 1967) to similar conclusions. Rogers' 1938 excavations, and Warren and True's 1959 (1961) excavations, are about 290 feet apart on the left bank of the San Dieguito River, yet both locations exhibit the same stratigraphy. The San Dieguito component underlies the La Jolla component, and they are separated from one another by a culturally sterile layer. Warren and True (1961:262) did hedge on the presence of millingstones in the San Dieguito Complex by stating that "grinding stones are extremely rare, if present at all."

The last example of Bull's attempt at critical analysis examined here relates to the silting in of the coastal lagoons and the relationship of resultant environmental changes to the prehistory. Bull states (1983:31):

> 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 variations 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 association with the rise in sea level.

Bull is wrong again, as a review of Warren, True and Eudey (1961); Shumway, Hubbs and Moriarty (1961); Warren, Warren and Chandonet (1961) and Warren and Pavesic (1963) will show. The "siltation concept" is based on a number of observations made in the late 1950s that included: 1) that the lagoons had, in fact, silted up and supported no shellfish, or only a very few shellfish (Batiquitos Lagoon supports no shellfish today); 2) there were numerous sites about the lagoon and aggraded valleys, some as far as 4-6 miles from the ocean; 3) these sites contain shellfish, and the most common species recovered from surface of sites were adapted to mud flats and sandy beaches; 4) radiocarbon dates suggested that occupation was more intense during the early period of the occupation than during the later period of the occupation, e.g., there was more early than late C-14 dates. On the basis of these and other observations, the hypothesis was constructed that lagoon silting reduced shellfish populations and, in turn, reduced human use of the sites along the shore of the lagoons. The excavations at SDi-603 were designed to test this hypothesis (Warren, Warren and Chandonet 1961), and to a large extent they did. Other data obtained since 1963 may not support this hypothesis, but none seems to have been published, since Bull does not site references for such data.

Bull (1983) has created a myth by presenting his readers with carefully selected data, assertions not supported by the data, and erroneous data. The "structure" he creates requires the collapse of the cultural sequence on the San Diego coast so that he can interpret the San Dieguito and La Jolla assemblages as resulting from different economic activities of the same people. Bull doesn't stop there, however. He equates San Dieguito with the Hunting People of The Santa Barbara coast and extends the "structure" to include the entire southern California coast. This "structure" is not tested, but used to address six "difficulties" in the existing sequence that Bull apparently finds disconcerting. The six difficulties are given "solutions" by Bull's "structure." A single example will illustrate the application:

Difficulty:

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 manifested in Ventura, Los Angeles or Santa Barbara counties (Bull 1983:51)?

Solution:

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 (1983: 57).

The problem with Bull's "structure" is that the equating of San Dieguito III, Pauma, and Hunting patterns is no more a solution to his first "difficulty" than is the eating of garlic a solution to warding off evil spirits. Bull's "structure" is a myth and will remain so until data are used to test hypotheses derived from it. For example, on the basis of his "structure," we can deduce that: 1) the San Dieguito III and the Hunting assemblages will exhibit functional and stylistic similarities and; 2) the San Dieguito III and Hunting assemblages will be the same age. The data do not support either of these deductions. Perhaps what Bull has presented to us is not a "structure," but a palimpsest (Binford 1980:17; Woolf 1980:818) assemblage found about the foundations.

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CRM CENTER NOTES

CA-SDi-10,000 Recorded

To date, more than 10,000 archaeological sites have been recorded in San Diego County. Site CA-SDi-10,000 described as a "sherd scatter with ash lens eroding from wash" was recorded in the BLM McCain Planning Unit by Archaeological Systems Management. The trip to Hawaii, yacht, new Rolls Royce, and large cash award for recording the 10,000th site was unexpectedly declined by John Cook.

Material for Future Issues Solicited

We again invite your participation in this publication and solicit your comments on present and/or past articles.

Manuscripts, comments, research notes, etc., should be addressed to:

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