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MISCELLANEOUS PAPERS ON ARCHAEOLOGY

UNIVERSITY OF CALIFORNIA
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UNIVERSITY OF CALIFORNIA
Department of Anthropology
Berkeley

CONTENTS

I. A STATISTICAL APPROACH TO DETERMINING WHICH MEMBERS OF A GROUP OF BURIAL SITES ARE DISTINCT

Sonia Ragir and John Stromberg

One of the major problems in archaeological analysis is the identification and interpretation of nonrandom (statistically significant) variation among archaeological assemblages from the same general cultural horizon. The Windmiller Culture sites of Central California show strong similarities in their overall cultural assemblages, but the frequency of particular types vary. If the variations within the assemblage are statistically significant, one might presumably determine their cause or causes: regional variations in the cultural tradition; cultural change through time; and/or changes in tool kit for the performance of different kinds of activity. An attempt to determine the cultural significance of the variation among the Windmiller Culture assemblages is outlined below.

Samples of projectile points are used as a basis for deciding which of the six archaeological components, all representative of the earliest cultural horizon in the Central Valley Delta region, could be members of a single statistical population. If certain assumptions are made (see below), the results allow the archaeologist to determine which sites are contemporaneous and which are separated in time. The discussion is divided into five sections: (1) description of the sites, (2) the model, (3) the method of analysis, (4) underlying assumptions, and (5) results.

Description of Sites

Five sites in the Sacramento-San Joaquin River Valley, Central California, are associated with the Windmiller Culture (Lillard, Heizer and Fenenga, 1939; Heizer, 1949; Ragir, ms.). Previous descriptions lacked clear evidence for artifact development through time or consistent trait clustering by region. Unique artifacts in each site suggest that at least some of them are not contemporary (Heizer, 1949). Different phases of the Windmiller Culture are found stratigraphically superimposed in only one site, CA-SJo-68 (Ragir, ms.). Assuming that similarity among artifact assemblages indicated a proportionately close cultural relationship between sites, we devised a computer program to compare artifact assemblages. Projectile points were used because they are the only type of artifact abundant in all Windmiller components. Furthermore, point assemblages have demonstrated temporal significance in adjacent parts of the western United States (Cressman, 1960; Butler, 1962; Lanning, 1963; Clewlow, 1967; O'Connell, 1967; Fenenga, 1953; Baumhoff and Burne, 1959). Projectile points from the five sites were typed into 18 categories according to blade, stem and base shape (Fig. 1).

The five Windmiller Culture sites are (1) CA-SJo-142, (2) CA-Sac-107C, (3) CA-SJo-68, (4) CA-SJo-56, and (5) CA-Sac-168.

1. SJo-142, the McGillivray settlement mound, stands about 1000 yards south of the Mokelumne River (SE quarter of Sec. 29, T.5N, R.5E, MDB and M, New Hope Quadrangle sheet). About six inches of fairly loose topsoil overlies an indurated gray-colored stratum about 10 inches thick. Below this stratum lie 30 inches of brownish-red mixed soil containing human burials and occupation debris. Burials are found to a maximum depth of 38 inches from the surface. The majority occur at a depth less than 30 inches and lie in shallow pits dug into the light yellow sandy clay which forms the natural base of the site. A total of 44 burials was excavated from SJo-142. Five of these were identified as intrusive Cosumnes (middle culture horizon) flexed burials lying in the gray soil horizon. In addition five cremations were probably intruded into the deposit. The projectile point sample from this site includes all specimens except those from the flexed and cremated later burials.

2. Sac-107C, Windmiller Mound, from which this culture takes its name, lies in the overflow bottom of the narrow Cosumnes River Valley (NW quarter of the NW quarter of Sec. 15, T.6N, R.6E, MDB and M, Elk Grove Quadrangle Sheet), some 12 miles north of SJo-142. The Cosumnes River originally flowed about 400 yards east of the site before man diverted it in the historic period.

   A highly compacted sandy reddish-brown clay, containing extended Windmiller culture burials, formed the natural elevation and was overlaid by a dark kitchen midden containing flexed burials and cremations from the two later culture horizons. The lowest, or C, component of the mound yielded a total of 59 Windmiller burials. Only points from known Windmiller extended burials were used in the analysis.

3. SJo-68, Blossom site, is a pure Windmiller culture site less than two miles from SJo-142. It lies 1.2 miles south of the Mokelumne River (NE quarter of the SE quarter of Sec. 32, T.5N, R6E, MDB and M, New Hope Quadrangle sheet). A loose topsoil is underlain by an extremely compacted, cemented layer about one to 1.5 feet thick. Below this "hardpan" deposit lay several feet of dark brown sandy midden. Extended burials lay throughout the deposit even in the calcareous upper level.

   A complete analysis (Ragir, ms.) of the approximately 194 burials excavated from the site led to its division into two Windmiller culture components; SJo-68A (the upper component found largely in calcareous

hardpan) and SJo-68B (the lower component in the brown sandy clay midden). These components were felt to be culturally distinct and the following analysis substantiates this division. The points from each component are treated separately in the following analysis.

4. SJo-56, Phelps settlement, a pure Windmiller Culture site, is located about 1.75 miles southeast of SJo-68 and two and a quarter miles southeast of SJo-142 (NW quarter of the NE quarter of Sec. 32, T.4N, R.5E, MDB and M, New Hope Quadrangle sheet). It is situated about 900 yards west of the Mokelumne River in the overflow plain.

The upper site deposit consisted of 12 inches of loose topsoil. Under the topsoil a calcareous "hardpan" stratum, 14 inches thick, contained a few burials. Below the hardpan a somewhat hardened or compacted refuse deposit, 28 inches thick, contained many extended burials and other evidence of occupation. Seventy-three burials were excavated from 18 to 54 inches below the surface. All but two burials were prone, fully extended, with their heads to the west in the typical Windmiller burial position. The present analysis does not include the grave goods of the flexed burials which are probably later and intrusive into the deposit.

5. Sac-168 is located on a slough near the western bank of the Cosumnes River (NE quarter of the SE quarter of Sec. 1, T5N, R5E, MDB and M on the Bruceville Quadrangle sheet) about 6 miles south of Sac-107C and the same distance north of the cluster of Windmiller sites SJo-142, SJo-56 and SJo-68) in the "big bend" of the Mokelumne River. A dark kitchen midden (Sac-168A), containing Hotchkiss burials and cultural material between 300 and 500 years old, overlay a cemented "hardpan" capping a compacted brown midden containing Windmiller burials. This Windmiller component (Sac-168B) yielded 25 burials and five features. Many of the Sac-168B burials appeared disturbed, probably a result of poor preservation, aboriginal digging in the deposit and a recent flood. Mortuary goods included only four projectile points. In order to obtain an adequate sample for the statistical analysis, we used all points found in the Windmiller component of the site (i.e., points from burials as well as dissociated ones found in the midden).[1]

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1. Subsequent analysis isolated five phases of Windmiller Cultural development (Ragir, ms.). SJo-68, Sac-107, Sac-168 and SJo-56 each contained more than one phase. The following analysis was, however, largely substantiated, the general temporal positioning of sites being the same.

The Model

Every statistical analysis employs a formal, precisely-stated model of the real-life problem to be analyzed. In this case the model is essentially a multinomial probability law, a mathematical formula which associates a probability with every possible outcome of a particular "experiment".[2] The following is an example of the use of such a probability law.

> A number of differently colored balls are selected one at a time, at random, from an urn, and then replaced. That is, a ball is selected, its color recorded, and then it is returned to the urn before the next selection. If we know the relative proportions of different colored balls in the urn, the multinomial probability law appropriate to this experiment will answer such questions as, what is the probability of drawing five black balls? three white? two green?

The list of proportions of differently colored balls in the urn (in statistical terminology, the parameter vector) is also an important part of the description of the experiment. For statistical purposes, the parameter vector completely characterizes the urn; that is, it contains all relevant information concerning the urn. Thus, for the purpose of statistical analysis, two urns are the same if their respective parameter vectors are the same and they are different if their parameter vectors are different. Urns with the same parameter vectors tend to yield samples of similar color proportions. And, concomitantly, samples which are identical in color proportions are likely to have come from urns with similar parameter vectors.

A multinomial probability law is quite general, and can be used to describe the process by which an anthropologist comes into the possession of a collection of projectile points produced by a group of Indians and left in their burial site. The "experiment" in this case is not so easily described as that of sampling from an urn, and in fact a number of possible alternate ways of characterizing the experiment depend on such variables as personal experience, technical vocabulary, or the context in which it is being discussed. Nevertheless, the situation is analogous to sampling from an urn because the burial site, or the culture represented by the site, is like the urn in that it is completely described by its parameter vector, and the process by which an archaeologist obtains a sample of flint points is analogous to the process of sampling from the urn. This sampling process is discussed at further length below. In fact, both situations (that of the urn and that of the burial site) may be described as multinomial experiments. Point samples which are identical in the proportions of various types are likely to have come from sites with similar parameter vectors, and the sites are therefore culturally identical with respect to points.

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2. Experiment is enclosed in quotation marks because, as we shall see, it may be used in a much wider sense that that to which one is ordinarily accustomed.

We are now in a position to apply the above terminology to the Windmiller culture sites. Assuming that the point samples from each of the six components are the results of six multinomial experiments and that each site (or living group responsible for it) is characterized by its parameter vector, we realize that the specific question becomes one of determining which of these parameter vectors are the same and which are different.

The Method of Analysis

A natural approach would be to consider each pair of parameter vectors (i.e. burial sites) separately and to perform a statistical test of the hypothesis that the two vectors in question are the same. There are, however, two serious drawbacks to this approach: (1) It is possible, for instance, to reject the hypothesis that sites one and three are the same while not rejecting the hypothesis that one and two are the same and that two and three are the same, and (2) the results of the statistical tests for the various pairs of sites are not independent of each other -- which raises the very complex question of what level of confidence is appropriate for the results of all the tests taken as a group.

A second approach would be to use some measure of the degree of association between two sites and to incorporate this measure into the statistical model. Thus, we might test the hypothesis that a particular pair of sites shows a high degree of association. This approach is subject to the second objection mentioned in the preceding paragraph; but, more importantly, one must realize that measures of degree of association tend to be very arbitrary, for many equally valid (or invalid) measures may be defined and each measure leads to a potentially different set of results.

The consideration just mentioned, plus others of a more purely statistical nature (see page 6), led us to set the problem in what is known as a decision theoretic framework. What this involves is, in effect, "listing" all the possible decisions we could make regarding the similarities and dissimilarities among all six sites taken as a group. For example, if the experiment dealt with only three burial sites, we would consider the following possibilities: Vectors 1, 2 and 3 are the same; 1 and 2 are the same and 3 is different; 1 and 3 are the same and 2 is different; 2 and 3 are the same and 1 is different; 1, 2, and 3 are all different. The five possibilities are referred to as "states of nature."

The states of nature have two particularly desirable properties: (1) They are mutually exclusive; that is, only one can hold true at a time, and (2) they are exhaustive; that is, they cover all possibilities, so one must always be true. These properties enable us to avoid the logical difficulties inherent in the pairwise comparison method previously discussed. Also, they provide a convenient framework in which to express the results

of the analysis. In other words, our knowledge concerning the similarities and dissimilarities among the burial sites will be completely contained in a set of probabilities that the various states of nature are true. Perhaps one state of nature will have a very high probability (say, above .90), in which case there will be an essentially clear-cut or unequivocal "solution" to the problem. On the other hand, several states of nature may be almost equally probable, reflecting the fact that our sample data do not permit us to completely "solve" the problem, i.e., to completely distinguish the true state of nature from all of those we are considering. The latter may result from the fact that the sites overlap in time, or that they are only relatively rather than completely alike or different from each other.

The statistical device used to determine the probabilities of the various states of nature is an extent of a statistical result known as Bayes' formula. This formula enables us to use the sample data (our information on projectile points) to move from one set of probabilities for the states of nature to a second set of probabilities for the states of nature. The first set of probabilities, referred to as the prior distribution, reflects our knowledge about the states of nature prior to utilizing the information contained in the sample data. The second set of probabilities, referred to as the posterior distribution, reflects the knowledge expressed in the prior distribution modified by the additional information contained in the sample data. The information contained in the sample data is expressed in terms of probabilities determined by various multinomial probability laws.

In general, the prior distribution can be determined in virtually any way whatsoever; even purely subjective assessments of the probabilities of the states of nature may be used. Since the posterior distribution depends on the prior distribution, any reservations (doubts as to validity of criteria) that apply to the prior distribution will also apply to the posterior. In the case treated in this paper, we have made the decision to keep the results of the analysis independent of other information (for instance, carbon-14 dating) and of subjective assessments of the probabilities of the states of nature. This is reflected in our use of a so-called "uniform" prior distribution. The uniform prior distribution assigns equal probability to each state of nature. Relying solely on the sample data, the uniform prior distribution eliminates the bias which might occur if we determined on subjective criteria, which states were more probable and which were less probable.[3]

This completes the discussion of the method of analysis, except that

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3. To be perfectly correct, we should add that within each state of nature the uniform prior is also uniformally distributed over all possible "values" of the parameter vectors. This added complication stems from the fact that each state of nature specifies only relationships among parameter vectors and not the "values" the vectors so constrained can take on (see Stromberg, 1967).

we should mention one related detail at this point. Recall that our first inclination was to approach the problem by comparing each pair of parameter vectors. This approach had considerable intuitive appeal, and one might consider whether or not there is some connection between this approach and the more general one we actually used. In fact, there is some connection; we can transform a set of probabilities for the various states of nature into a set of probabilities regarding whether both members of each of the possible pairs of vectors are the same. Consider the following example of three burial sites already discussed above. (We shall introduce here a simple and convenient notation for the states of nature, using slashes to separate distinct groups of vectors; thus 12/3 means vectors one and two are the same, three is different). Let the probabilities of the five possible states of nature be

$$\underline{P}\ (123) = .10,\ \underline{P}\ (12/3) = .50,\ \underline{P}\ (13/2) = .05,$$
$$\underline{P}\ (23/1) = .15,\ \underline{P}\ (1/2/3) = .20.$$

Then the probability that vectors 1 and 2 are the same is equal to $\underline{P}(123) + \underline{P}\ (12/3) = .10 + .50 = .60$. Similarly, the probability that one and three are the same is equal to $\underline{P}\ (123) + \underline{P}\ (13/2) = .10 + .05 = .15$, and the probability that 2 and 3 are the same is equal to $\underline{P}\ (123) + \underline{P}\ (23/1) = .10 + .15 = .25$. This device allows us to transform our results (in the form of a posterior distribution) into a set of probabilities that each possible pair of vectors is equal. (See section on Results.)[4]

Underlying Assumptions

The statistical analysis described above is expressed within the framework of a model, and thus the results it produces are applicable to the real-life problem under consideration only to the extent that the model is a close approximation of reality. Several major assumptions underly the model.

In order to use the results of this analysis to determine which burial sites were contemporaneous and which were separated in time, one must assume that the more two sites are separated in time, the more their respective parameter vectors will differ. In particular, this assumption implies that there is no significant cyclic behavior regarding types of projectile points. Such cyclic behavior would severely restrict the usefulness of our results for dating, although it would not affect their value for determining which sites are similar with respect to points. For instance, the length of women's skirts would not be a useful variable to use for dating samples from different periods in the twentieth century. (Richardson and Kroeber, 1940).

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4. Note that we have avoided the problem of inconsistency discussed earlier by using a decision theoretic framework instead of testing hypotheses.

A second important assumption is that it is appropriate to describe the process by which the anthropologist acquired his sample of points by means of multinomial probability laws. While such considerations as replacement of early point types by later groups (Section 2, the Model), or regional variation in point use within the delta are probably not of major importance (Heizer, 1949; Ragir, ms.), there are certain situations that may have occurred which could seriously invalidate the results. In particular, if the persons collecting points at one of the sites had biased the sample toward certain types of points, this might render the results meaningless. In effect, this would violate the assumption of random sampling which is necessary for the application of the multinomial probability laws. It is unlikely that such a bias operates with regard to such distinctive items as points.

A third facet of the analysis, although not properly an assumption, warrants discussion here. We assume that two burial sites are different if their parameter vectors differ. However, the concept of the difference between two parameter vectors is ambiguously defined in statistics, and in fact there are a number of ways of measuring this difference, all of which have considerable validity. (This is the statistician's version of the problem of measures of degree of association mentioned earlier.) Consequently, the anthropologist should keep the fact in mind that statistical methods using various measures of the difference between two parameter vectors may yield disparate results from the same data, especially when fine distinctions are being made. Moreover, statistical results involving this ambiguity should not be reported without mentioning the statistical method used (in this case a Bayesian technique with the uniform prior distribution described on pages 6 and 7).

Finally, the statistical analysis used here distinguishes among those parameter vectors which are <u>exactly</u> the same and those which differ even slightly. It would probably have been more realistic to distinguish among those parameter vectors which are almost identical and those which are quite different. However, this method is not as serious a departure from reality as it might appear, because (1) in the problem treated in this paper the geographical location of the sites is such that if two sites were contemporaneous, their respective living groups probably shared a common culture with respect to points, and (2) although the analysis attempts to distinguish among those vectors that are exactly the same and those that differ at all due to the limitations of the data, it is unable to determine that two parameter vectors differ if they are sufficiently similar to one another yet not identical.

Results

Recall that the method consists, in very broad outline, of determining the states of nature (in this case there are 203), which are assigned equal probability a priori, and then applying Bayes' formula and a set of multinomial probability laws to transform the prior probabilities for the states of nature into a set of posterior probabilities which reflect the added information contained in the sample.

Point type	Site					
	SJo-142	Sac-107C	SJo-68B	Sac-168B	SJo-68A	SJo-56
1	2		3	2	35	6
2	3	8	3	2	22	3
3a	1	8	1	3	42	3
3b		6	27	1	7	
5a	8	11	8	6	5	19
5b				1	3	
5c			3			
5d			1	2		
5e	1			1	1	2
6a	1	1				
6b	1					
6c	1		3		7	7
7a	4	12	6	1	9	10
7b	2	1		1		4
7c		1		1	5	9
7d					1	
9a			1		1	1
9b			1			
Totals	24	48	57	21	138	64

Table 1. Number of identifiable points (total 352) with definite provenience from six Windmiller Culture sites.

Posterior probabilities of the various states of nature are shown in Table 2.

States of Nature[5]	Probability
SJo-142, SJo-56/Sac-107, Sac-168 SJo-68A/SJo-68B	.3022
SJo-142, Sac-107, Sac-168, SJo-56/ SJo-68A/SJo-68B	.2891
SJo-142, Sac-168, SJo-56/Sac-107/ SJo-68A/SJo-68B	.2482
SJo-142, Sac-107, Sac-168/SJo-68A SJo-68B/SJo-56	.0610
SJo-142, Sac-168, SJo-56/Sac-107, SJo-68A/SJo-68B	.0445
SJo-142, SJo-56/Sac-107/SJo-68B Sac-168/SJo-68A	.0180
SJo-142, Sac-107/Sac-168, SJo-56/ SJo-68B/SJo-68A	.0167
SJo-142, Sac-107, SJo-56, SJo-68B Sac-168/SJo-68A	.0054
SJo-142, SJo-56/Sac-107, SJo-68B Sac-168/SJo-68A	.0032
SJo-142, SJo-56/Sac-168, SJo-68A/ Sac-107/SJo-68B	.0030
Sac-107, SJo-68B, Sac-168/SJo-142, SJo-56/SJo-68A	.0029

Table 2. Posterior probabilities of the various states of nature.

The probabilities of all the other states were individually less than .003 and collectively less than .0058, statistically insignificant. These results are rather striking when one considers that a priori we assumed all 203 states of nature to be equally probable, and then when

5. The order in which the sites are listed is not significant. Only the grouping and separation of sites are significant.

we modified these probabilities on the basis of the information contained in the sample we found that over 99 percent of the probability had shifted to only eleven of the 203 states of nature and more than 80 percent had shifted to just three.

The posterior probabilities of the states of nature transformed into probabilities that the two vectors for particular pairs of sites were the same are shown in Table 3.

Pair	Probability	Pair	Probability
SJo-142, SJo-56	.92	SJo-142, Sac-107C	.37
Sac-107C, Sac-168B	.65	Sac-107C, CJo-56	.29
SJo-142, Sac-168B	.64	Sac-107C, SJo-68A	.05
Sac-168B, SJo-56	.60	SJo-68B, Sac-168B Sac-168B, SJo-68A	.009
		SJo-142, SJo-68B; SJo-142, SJo-68A; Sac-107C, SJo-68A; SJo-68B, SJo-68A; SJo-68A, SJo-56	.006

Table 3. The probability that point assemblages from particular pairs of sites are identical.

Given the probabilities stated above, we are able to reach a tentative ordering of sites. Interpretation of these results, however, involves the same problem of determining direction of change encountered in all seriation techniques. Assuming that SJo-56 and SJo-142 are the youngest of the Windmiller components (Ragir, ms.) one gathers that occupations at SJo-56 and SJo-142 probably overlapped. Sac-107C and SJo-168B may have overlapped in time. Sac-168B is, however, as close to SJo-142 as it is to Sac-107C, and thus is probably younger than Sac-107C. The position of the two SJo-68 components in the temporal sequence must be based on considerations other than point analysis because both components are different from all other Windmiller communities analyzed. However, the point analysis does support the hypothesis that the two SJo-68 components, SJo-68A and SJo-68B, are culturally distinct. SJo-68B is probably the oldest of the Windmiller Components (Ragir, n.d.). The SJo-68B component appears to contain fewer artifact types which continue into the later Cosumnes components. The lack of late forms of artifacts and its stratigraphic position below a younger and distinct Windmiller component support its early position in the sequence.

There is no evidence in the point analysis alone which suggests the

position of SJo-68A in a chronological sequence of the known Windmiller components. Its stratigraphic position puts it younger than SJo-68B, while certain bone and shell artifacts indicate that it is at least as late as and perhaps later than Sac-107C. Ragir (n.d.) places it between Sac-107C and Sac-168B in time. Regional differences and the fact that the site is so much more completely excavated than other Windmiller sites (Ragir, n.d.) with almost four times as many Windmiller burials may account for some of the differences in the projectile point parameter vectors.

Obviously this method of point analysis cannot solve all the problems involved in the chronological ordering of sites. However, it does build a framework within which an archaeologist can define problems which may be solved with additional information.

In the light of all the evidence, the suggested ordering of the Windmiller sites should be from youngest to oldest as follows:

(1) SJo-56 and SJo-142 contemporaneous (SJo-142 may have been occupied somewhat earlier than SJo-56), (2) Sac-168B perhaps partially contemporaneous with the early part of SJo-142 and the later part of Sac-107C, (3) SJo-68A may be as young as Sac-168B, but regional differences and a major difference in sample size seem to have caused great differences in point parameter vectors, (4) The point population of SJo-68B is distinct from all other Windmiller sites. Its stratigraphic position and total assemblage suggest it is the oldest of the sites under consideration.

In writing this paper the authors experienced considerable difficulty in deciding what level of statistical knowledge could be assumed on the part of the reader. In fact, this question was never resolved. The technique presented here is one of the first attempts to approach problems of this type on a firm statistical footing and as such it should be of interest to a fairly large number of anthropologists.

The method described above has the advantage of being superior to Chi square techniques in that it is not an asymptotic result (and we are in fact dealing with small samples in this paper), and it avoids the problem of testing multiple hypotheses, which causes serious difficulties if Chi square techniques are used. For further excursions into the statistical theory behind this method of analysis and for a detailed description of how to actually calculate results, the reader is referred to John L. Stromberg's Master's thesis[6] in the Department of Statistics, University of California, Berkeley, 1967. There are presently probably only a few anthropologists with sufficient statistical background to be able to apply the method themselves.

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6 Copies are on file in the University of California Library, Berkeley.

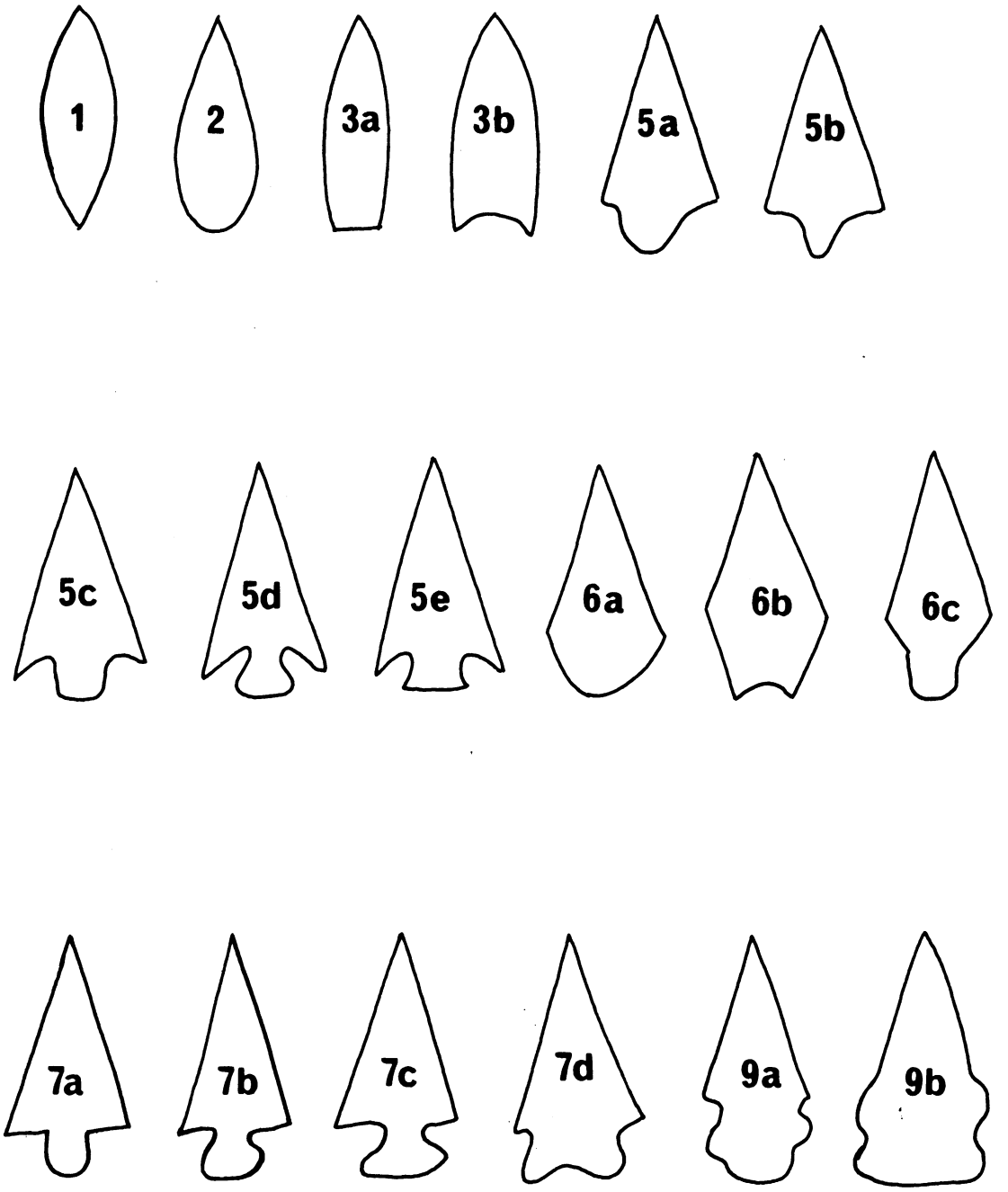

Fig. 1. Projectile Point Typology, Windmiller Culture.

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II. CAN POTTERY RESIDUES BE USED AS AN INDEX TO POPULATION?

Sherburne F. Cook

This paper attempts to explore a field which is vast in scope, but which has been entered only casually and with few concrete, satisfying results. It attacks the problem of how archaeology may contribute to knowledge of demography among populations for which there exist no head counts and no written records. The procedure must be indirect and must establish associations between the physical aspects of departed cultures on the one hand, and, on the other hand, the number of human beings who constituted those cultures. Thus we must seek to utilize material residues as a clue to the magnitude and nature of the population which produced them. It is recognized that no simple formula exists for solving this problem, and that first results may be dubious, incomplete, and disappointing. It is worthwhile, however, to discover where information is lacking, and what blank spaces must be filled in before the analysis can be fully accomplished.

Of all the material substances deposited by former human populations, the most conspicuous, ubiquitous, and prolific are those derived from ceramics. Pottery, the product of fire clay, is virtually indestructible, and can be recovered in immense quantity from habitation areas over much of the surface of the earth. It is therefore widely available as material to examine the possibility of whether it may not be utilized for evaluating population sizes, and, to a limited extent, interrelations between population and environment.

If we are to establish a numerical relationship between an accumulation of ceramics, usually in the form of broken fragments called potsherds, and the population which produced it, methods must be developed which will be broadly applicable to archaeological sites. At the same time these methods will vary widely in detail from one area to another, first, because no two inhabited localities will have passed through exactly the same series of experiences, and, second, because excavators and students have differed enormously in their competence and in the categories of data which they wished, or were able to obtain. For these reasons only a few rules of a general nature can be formulated. Beyond them, an analysis of each individual site must be undertaken.

In view of these considerations we present a discussion which employs the device of the case history. From the almost unlimited range of choice provided by archaeology, we select, almost at random, a few examples for detailed examination, and devote especial attention, as they arise, to certain

subsidiary problems which we encounter. The broad area to be considered is the American Southwest. This area has been selected because it has been exhaustively studied, and is characterized by relatively well-dated sequences. Moreover, it manifests a prolific display of pottery. We are concerned primarily with four representative sites.

The first test site will be the Pueblo of Peco, and the discussion is based upon the work of A. V. Kidder, as published in Southwestern Archaeology (1924) and Pottery of the Pecos, volumes I and II (1931 and 1936). We have here an important site which illustrates an advanced culture and which persisted into historic times.

A second extensive archaeological investigation, one which merits attention principally because of the methodological questions it raises, is the study made by Newell and Krieger (1949) of the George C. Davis site, Cherokee County, Texas. This site consists of a ceremonial mound built upon a former, or contemporary village.

The third archaeological situation to be explored is the Snaketown site on the north bank of the Gila River, thirty miles southeast of Phoenix, Arizona. Its initial excavation has been described exhaustively by Gladwin, Haury, Sayles and Gladwin (1937). Here will be found a habitation surface on the desert floor, covering roughly one square kilometer, numerous small, intrusive pits, the remains of various ceremonial structures, and about sixty refuse mounds.

The fourth sample is the S-U site, a Mogollon village near Pine Lawn on the headwaters of the Gila River, southwestern New Mexico. It was excavated in 1939, 1941, and 1946 by P. S. Martin, J. B. Rinaldo and their collaborators, who published their findings in the Anthropological Series of the Field Museum of Natural History, Vol. 32, Nos. 1,2, and 3, 19 -19 .

The initial problem which confronts the investigator who attempts to study pottery usage in these four sites, or indeed in any site whatever, is to determine the quantity of fragments, or sherds, which are contained in the habitation area. We confine ourselves at the present stage to the number of fragments, for counting is the easiest field or laboratory procedure, and most excavators have reported their results in number rather than weight. We first examine the record for the Pueblo of Pecos.

The first step is to calculate the volume of the pottery-bearing material. This is derived from two sources: rubbish heaps, and the surface of the mesa where the pueblo was constructed. On the north and east sides of the pueblo, according to the plan shown by Kidder (Plate 7, Southwestern Archaeology) the rubbish heaps extend for a distance of 1,333 feet, or 406 meters. The maximum depth, near the wall of the mesa, is 20 feet. However Kidder in his diagram of the profile of the heap (page 19, Southwestern Archaeology) does

not include a scale of linear measure. It is therefore necessary to set the maximum depth as shown at 20 feet and project the figure onto coordinate paper, whereby the area of the cross section may be computed by graphic interpolation. It may be argued with reason that the cross-sectional area of the rubbish heap is not likely to be uniform throughout its length and therefore desirable accuracy would require at least sample measurements at intervals. In the total absence of such information, however, it is necessary to depend upon the one datum provided by Kidder. The value obtained is 450 square feet, or 41.8 square meters.

Another diagram of the deposits is given on page 22 (Southwestern Archaeology). This diagram, although incomplete as published, may be interpolated and used as a profile. The value secured here is 564 square feet, or 52.4 square meters. The mean of the two estimates is 47.6 square meters. The volume of the rubbish heaps is then 406 x 47.6, or 19,320 cubic meters.

The surface of the mesa is demonstrated by Kidder's data (Pottery of Pecos, Vol. 1, page 41) to contain pottery to a depth of ten feet. The area of the mesa top may be estimated from the diagram given on page 20 of Southwestern Archaeology as 400,000 square feet. Therefore the volume of pottery-bearing material is 4,000,000 cubic feet, or 112,000 cubic meters. The aggregate from the two sources, rubbish heaps and mesa top, amounts to 131,320 cubic meters.

The second step must be to estimate the total number of fragments. Normally a large series of samples would be taken from well distributed points in the site in order to test the variability in potsherd density. However Kidder opened only three pits, or trenches, making the sample number one or three depending upon whether the three openings are regarded individually or collectively. The test therefore rests upon a very insecure statistical basis.

Kidder is also ambiguous with reference to the volume of dirt removed as well as to the number of fragments found. In Pottery of Pecos Vol. I, page 41, he states that a total of 4,219 fragments were removed from the stratigraphic pits on the mesa top and that the volume of earth taken out was 693 cubic feet, or 19.4 cubic meters. According to these figures the potsherd density was 217 pieces per cubic meter.

The three trenches are described in detail on pages 37 and 41. In the text on page 41, Kidder says: "The three tests consolidated comprise a single test of considerable proportions; one which involves.....a surface area of 208 square feet and a volume of 77 cubic yards." On the other hand, in his descriptions of the individual tests he states that trench XIX has a "surface area about 3 by 4 feet", trench XX a "surface area about 5 by 6 feet", and trench XXI a "surface area about 5 by 6 feet". In other words, by this reckoning the total surface area was 72, not 208 square feet.

In Kidder's table XII (Pottery of Pecos, Vol. I) the combined results of the three trenches are given by cuts from no. 4 to no. 11, inclusive. Since it is stipulated that each cut measured slightly under one foot in depth, the total of eight cuts may be considered as 7 feet. The volume will equal 72 x 7, or 504 cubic feet, i.e., 14.11 cubic meters. The fragment number, as totalled from individual counts, equals 3,933, and the density is 278 per cubic meter. This figure is probably closer to the truth than Kidder's estimate of 217 pieces per cubic meter, and is here provisionally accepted. The entire number of fragments in the site is now seen to be 278 x 131,320, which amounts to 36,506,960.

The George C. Davis site originally consisted of an inhabited village built upon a terrace above the Neches River. Many of its housepits were uncovered by Newell and Krieger. The occupancy of the village as such comprises phase 1 in the history of the area. Then the people contructed a ceremonial mound on the village site itself and followed it later by another, secondary mound placed upon the first. This work occurred during phases 2 and 3. However, Newell and Krieger are of the opinion that all three phases followed one another without any significant interruption, and that there were no profound cultural changes from beginning to end. The entire sequence constituted the Alto focus.

The site contained large numbers of potsherds scattered throughout the mound, under the house floors, and over adjacent fields. That portion of the village area shown on Map 4, page 16 of Newell and Krieger's monograph embraced a rectangular area 300 x 350 feet, or 105,000 square feet. Of this space, 588 sections 10 feet square, or 58,800 square feet were excavated, so as to uncover the full depth of the deposit. This is 56 percent of the area shown.

Approximately one half the surface area of the mound was uncovered. However, as is explained on page 17, not all the content was removed. The excavation proceeded from the level of the base, or below it, into the mound by a series of steps, each 5 feet high and 10 feet wide. Since we do not know from the published record the exact amount of dirt removed, we can only make a fair estimate that perhaps one quarter of the total quantity was obtained.

Newell and Krieger found 70,533 archaeological specimens in the village area, and 27,292 in the mound. Of the total, 96,000 were sherds. If we consider the factors mentioned, and if we project these figures to the entire site, we get close to 126,000 in the village deposit and 109,000 in the mound, or for both portions 235,000 specimens. However, sherds were found for a considerable distance scattered over the terrace upon which the village and the mound were built. In order to account for this additional source, we may add 25 percent, or almost 59,000 pieces, making a total of 294,000. The text states that of these artifacts, 98 percent were potsherds, or approximately 288,000.

A careful reading of the monograph and examination of technical

descriptions can lead to no conclusion other than that stated above; the excavated area shown in figure 4 of the text constitutes virtually one quarter of the mound and one half of the inhabited village. Furthermore, it appears that the excavated sample was exhaustively searched, and that the 96,000 sherds reported by the authors represent substantially all those contained in the dirt which was moved. Nevertheless, on page 78, the statement occurs, with reference to the 96,000 potsherds: "This, of course, does not represent, and perhaps does not even approach, the total made at the site as a whole, for only a small fraction of the terrace was explored." Adjustment has been made for those portions of the mound and the village site which were not excavated, and the total has been increased by 25 percent in order to account for unexplored parts of the terrace. In view of the statement cited above, the possibility remains that our estimate is too low.

We may now turn to Gladwin's account of Snaketown, written in 1937. His discussion on pages 7, 8, and 9 leads to the inference that the habitation area coincides more or less with the grid measured off by the excavators, a grid which consisted of test blocks, each 60 meters square. With 16 of these north to south and 16 east to west, the total would be 256 blocks and 921,000 square meters. However, Gladwin says, on page 9, that he tested "each one of the one hundred and fifty blocks which covered the site". The total area then becomes 540,000 square meters. The potsherds were found in three types of situation; in the 60 refuse mounds, in numerous small excavated pits, and in a surface sheet covering the area in general.

Of the refuse mounds, one, no. 29, was selected for intensive study, and three stratigraphic trenches were dug therein. On page 25 of the 1937 account, footnote 3 contains the sentence: "The analysis of the 230 odd sections in the three trenches of Mound 29, resulted in the handling of over 170,000 sherds." In addition to the total number, we must also know the density of the sherds in the excavated trenches. Although the authors state, on page 25, that the number of sherds per section of 0.5 cubic meter averaged approximately 800, and in some instances ran as high as 4,000, nevertheless it is reasonable to make an independent calculation.

There were 230 sections of dirt moved in Mound 29, each having the dimensions 2 x 0.5 x 0.5 meters, or a volume of 0.5 cubic meter. The ground was sloping, and from the diagrams shown on figures 5, 6, and 7, it may be estimated that 48 sections of trench 1 and 20 sections of trench 2 contained only one half the standard mass of soil. We may use, therefore, 230 - 34, or 196 full sections, with a volume of 98 cubic meters. The density, then, is 868 sherds per section, and Gladwin's statement is confirmed. Otherwise expressed, the density is 1,736 sherds per cubic meter. Now we need to know the volume of Mound 29 as a whole. According to the profile shown in Figure 5, and the contour surface map in Figure 4, the mound is a segment of a prolate spheroid, since the base of the mound is bounded by an ellipse. However, the

axes of this ellipse are sufficiently close to warrant averaging them and treating the mound as the segment of a sphere rather than of a spheroid. From the figure an approximation of the dimensions shows an average radius of the base (a) of 34 meters and a height (h) of 3.5 meters. Then we may apply the formula:

$$\text{volume} = \frac{h(h^2 + 3a^2)}{6}$$

This yields 6,380 cubic meters. Then, if, from the trenches, the sherd density is 1,736 pieces of pottery per cubic meter, the total in the mound is close to 11,080,000.

The sherd count for Mound 29, however, is of relatively little value since the relation of the mound by itself to the population of the entire settlement is unknown. What is really needed is the sherd count of the complete site.

With reference to mounds, we have already outlined the method for attacking the problem. When not merely Mound 29 as an individual is considered, but many such mounds collectively, it is unavoidable that they be treated as segments of spheres, and that a uniform sherd density be ascribed to all of them. To do otherwise would require extensive field examination.

From page 21 we learn that there are 60 mounds varying from 1 to 3 meters high and from 10 to 60 meters in diameter. Using the approximate mean values, we may call h equal to 2 meters and a equal to 15 meters. If the formula is applied as above, the total volume becomes 42,700 cubic meters. At 1,736 sherds per cubic meter, the total sherd number if 74,000,000.

For pits there is no definite information. Merely as an estimate, it may be held that the aggregate content of the pits is one percent of the mound content, but that the sherd density is twice as great. Then the pits would hold 1,500,000 sherds.

The sheet deposit extends over about 540,000 square meters, according to the statements previously cited, but from this value must be deducted the area covered by the mounds. If the average diameter of the latter is 30 meters, the total area is 706.86 x 60, or 42,500 square meters, leaving, in round numbers, 500,000 square meters for sheet deposits.

If we continue to base estimates upon density, that is, number of sherds per unit volume, we may note that the average depth of the surface layer is of the order of 0.5 meter, for, as is stated on page 9, "As a general rule caliche was found at about half a meter." Then the volume is 500,000 x 0.5, or 250,000 cubic meters. We took the average sherd density in the mounds as 1,736 sherds per cubic meter, but this is an extremely high value for a surface

deposit. Moreover, the sherds tend to be accumulated at the surface of the ground and immediately below it. Hence an even distribution to a depth of 50 centimeters can not be assumed. If, as a compromise, we estimate the maximum depth of significant occurrence to be 5 centimeters, the volume becomes 25,000 cubic meters. Were the density equal to that in the mounds, 1,736 sherds per cubic meter, the sheet deposit would contain 43,400,000 sherds. The value for the entire site would then be 118,900,000 sherds.

An alternative method is to disregard depth and to consider that the potsherds lying on the surface, together with those directly below it, constitute essentially the entire deposit, and that their number is a function of the surface alone. Since the excavators made no counts according to area, it is necessary to make a reasonable guess. We take 100 sherds per square meter as representing a fair average for the complete site. This means one sherd on every 10-centimeter square. If this number is too low for heavily occupied spots, in the center of the settlement, it is too high for the periphery, since habitations gradually taper off into desert. On 500,000 square meters there would then be 50,000,000 sherds, as the value for the site as a whole would be 125,500,000 sherds.

At the S-U site in New Mexico, excavation was carried out during three seasons, 1939, 1941, and 1946. Concerning each of these a separate monograph was later published, with paging continuous from the beginning of the first to the end of the third. In the second and third seasons the work extended and amplified what had been done in the first season, so that the data from all three must be considered as a unit.

The site lies on a flat ridge at about 1,950 meters elevation. In its almost level surface were found nearly thirty housepits, all of which were excavated at one time or another. Map No. 2, in the monograph covering 1939 shows 8 houses lying within a rectangular space 50 x 100 meters. Map No. 13, in the monograph on the 1941 work, extends and relocates the area to include the 10 houses studied in 1941. All 18 are placed within a rectangle of approximately 100 x 200 meters in dimensions. These two maps show also 1-meter contour lines, roads, and stream courses. Map No. 25, in the monograph on the 1946 season, adds the 9 houses dug in that year, and placed within a rectangle of 75 x 200 meters which is indicated as being subjoined to the preceeding area at the northwestern end of the latter. There are, however, no contour lines or other topographic features on this map.

If these fractions are pieced together, they produce a roughly rectangular area 100 x 400 meters. How far the site extends peripherally can not be determined from the descriptions. However, the fact that the ridge falls away steeply on all but the northwest side, implies that the habitation space was confined to the level top, covered by Martin's rectangles. We therefore take the latter as defining the site and assign an area of 40,000 square meters.

During the three seasons, more than 45,000 sherds were gathered. These were found in two types of locality, in and associated with the houses, and scattered at large over the surface of the site. The recovery for the houses, as computed from the tables on pages 84, 247-248, and 372-374, was 41,487. There were 26 houses, with a total of approximately 885 square meters floor space; hence there was an average of 45 sherds found per square meter of interior area.

The site surface, apart from the houses, was tested by a series of trenches, of which 13 are shown on maps 2 and 13, with corresponding sherd counts in the tables. Also the soil down to the sterile sub-site was tested by stripping in a reported 6 places. The depth was not more than half a meter if one may judge by the illustration in Figure 47, on page 147. Therefore the total sherds may be estimated with reference to surface rather than to volume. Dimensions of trenches and stripping are not given, but it is possible to get a close approximation from the maps, which are drawn to scale. In this manner it is found that the aggregate surface area of the 13 trenches and 6 strippings described in the first two monographs, was 603 square meters. The sherds removed were 4,221, or very close to 7 sherds per square meter.

From the 40,000 square meters estimated for the site may be deducted 885 for the houses, leaving 39,115. At 7 sherds per square meter, there would have been 273,805, or, let us say, 274,000 scattered over the surface and in the top soil. When we add 41,000 for the house interiors, the total becomes 315,000.

At the outset of this discussion, it was decided to express sherd quantity in terms of number. However, for many purposes, including the estimate of populations, it is preferable to work with weight of material rather than with number of pieces. But first it must be decided whether a stable relationship exists between these two parameters.

A simple method for comparing results by number and by weight is to determine the correlation coefficient, r, for a series of samples. Any value that did not reach the one percent level of probability for the number of samples employed, would be subject to suspicion. In this manner have been tested the series of Gifford (1951) as recast by Solehim(1960, table 4), and those of Solheim (1960, tables 1 and 2). To these we have added the data of King (1949), as presented by Baumhoff and Heizer (1959, table 2). The latter table shows the potsherds arranged according to three criteria: firing technique, kind of ware, and type of design. The results may be given in brief as follows.

Source	Number of samples	Value of r, calculated	Value of r, at the one percent level
Gifford	9	+0.984	0.765
Solheim, table 1	13	+0.974	0.661
Solheim, table 2	51	+0.973	0.354
King, by firing technique	8	+0.938	0.798
King, by kind of ware	8	+0.927	0.798
King, by type of design	22	+0.944	0.526

The very high values of r, with all these series, well beyond the one percent level of probability, indicate a close correspondence between weight and number as a means of expressing quantity of potsherds, when field collections, or other large aggregates are involved.

The question now becomes that of estimating the average weight of a potsherd, when it is encountered among masses which include many types, and which are derived from the most diverse times and places.

It is certainly obvious that a potsherd has no fixed weight. The density of the pottery itself will depend upon the nature and processing of the materials used. Furthermore, the average size of the pieces into which a vessel will break will be a function of the shape of the vessel, its strength, and the stresses to which it is subjected. These facts are demonstrated by all authors who have published pertinent data, such as those already cited. In addition, there are several calculations on record according to which average weights have been quite accurately determined.

Kroeber (1925) at San Angel, Mexico found a mean weight of 8.27 grams for 4,050 sherds. Gamio (1922) at Teotihuacán with more than 100,000 fragments obtained a mean weight of 18.8 grams. The data from Bc 50-51, as set forth by Kluckhohn and Reiter in the table on page 41 of the University of New Mexico Bulletin No. 345 (1939) may be rearranged as follows.

Pottery Type	Number of sherds	Total weight in ounces	Mean weight per sherd in grams
Exuberant	834	359	12.18
Escovada	595	200	9.51
Lino	435	131	8.52
Red Mesa	276	90	9.23
Gallup	194	92	13.42
McElmo	75	32	12.07

The range for all types is 8.52 - 13.42, and the mean for the total number is 10.62 grams per sherd (1 ounce equals 28.35 grams).

King (1949) reports that 5,881 pieces weighed 2,856 ounces. This is

0.486 ounce each, or 13.75 grams. Smith (1952) weighed 10,817 sherds and found the average to be 0.482 ounce, or 13.64 grams per sherd. Gifford's values (1951) were much lower. For 9,783 sherds the mean was 4.14 grams.

Solheim expressed his results as number of sherds per ounce, thus using the reciprocal of the more common unit weight per sherd. According to his table 1, the mean of thirteen types of classified sherds was 2.14 pieces per ounce, or 13.25 grams per piece. When the total number of sherds was used, 5,317, the values were 2.4 pieces per ounce, or 11.81 grams per piece. The difference was due to a preponderance in the entire sample of a very light type of pottery. A similar calculation based upon table 2, with thirteen types and four depth levels (52 items) gives a mean of 12.89 grams per piece. The Standard Error of this mean is $\pm$0.1 gram per piece, a very small value indeed, when compared with other sources of variation.

The present writer weighed 9,097 pottery fragments from the Isabel Kelly collection in the Museum of Anthropology at Berkeley, California. This collection is from western Jalisco, Mexico. The mean values for the different pottery styles varied from 8.69 grams for "Tuscacuesco incised red" to 109.30 grams for "Tuscacuesco polished utility". The overall mean was 25.95 grams. However, the latter value was no doubt spuriously high since the material was not a true sample of a site but represented a selection made for study and museum type collection purposes.

A recent discussion of the problem has been contributed by Johnson(1964) who examined potsherds from two Arizona sites only a few miles from Snaketown. In order to expedite the work he consistently weighed the sherds instead of counting. His criterion he explains on page 153: "In order to make the weights comparable to the counts published in the past, the average number of sherds per pound was calculated for Gila Plain and for Hohokam buff ware." There were 35 buff ware and 22 Gila Plain sherds to the pound. Johnson appears to have used the average of the two types, for, thereafter, he calculated that the 1,795 pounds which he collected at site Arizona U:13:9 represented 52,500 sherds. This amounts to 29.2 sherds per pound, and (35/22)/2 equals 28.5. Throughout the paper approximately the same factor, 29.2 is employed. The corresponding average sherd weight is 15.4 grams.

Johnson's procedure, although reasonable, is subject to question on the ground of sampling method. On page 146, he states that "Material from two of the stratigraphic tests was screened through a half-inch screen, but the soil from the other tests was simply spread out on the ground and the larger sherds collected."

Passage through a half-inch screen eliminates many of the smaller sherds, which would be counted if the material were picked over carefully by hand. Hence a bias is created in favor of large size. This bias is vastly accentuated by a

deliberate selection of only the larger pieces in the unscreened soil. The final value of 15.4 grams per sherd is therefore too high. No correction is possible because there is no unbiased sample for comparison. It is noteworthy, however, that the average weight, 15.4 grams per sherd, is close to the range which is usually observed.

If we omit the last two examples on account of their obvious statistical bias, the mean of six means is 11.41 grams. For the analysis of the sites here described, this figure may be rounded off to an even 11 grams, with the full realization that this is a pure estimate, adopted for working objectives only.

The next, and perhaps the most difficult step in the development of the study must be the consideration of the mean annual production, or acquisition of pottery. The most serious attempt to measure the life of domestic pottery and estimate its durability is that of Foster (1960), who worked at Tzintzuntzan, Michoacan, Mexico. Foster reports an approximate count of the vessels found in four households, together with an appraisal of the factors which determine their longevity.

The vessels, of which there were 50 to 75 in each kitchen, may be segregated roughly according to size and to function.

1. Cooking and eating vessels in daily use. From Foster's description the inference is reasonable that perhaps two fifths of the vessels in each house were constantly employed. There was general agreement that the average lifetime of such an article was about one year.

2. Cooking and eating vessels in occasional use. These were of various sorts, from tea cups to large casseroles, and may have constituted another two fifths. They were kept protected most of the time and were taken out relatively rarely, so that they lasted for several years. The values quoted range from 2 or 3 to 10 or 12 years. We may say 8 years as an approximation.

3. Storage and semi-ceremonial vessels. These can not have amounted to more than one fifth of the total. They were uniformly large. The maximum capacity mentioned by Foster was 45 liters. Due to their size and strength their lifetime extended for many years. Let us say 20 years as an average.

Foster made no attempt to determine weights. For the purpose of making a rather crude estimate, the writer weighed a series of five unglazed flower pots, ranging in outside diameter from 10 to 20 centimeters, and in height from 9 to 19 centimeters. The capacities approximated 0.5 to 5 liters, the range of most table and culinary pottery. The weights averaged 940 grams, or not far from one kilogram. Thus there may be ascribed a mean weight of

one kilo to the vessels of Tzintzuntzan which fell in types 1 and 2. The vessels of type 3 were much larger. Some may have reached 15 or 20 kilograms, but these were very few in number. The order of magnitude can not have deviated very widely from 5 kilograms, and this value will be assumed.

If we consider 50 vessels to be the domestic stock, we get for the three types respectively 20, 20, and 10. By weight this amounts to 20, 20, and 50 kilograms. At the turnover rates sugested previously, the outcome is a destruction and replacement rate of 20 + 2.5 + 2.5, or 25 kilograms a year. This seems to be a very high value. It is properly subject to further examination before it is accepted as applicable to archaeological habitation sites.

Tzintzuntzan is a town of close to 2,000 inhabitants, the head of a municipality of 7,820 persons. It lies in the rich lowland along the shore of Lake Patzcuaro, and has easy commercial outlet to the State Capital, Morelia. Furthermore, it is the center of a substantial pottery making industry. This feature is emphasized by the fact that the heads of two out of four of Foster's reported households were themselves potters. As such, they were no doubt living in comparatively comfortable economic circumstances. These factors are all conducive to a very substantial home store of earthenware vessels. Indeed, one would be justified in regarding the approximate 50 vessels found by Foster as the maximum likely to be seen in modern Mexico outside of the relatively affluent urban residences.

If one explores the thoroughly Indian areas, mamy of which are still to be encountered in rural Mexico, a somewhat different condition is observed. According to my observations many of the homes of the Mixteca Alta in northern Oaxaca, and of the Otomi region in central Hidalgo, the inhabitants are desperately poor, and their possessions represent the minimum with which it is possible to subsist. Seldom, if ever, do such households maintain a supply of more than ten or a dozen pottery vessels, and frequently they get along with fewer. Moreover, the proportion of vessels in continuous use is much higher than at Tzintzuntzan. Relatively few are reserved for ceremonial occasions, and no more than one or two function for long-term storage. We might, therefore, allow 7 vessels of one kilogram each for everyday purposes, 2 of one kilogram each for occasional use, and one of 5 kilograms for storage. Such an array of ceramics will not only depict the domestic situation in the remote Indian regions of modern Mexico, but will more or less simulate the probable state of affairs in the pre-Hispanic, pre-industrial culture of the pueblos.

In his discussion of durability, Foster says: "Although it is only a guess, I suggest that pre-glaze, pre-kiln cooking vessels in daily use might better be calculated as having an average life of six months, rather than one year." This suggestion is based primarily upon the greater strength of wares fired by European methods and by the greater protection offered in modern times by hearths and stoves raised to waist level above the floor. These factors certainly have exerted an influence upon probable length of life, but they may have been offset by the factor of availability. Today, at the lowest

economic level in monetary terms, and at pre-Hispanic sites as measured by expenditure of energy, the cost of pottery is and was far greater than in the homes which Foster observed in Tzintzuntzan. As a result a stronger effort would be made to conserve those articles which already had been obtained. Foster recognizes this motive, when he writes: "Perhaps in villages where it is more costly greater care is taken of it, so that technically inferior vessels would last longer than would be the case in Tzintzuntzan." In view of the relative difficulty in securing an adequate supply of household ware encountered by even advanced pre-Hispanic native cultures, such as at Pecos Pueblo, it will not be unreasonable to increase Foster's estimate and place the life expectancy of a domestic vessel in current use at 9 months. If we adhere to the time estimates suggested previously, we would allow 8 years for vessels in occasional use, and 20 years for the few which served for storage purposes. Then, if the average weights of the three types of vessels were respectively 1, 1, and 5 kilograms, and the number per household were respectively 7, 2, and 1, the annual turnover would be 5.75 kilograms.

These values are only suggestive. It is impossible to achieve real precision with such data. For working purposes we may revert to the estimates for the comparative number of domestic pots and vessels, approximately 50 found by Foster at Tzintzuntzan, and somewhere near 10 among the more primitive indigenous populations in Mexico. The ratio is 5 to 1. The annual loss and replacement, vessel by vessel, because of a series of compensating factors, appears to be more or less the same in both types of environment. At Tzintzuntzan this turnover was found to approximate 25 kilograms per year. Therefore, the gross turnover, per family per year, would be about 5 kilograms in the most primitive areas. Since the latter may be regarded as not very different in broad economic status from that characterizing the pueblos of 1300 to 1700 A.D. we may adopt this rounded value for Pecos Pueblo.

If we ascribe this rate to Pecos Pueblo, we are still confronted with the question whether the inhabitants at the other three sites used pottery as liberally as at Pecos. Undoubtedly the culture was less complex, and pottery less available, although Snaketown was a large community and may well have approached the pueblos in sophistication. At the Davis and the S-U site, however, living conditions were much simpler, there was relatively less ceremonial development, and the domestic arts probably remained at a more primitive level. Therefore turnover rates of 5 kilograms per family, per year, may be assigned to Pecos and Snaketown, and 3 kilograms to the Davis and S-U sites.

It is clear from this discussion that the study of turnover rates are needed and that these must be prosecuted with much greater intensity in order that a sound basis for calculation be established. As matters stand now, with almost total lack of reliable field evidence, values which are tentatively assigned must remain little more than reasonable guesses.

At the present juncture, methods have been outlined whereby it may be possible to determine the number, and the weight of potsherds contained by a specified site, together with the rate of loss and replacement of the original pottery. However, rate implies time. Therefore the final unknown in the equation is the duration of the habitation, or pottery-producing period at the site. Time has always been the central problem for archaeologists, and even with the aid of modern technology, through dendrochronology and radiocarbon, they are still confronted with serious difficulty. Nevertheless, in the present context it will be necessary to use their findings, although these findings may generate more problems than they solve.

With respect to the Pecos Pueblo, there is no alternative but to accept Kidder's statement (Pottery of Pecos, Vol. II, pages XVII and XVIII) that the period encompassed by the ceramic activity with which we are concerned lasted 400 years, specifically from ca. 1300 to ca. 1700. If, now, the various estimates are assembled, which have been proposed, it is found that if the average weight per sherd is taken as 11 grams, and there are 36,506,860 sherds, the total weight is 401,575 kilograms. Spread over 400 years, an annual destruction and production of 1,004 kilograms must be accounted for. If the breakage is assumed to be 5 kilograms per year per household, then the average number of households at Pecos was 201.

The social family, or household number under aboriginal conditions may be set at six. In a population which is merely maintaining itself, the biological family must equal at least four. In one which is increasing, this number must be exceeded. The household, in most societies has embraced additional members. The minimum, for single family dwellings must be regarded as five, and with multi-family dwellings many more. In California we have found consistently six (Cook and Heizer, 1968), and similar values have been reported from other areas. Hence, if there were six members per family at Pecos, the average population throughout the four centuries would have been 1,206.

A further refinement is possible. Kidder's estimate of 400 years may be considered to cover the eight stratigraphic cuts shown in table XII (Pottery of Peco, Vol. 1, page 41). Then in addition it may be assumed that each cut corresponds roughly to fifty years, beginning with 1300 A.D. The mean number of fragements per cut is 492. This number, when used as an average figure per cut of 7/8 foot in thickness, has been shown to correspond to a population of 1,206. It follows then that the ratio

$$\frac{\text{actual fragment number in a cut}}{492}$$

equals the ratio

$$\frac{\text{population in the 50 year period corresponding to the cut}}{1,206}$$

If the values given by Kidder for each cut are tabulated, the following populations may be derived.

Cut No.	Secular Period	No. of Fragments	Calculated population
4	1650-1700	562	1,375
5	1600-1650	886	2,170
6	1550-1600	804	1,970
7	1500-1550	620	1,520
8	1450-1500	308	755
9	1400-1450	334	819
10	1350-1400	215	526
11	1300-1350	204	500

A check on the accuracy of these population estimates exists in certain statements derived from historical sources. Kidder (Southwestern Archaeology, page 13) quotes the Memorial of Fray Alonzo de Benevides to the effect that at about 1650 A.D. the town "contains more than two thousand souls". The population estimated by the pottery method for the period 1600-1650 is 2,170. Kidder states that "By 1750, the population had shrunk to one thousand". The figure obtained here is 1,375.

It must be conceded that the correspondence here observed between the pottery and the historical estimates is deceptively close. On the other hand the claim may be advanced that the results by the pottery method fall within the correct range of magnitude, despite the numerous and relatively unsupported assumptions which had to be made.

The George C. Davis site presents problems for which there are no immediate, neat solutions. Concerning duration of occupancy, the authors state in their original work (page 234): "Occupation was continuous through the three 'phases'. . .We do not know whether the total time was one century, five, or ten. Let us say 'several centuries' ". However, it is possible to narrow this estimate to a certain extent.

At the conclusion of their section concerning Middle American affiliations, Newell and Krieger say (page 232): "...nor can it (the Davis site) be said to date as early as 200 or 300 A.D. ". They go on "...we may suppose Phase I to date somewhere around 500 A.D., if not earlier." Consequently the beginning of the village may be put somewhere near 500 A.D.

With regard to the termination (page 235): "After its abandonment by the Alto Focus people, a long time must have passed before a late prehistoric group (Frankston Focus) lived here briefly, leaving a few surface potsherds." In another place it is stated that the Frankston Focus "...is now regarded as entirely prehistoric". Prehistoric is taken as prior to the mid-seventeenth century.

If two hundred years is set as the extreme limits for the Frankston occupation, plus "a long time" before that, the end of the Alto Focus must have occurred no later than the year 1000 A.D. If so, the maximum duration of the latter culture could not have exceeded five centuries. As a first approximation 300 years may be considered as the duration.

Since the publication of the monograph by Newell and Krieger, the time relationships at the Davis site have been thrown into a state of confusion by conflicting radiocarbon dating. Indeed, the controversy is worth describing as an example of the difficulties encountered by even the most exacting archaeologists.

Several years after the original excavations had been completed, Krieger (1951, page 144) reported a date, determined by Libby, of 1553 $\pm$ 175 years B.P. (approximately 400 A.D.)for a sample of corn taken from a cache pit of Feature 31. Krieger said: "According to the published interpretation, feature 31 belongs to phase 1 of the Alto Focus, had most of the pottery types of this focus on its floor and probably did not antedate the initial temple mound construction by many years." The implication of Krieger's remarks is that the focus, and hence the habitation of the site did not persist more than two centuries, and that it flourished some time between 300 and 700 A.D.

A decade later Griffin and Yarnell (1963) secured a second sample of corn from a posthole of feature 31, which was dated at the radiocarbon laboratory at the University of Michigan. The date was 655 $\pm$ 150 B.P., or close to 1300 A.D. The dilemma is obvious. Two presumably well documented samples of corn are respectively analyzed by two highly competent laboratories, and found to date 900 years apart. Griffin and Yarnell, on archaeological grounds, prefer the later date, although they concede the remote possibility that both may be wrong.

The merits of the controversy cannot be decided here. Of primary interest in the present context is the duration of the culture which produced the potsherds and stored the corn, rather than the absolute date of its existence. It must have been short if Newell and Krieger are at all correct in their view that after abandonment by the Alto Focus people "a long time must have passed" before the late prehistoric group occupied the site - all this after 1300 A.D., according to Griffin and Yarnell.

If the population is computed on the basis of weight of potsherds, the previously stated average weight per fragment of 11 grams may be accepted as valid. Then 288,000 fragments weigh 3,168 kilograms, and, if the culture lasted 300 years, the annual rate of descruction and replacement would have to be 3,168/300, or 10.56 kilograms. For Pecos Pueblo the annual turnover per family was assumed to be 5 kilograms of pottery. This figure may be too high for a primitive group such as that which inhabited the Davis site, and

therefore may be reduced from 5 to 3. If this value is applied the outcome is 3.5 families, or 21 persons. If only 200 years of occupancy is allowed, there will be 5.25 families, or 31.5 persons. Although it is admitted that a relatively small settlement is being considered, this result seems unreasonably low. It merits further analysis.

An alternative procedure is to utilize Newell and Krieger's estimate of whole vessels. They found that of the 96,000 sherds collected, 11,840 were capable of being used for typing the pottery from which they came. Of the 11,840 pieces, 10,279 were actually so employed and were found to represent close to 5,031 vessels. Thus the average number of pieces necessary to establish the existence of one vessel was 2.04. The number varied with the form of the vessel, with a range of 1.3 to 3.5.

The remaining 85,000 sherds are considered to have come predominantly, but not exclusively, from the same vessels as those typed by the first 10,279. The final conclusion of the authors is that the "96,000 sherds must have come from not less than 7,000 vessels, and 10,000 is easily possible". If the median of Krieger's estimate is used, 8,500, and if it is assumed that 96,000 sherds corresponded to these 8,500 vessels, all the sherds (288,000) would indicate 25,500 vessels.

The number of families will now depend upon the duration of occupancy and the annual turnover of <u>vessels</u> per family. For the first factor periods of 200 and 300 years have been used. For the second it is necessary to convert the estimate by weight of 3 kilograms per year lost and replaced by a family to one in terms of number of vessels.

The reconstructed vessels shown by Newell and Krieger in their figures 30-43 inclusive, have diameters ranging from 15 to 40 centimeters, and depths from 5 to 40 centimeters. Nothing is stated regarding the relative frequency of occurrence of these specimens, and it must be assumed that the larger sizes illustrated in order to display types of design were actually present in relatively small numbers. Hence we may assign an average weight of one kilogram to the vessels in common daily use. Then, since the annual turnover would have been approximately three vessels, 25,500 vessels over 300 years means a production of 85 per year. If the usage was three per family per year, the families amounted to 28.3, and the persons to 170. If 200 years are assigned as the duration, the corresponding value is 255 persons.

It is evident that the use of potsherd weight and the use of vessel number lead to widely divergent results regarding the population at the Davis site. Of the two methods, the latter produces a much more reasonable estimate. The cause, probably, lies in a failure of Newell and Krieger to find, or to report <u>all</u> the sherds present. On the other hand their reconstructions of whole vessels works out very well, even though the reconstruction was based upon vastly fewer sherds than were in fact produced by the breakage of the vessels concerned.

The lesson to be appreciated is of course that for a valid population estimate based upon sherd weight or number, the total yield of the area must be known, or must be capable of calculation from a series of samples.

If greater flexibility in the result is desired than is possible with a specific numerical estimate, a range of probably values may readily be established. Thus assume durations for the Alto Focus of 100, 200, 300, 400, and 500 years, and turnover rates of 1, 2, 3, 4, and 5 vessels per family per year. Then the calculated populations, at 6 persons per family, would be expressed in a 5 x 5 set

Vessel turnover in kilograms \ Years	100	200	300	400	500
1	1,530	765	510	382	306
2	765	382	255	191	153
3	510	255	170	127	102
4	382	191	127	95	76
5	306	153	102	76	61

The most probable values show a range of 127 to 255 persons. A similar procedure may be adopted with any site for which the data do not appear to justify a single, specific estimate.

With respect to Snaketown, the time factor is discussed in detail by Gladwin and his collaborators (1937, pages 170, 178, 184, 189, 192, 197, 202, 273 ff). Haury, who did most of the writing on this subject, held the opinion that the pottery-producing era included six periods (neglecting the earliest, or Vahki period) of approximately 200 years each. The entire interval would thus have lasted from 100 B.C. to 1100 A.D., or 1,200 years. However, Haury (pages 178 and 184) felt that much more than half the total pottery was produced during the last two phases (Santa Cruz and Sacaton), which extended 400 years from ca. 700 to ca. 1100 A.D.

More recent information is provided by Gladwin in his book, A History of the Ancient Southwest (1957, pages 101, 136-147, 295). He makes it clear that the Hohokam settlement at Snaketown, which represents almost the entire habitation of the site, was encompassed by the period 700-1100 A.D. Consequently there is little doubt that 400 years is a full and yet reasonable estimate for the duration of pottery production on a significant scale.

There were derived, and previously described, two values for number of sherds, depending upon alternative methods for computing the surface

deposit: 118,850,000, and 125,500,000. At 11 grams per sherd these numbers reduce to 1,307,350 and 1,380,000 kilograms respectively. For the period of 400 years the corresponding production is 3,268 and 3,451 kilograms, when considered on an annual basis. If the breakage and replacement (i.e. the turnover) per family per year was 5 kilograms, the mean number of families would have been 654, or 690. At six persons per family, the 400-year average population would have been 3,924 or 4,140. Since we do not have a precise figure, we set the range of probable number at 3,500 to 4,500.

This value, 3,500 - 4,500, is perhaps too high, and may be due to an overestimate of the number of potsherds in the surface sheet deposit. However, the correct order of magnitude is indicated, for a town covering half a square mile and supporting the pottery manufacture, as well as the other industries of Snaketown, must have contained several thousand persons at its apogee. If further refinements in the sherd calculations were made possible by additional field data, a closer approximation to the truth could be obtained.

The dating and duration of the S-U site is bound up with the controversy which has been proceeding for many years with respect to the chronology of the southwestern cultures. Martin and his co-workers were able to show that the site was representative predominantly of the Pine Lawn phase of the Mogollon cultural complex. Thus, only three houses out of the nearly 30 which were excavated could be ascribed to later periods. Martin expressed the opinion that the Pine Lawn phase antedated the year 500 A.D. and lasted several hundred years.

Gladwin, in his monograph reviewing the work at Snaketown (1948, page 224) attempted a synthesis of numerous southwestern sites, using primarily tree-ring dating. He dated the Pine Lawn phase at the S-U site as from 700 to 850 A.D., a period of 150 years. Contrary opinion has been expressed in a more recent paper by Bluhm (1960) in her study of settlement patterns in Pine Lawn Valley. According to her text, and table 1, the Pine Lawn phase in this area was preceeded by a non-ceramic culture, and lasted from 200 B.C. to 500 A.D. Since she had the advantage of more than a decade of investigation subsequent to Gladwin, her scheme may be accepted.

It has been estimated that the S-U site contained 315,000 sherds, or 3,168 kilograms. From these we wish to eliminate those produced during the 3-circle phase. Martin's count of sherds found in house pits was 42,387. In houses W, X, and Y, from the 3-circle phase, there were 4,683 sherds, leaving 37,704 in those from the Pine Lawn phase. By proportion, in the entire site, there would be 2,818 kilograms of Pine Lawn sherds.

If the Pine Lawn phase at the S-U site endured the full 700 years allocated by Bluhm to the phase in the valley as a whole, and the turnover

rate, by analogy with the Davis site, was 3 kilograms per family per year, the average population included 1.34 restricted, or nuclear family, that is, approximately 8 persons. This is clearly an under-estimate and requires adjustment.

Although Bluhm considers that the Pine Lawn phase, as a cultural entity, persisted in the valley for 700 years, she shows in table 1 that there have been discovered 21 sites of this character. There is no implication that any one site endured through seven centuries. Indeed, there is nothing in her analysis to contradict the opinion of Martin that the Pine Lawn phase, at the S-U site, lasted 200-300 years. The probable life span of the site may therefore be reduced to 250 years.

A second adjustment concerns turnover rates. A rate was provisionally assumed throughout the existence of the S-U site of 3 kilograms per family per year, the same as for the Davis site. But it is clear from Bluhm's discussion that at the beginning there was no pottery at all, and that its manufacture developed during the 700 years of the Pine Lawn phase. Therefore, if the rate in 500 A.D. was 3 kilograms per family per year, in 200 B.C. it was zero kilograms. The average of the two extremes might lie somewhere between 1 and 2 kilograms, for example at 1.5.

If, now, we recalculate the population by substituting 250 years for 700, and 1.5 kilograms for 3 kilograms, we get 7.5 families, or 45 persons. This estimate may be regarded as acceptable.

Bluhm's formulation of the data makes possible a few further interesting conjectures concerning total population. She lists in table 1, among other items of information, the number of sites, and the number of rooms known for each cultural phase. Since, in the earlier phases, a room is equivalent to a house, the latter term may be substituted for the former. Then she divides quite empirically the number of sites and that of houses, by the number of centuries during which each phase lasted. Thus for the Pine Lawn phase there are known 122 houses (rooms) and the time interval is seven centuries. There are therefore 17 rooms, or dwelling units per century. The population of the valley is then assumed to run parallel with the houses per century.

The estimate of population for the S-U site, as derived from pottery residues, may now be introduced. This site contained roughly 24 houses from the first, or Pine Lawn phase, and is considered to have had a life of 250 years. Then it contained 10 houses per century, a value which in turn represents an average population of 45 persons. For the entire valley during the Pine Lawn phase, Bluhm gets a value of 17 houses per century. The corresponding level of population is 17/10 of 45, or 76 1/2 persons.

Bluhm says (page 538) that the valley is 10 miles by 3 miles, with 20 square miles of "habitable land". Just what is meant by "habitable" is not

clear, but we may use the entire area of 30 square miles, and estimate a density during this period of 2.5 persons per square mile. This is a reasonable magnitude for a culture which is just beginning to acquire agriculture and the ceramic arts.

If, according to Bluhm's hypothesis, population was proportional to dwelling units per century, then that of the Georgetown phase was the same (17 dwelling units and 76 1/2 persons). However, the San Francisco-Three Circle phase saw an increase to 225, and the Reserve phase to 613 persons, or approximately 20 to the square mile. Thereafter, a disastrous decline took place in the Tularosa phase, with rapid extinction of the group as a recognizable demographic unit.

These figures are completely speculative, but are by no means absurd. They illustrate more clearly than any purely relative scale the population status of the cultural complex over a period of several centuries.

The question propounded in the title of this essay (can pottery residues be used as an index to population?) still awaits a final answer. In the meantime, approximate, average populations have been calculated for four representative sites in the southwestern United States, but these values, although they lie within the limits of reasonable probability, are not definitive. The process of estimation has itself encountered new problems, the solutions to all of which are not yet clearly visible. However, progress is being made. The framework of future construction has been put together, and the broad outline of method has been established. Pottery residues, or indeed any residues can serve as an index to population provided the four cardinal parameters can be defined in numerical terms. These are, it will be recalled, total amount of residue, turnover rate, duration of production and use, and association of unit quantity of residue with unit population.

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III. RADIOCARBON DATE FROM THE POINT ST. GEORGE SITE NORTHWESTERN CALIFORNIA

Richard A. Gould

A radiocarbon date of 2260 ± 210 years B.P. or 310 B.C. from the Point St. George I occupation at the Point St. George Site, Del Norte County, California, indicates the earliest evidence of human habitation so far discovered along the northwestern California-southern Oregon coast.

In 1964 a field party from the University of California at Berkeley carried out excavations at the Point St. George Site (CA-DNo-11) in Del Norte County, California. The published report on this site (Gould, 1966) described the stratification and cultural materials found there but did not include any radiocarbon measurements. Nevertheless, charcoal samples were collected during these excavations, and the results are now available.

In the original report, two natural units containing cultural material were distinguished. The upper one consisted of shell-midden deposits with stone projectile points, bone needles, stone net-sinkers, and a wide variety of distinctive artifacts comprising an assemblage resembling that of the ethnographic Tolowa Indians of this region. This proto-historic midden layer was labelled Feature 31, and the cultural materials it contained were treated as the Point St. George II occupation. Underlying this midden deposit in Trench 3 at the site was a layer of smooth, dark brown sand (Feature 36) varying from about two to four feet in thickness and containing chipped stone projectile points and other artifacts differing considerably in style from those occurring in the midden levels. This earlier occupation was designated as Point St. George I. Underlying the Feature 36 soil there was sterile, light yellow sand called Feature 32 in the report. This sand underlay the other trenches at the site, too, although it varied in texture and compactness.

In 1964 five charcoal samples were collected from a series of small hearths at the site. One of these occurred in the Feature 31 - Point St. George II levels and was not processed because of the obviously proto-historic character of the associated materials. The other four samples came from different levels and squares within the Feature 36 - Point St. George I occupation. Two of these samples were discarded because of their extremely small size, while the other two were combined to make a single sample for processing. The combined sample consisted of charcoal and charcoal-rich sand from a pair of small, rather indistinct hearths in Square No. 52, Trench 3 at depths of 42 and 45 inches, that is, in the lowest foot of the Feature 36 deposit. This sample from these two hearths weighed 2.4 grams (after cleaning).

The sample (No. I-4006) was processed by Isotopes, Inc. of Westwood, New Jersey, in January, 1969, and yielded a date in years (B.P.) of 2260 ± 210 or 310 B.C.

The Feature 36 soil from which this sample was taken was entirely free of root contamination, and the hearths occurred in close association with stone artifacts which were typical of the Point St. George I assemblage. This soil appears to consist of sand (Feature 32) mixed with fine particles of charcoal and other organic materials which give it a distinct dark coloring. The diffuse nature of the hearths in this soil horizon is evidence for disturbance by wind during and after the Point St. George I occupation. Today the entire surface of Point St. George is subjected to strong northwesterly and southeasterly winds which have created several large, sandy blowouts and have prevented the growth of trees anywhere on the Point except in a few small swales. Geologically, the Feature 32 sand (and the Feature 36 soil, which is a derivative of this sand) is part of the Battery Formation, "A thin marine-terrace capping of unconsolidated sands exposed over the southern portion of the Crescent City platform," (Maxson 1933:136).

On the basis of fossil shells and geological positioning, both Maxson (1933:136) and Back (1957:24) regard the Battery Formation as a Pleistocene deposit. Back has commented:

> "The Pleistocene age of the Battery Formation seems to be well established, but its position within the Pleistocene is not clearly indicated. The horizontal attitude, low degree of induration, and Recent aspect of the fauna identified by Hertlein suggest a late Pleistocene age for these deposits." (1957:24).

Battery Formation sands covers the entire surface of Point St. George except for a small exposure of Point St. George Formation beds (Pliocene) on the north side of the Point (about 200 yards beyond the nearest excavations) and the headland of undifferentiated rocks exposed along the west and southwest sides of the Point.

As summarized by Heizer (1964:132-133), there are radiocarbon dates for site Cs-23 on the lower Coquille River, Oregon, and the base of the Gunther Island site (Hum-67) at Humboldt Bay, California, of 350 years and 1050 years, respectively. There is an estimated date of 1620 A.D. for the Tsurai site (Hum-169) at Trinidad Bay, California...and further north along the Oregon coast there are three radiocarbon dates from site Ti-1 of 150, 280, and 500 years. More recently, there is a radiocarbon date of about 1310 A.D. for the early levels of Hum-118, a site excavated at Patrick's Point, California (Elsasser and Heizer, 1966:103). A comparison of artifact assemblages shows there are close resemblances between the materials from Gunther Island (Loud, 1918; Heizer and Elsasser, 1964), Patrick's Point, and Tsurai (Heizer and Mills, 1952) and the Point St. George II occupation. Comparative as well as stratigraphic evidence supports the idea of a relatively late date for Point St. George II.

Thus the radiocarbon date for the Point St. George I occupation is the earliest date so far obtained for archaeological remains from the southern Oregon and northwestern California coast by at least 1000 years. Elsasser and Heizer

have stated:

> "It has been suggested, though without much evidence to support the proposition, that the northwestern coast of California was the last area of the state to be permanently settled. This may be true, but it may also be wrong." (Elsasser and Heizer, 1966:2).

The question now is: does the Point St. George I occupation and its associated date of 310 B.C. represent the earliest human habitation of this coastal region? Elsasser and Heizer have rightly cautioned against relying too heavily on negative evidence. Before starting excavations in Del Norte County I was able to examine several large surface collections of artifacts from sites on the coast between Klamath, California, and Pistol River, Oregon (Gould, 1965) and in 1965 I also examined surface collections of artifacts from the Oregon and northern California coast presently housed at the American Museum of Natural History. Out of the roughly 600 artifacts (mainly chipped stone projectile points) in these collections, there were none present that did not seem to belong to assemblages related to either the Point St. George I or II occupations. In other words, despite fairly intensive excavations, surface surveys, and studies of surface collections by myself and other investigators, there as yet exists no unambiguous evidence for human habitation of the northwestern California or southern Oregon coast prior to the Point St. George I occupation. Two ways to test this hypothesis are to continue archaeological surveys and excavation in this region either until more ancient material is found or the probabilities of finding such material become diminishingly small.

Another approach to this problem would be detailed geological studies of the coastal terraces of this region with the aim of determining the earliest date when the coastline would have been suitable for human habitation. Present geological studies indicate that the marine terrace which includes Point St. George (the Crescent City platform) was partially submerged during the late Pleistocene. Since that time there has been a series of at least three uplifts which extended and diverted the course of the Smith River and lowered the sea-level in relation to the land surface of the Crescent City platform (Back, 1957:36-37). Terraces indicative of these uplifts are visible along the Smith River at Hiouchi Valley about 5 miles inland and along the lower reaches of the river (the most recent being the Fort Dick terrace) above the present flood-plain (Back, 1957:37). However, no attempt has been made yet to date these uplifts in terms of years.

On the basis of the materials collected so far and the radiocarbon date, more archaeological excavations are needed to determine the nature and extent of the Point St. George I occupation at the Point St. George Site. Moreover, it is also recommended that additional samples be obtained for radiocarbon dating.

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IV. THE RUMSEN OF MONTEREY, AN ETHNOGRAPHY FROM HISTORICAL SOURCES

Sylvia M. Broadbent

Preface

In presenting the following essay, I beg the reader's indulgence for its deficiencies. It was written in 1953, when I was a first-year graduate student, and it looks a bit unsophisticated to me now. I had filed it away, intending to revise it and add more material to it before publication. However, it has been sitting in my files for seventeen years now, and when Prof. R. F. Heizer suggested publishing it in the CONTRIBUTIONS series I decided that it was better to make it available for whatever usefulness it might have in its present form, instead of waiting interminably for the hard-to-find time to revise it completely. I have corrected a few errors of translation and tried to smooth out one or two rough passages, but that is all. I therefore ask that it be accepted, with my apologies, for what it is: the work of a beginner.

Introduction

The Rumsen of Monterey were one of the many small groups of Indians in California who were thoroughly absorbed into the Spanish missions and have since become extinct. Their culture vanished gradually as they were incorporated into Mission life, and later Mexican and American communities. Their language is now forgotten. Very few even of their descendants are left. Although many of the fine old Spanish families have a little of the Indian in them, they are very loath to admit it. Socially, culturally, and linguistically, the Indians of Monterey are dead.

It is, therefore, impossible to obtain much information concerning the Rumsen by standard ethnographic field methods at this late date. There are no informants left to interview. There is, however, one place where such information may be obtained: the historical record. The material is scattered, and consists mainly of small items, the result of chance observations rather than purposive research, but it is of some value. It is to be found in the official and private journals and memoirs of travellers, the accounts of early settlers, documents concerning the Mission, letters, and the like. The present paper represents an attempt to gather together some of this material and to present it in an orderly manner.

One major source of error in this paper will undoubtedly be the fact that not only the Rumsen, but also Esselen groups were drawn into Carmel Mission. Hence, when writers speak of the Indians of Carmel Mission, it is

impossible to tell whether they are referring to the Rumsen or to the Esselen except in a very few cases. However, the Esselen of the Santa Lucia Mountains and the Big Sur region were the immediate neighbors of the Rumsen. It is probable that the two groups were culturally very similar, and that most statements apply to both.

History of white contact and early descriptions of the Indians of the Monterey Region

The first recorded white man who is believed to have seen Monterey Bay was Cabrillo. Wagner (1923-27, VII:26) believes that the Bahia de los Pinos in which Cabrillo anchored November 16, 1542 (ibid., p. 50) was the northern part of Monterey Bay. Cabrillo, however, did not land, owing to the heavy surf. He left no description of the Indians of the region.

The next voyager who may be presumed to have visited the region is Cermeño. Wagner (1923-27, III:6) states that Cermeño's "Bay of San Pedro" "was certainly Monterey Bay..." It was seen by Cermeño on December 10, 1594 (ibid, p. 15). He did not stop there, but anchored farther south, perhaps near Point Sur.

The first travellers to describe the Indians of the Monterey region were those of the Vizcaino expedition of 1602-3. Several accounts of this expedition remain to us. A number are in the Archives of the Indies in Madrid (Carrasco y Guisola, 1882): these include Vizcaino's own account, and the so-called Bolanos derrotero, prepared in actuality by Geronimo Palacios. An important account of this voyage was written, perhaps ca. 1611 (Wagner, 1923-27, VII:269), by Fray Antonio de la Ascención, a Carmelite friar who was with the expedition. The original manuscript of this account is in the Ayer collection of the Newberry Library; the Bancroft Library has a microfilm copy. A stylistically unrepresentative translation of this account has been published by Wagner (1923-27, VII:295-394); it does not seem to have been published in the original. Torquemada (1723: 1st ed., 1611) included an abbreviated and somewhat garbled version in his Monarchia Indiana. This was translated into English in the eighteenth century. It was reprinted in a limited edition in 1933 (Vizcaino, 1933) by the Book Club of California. The version that will be used here is the original manuscript, with the writer's translation, which attempts to adhere to the style of the original more closely than did Wagner's.

Sebastian Vizcaino had been sent by the Viceroy of Mexico to attempt to find a suitable spot on the coast of California for a harbor for the ships from the Philippines. The galleons were already sailing down the coast of California on their way to Acapulco, making landfall at Cape Mendocino or the Sierre de Santa Lucia south of Monterey. Ascención states (ms, f.74^{r}) (1):

> "...and they were near a sierra, very high and white, and on the slopes all reddish, covered with much brush, this

> is called the Sierra de Santa Lucia, which is ordinarily sighted by the ships which come from the Philippine Islands..."

Vizcaino arrived at Monterey Bay December 16, 1602 (Ascención, ms. f. 74v; Carrasco y Guisola, 1882, p. 98). He decided that it would be an admirable port for the scurvy-ridden galleons to recuperate, and at once dispatched a glowing account to the Viceroy the Conde de Monterrey, after whom he had named the port. He left Monterey January 3, 1603.

The following accounts of the Indians of the region were included in the official reports filed in the Archives of the Indies:

(Carrasco y Guisola, 1882, p. 99; Vizcaino's own account) (2):

> "The land [is] well populated with Indians without number, many of whom came on different occasions to our camp. They seem to be gentle and peaceful people; they say with signs that there are many villages inland. The sustenance which these Indians eat most of daily, besides fish and shellfish, is acorns and another fruit larger than a chestnut; this is what we could understand of them."

(Carrasco y Guisola, 1882, p. 169; the "Bolanos Derrotero") (3):

> "There are Indians although they are distrustful of dealing with us. That is to say that the aforesaid Indians came in peace, and from appearances are good people, they brought us shellfish and made great efforts to bring us to their town which they made signs was inland...."

The following is Ascención's account (Ascención, ms. f. 83^{v}) (4):

> "The port is all surrounded with rancherias of affable Indians, good natives and well-disposed, who like to give what they have, here they brought us skins of bears and lions and deer. They use the bow and arrow and have their form of government. They were very pleased that we should have settled in their country. They go naked at this port."

Vizcaino made several attempts to persuade the authorities to make a settlenent at Monterey, but nothing was done about it until 1769. In the intervening hundred and fifty years, it is uncertain whether Monterey was ever visited by the Spanish. It seems quite likely that the Manila galleons occasionally stopped in Monterey on their way down the coast, but there is little direct evidence that they did. Cabrera Bueno, chief pilot of the Filipine fleet, in a book on navigation published in Manilla in 1934 gives a derrotero--a set of sailing directions--for the route from Cape

Mendocino to Acapulco. His description of Monterey does not appear to have been copied directly from the Vizcaino reports; his phrasings are different, and he mentions details which are not in the Vizcaino reports. He says of it (Cabrera Bueno, 1734, p. 303) (5): "...and it is a good port for the relief of the ships from China for it is the first land they recognize, when they come to New Spain.." It seems possible that Cabrera Bueno, or other pilots of the Philippine fleet, may have actually seen the bay.

A second suggestion of occasional visits by transPacific trading vessels comes from the San Carlos baptismal records. Entry number 834 reads (6):

> "On the 4th of October /1783/ in the church of this Mission of San Carlos of Monterey I solemnly baptised an ancient man of more than a century of age, native of the rancheria of Sargenta ruc, married, whose wife still lives; his birth was in this neighborhood, and he says that he remembers when of old the ship from China used to make port here, that they dealt with these people giving them glass beads for otter skins, and that on one occasion they left on Cypress Point a cask or barrel that they might avail themselves of the iron of the bands, he was called in his gentile state Pechipechi and was held by his own people in great veneration, I gave him for a name Juan Francisco, his godfather was Manuel Buitron, corporal of the guard, and in order that it should be recorded I sign it, Fr. Matias Antonio de Santa Catarina."

It is uncertain how much weight this statement should be given. The phrase "Nao de China" (ship from China") is probably the priest's own interpretation of the old man's statements; it seems unlikely that he himself would have known where the ships came from, much less the Spanish expression. Moreover, Sargentaruc was near the Big Sur River (Culleton, 1950, p. 86), and would be an unlikely spot for the galleons to harbor. The suggestion, however, is strong and tempting.

More definite information than these rather shadowy and dubious suggestions could, perhaps, be found in the records of the Manila trade. For the present study, this has not been investigated. However, it is of some importance; it might mean a hundred and fifty years of occasional contact with whites, instead of one contact in 1602 and then none until a permanent settlement was made in 1770.

1769 saw the first attempt to establish a Spanish settlement in Monterey. This was the date of the first Portolá expedition, travelling north overland from San Diego to find Monterey to establish a mission there. The Spanish had several reasons for making settlements in California at this time. The Franciscans saw there a virgin field for missionary work. The government and the military were beginning to be afraid of Russian and English expansion. They

saw that if they did not soon establish a port at Monterey, the only harbor they knew of on the coast, the Russians or the English would be there before them and have a port actually within the Spanish domain. So the Franciscans, under Father Serra, established a mission at San Diego, and Portolá and his party went north to find Monterey. They carried with them, as a guide, Cabrera Bueno's aforementioned work on navigation. The accounts of the trip written by Crespi (Palóu, 1857, VI:285-423), Costansó (Teggart, 1911, and Costansó, 1950), and Portolá himself (Smith and Teggart, 1909) show that they did not have an easy time. They travelled up the coast until the Santa Lucia mountains made further progress impossible; then they turned inland and came out in the Salinas Valley, which they followed to the sea.

There, they found themselves on the shores of a great sweep of bay. They recognized Point Pinos and Point Ano Nuevo, the north and south limits of Monterey Bay. But although they explored up and down the coast, they were unable to locate the famed harbor they were supposed to take possession of, "el puerto de Monterrey". They could not find anything that looked like a harbor to them. Carmel Bay attracted their attention, but it did not fit the description in Cabrera Bueno, which seems to have been their sole on-the-spot source of written information: although they speak of Vizcaino, there is no sign that they had any of his writings with them. Following a set of sailing directions backwards on land must have been confusing at best, especially for a party of landsmen. However, the real difficulty seems to have been that they did not realize, either from their available information or from what they could see, that the famous harbor was merely the southeast corner of Monterey Bay. Although it is well-sheltered from prevailing winds, as nearly all the early writers (including Cabrera Bueno) emphasize, there is deep water close to shore and the bottom is sand, these features were not easy to recognize from land. From the sea, it seems to have been much easier to recognize; when the San Antonio arrived in 1770, she does not appear to have had the slightest difficulty finding the sheltered anchorage.

Failing to find any satisfactory harbor between Point Pinos and Point Ano Nuevo, the confused party decided that their destination must still lie ahead of them.

Whatever the reason for their confusion, they decided that this was not Monterey Bay, and that it must lie ahead. So, on October 6, 1769, they left their camp at the then mouth of the Salinas River, and proceeded northwards. Travelling up the coast, they came to what was then known as San Francisco Bay, and now as Drake's Bay, which they recognized by the Faralones and Point Reyes. They then realized that they were too far north, and that Monterey Bay must lie behind them. So they decided to retrace their steps, and on November 28, 1769, arrived once more at their old camp at the mouth of the Salinas; they later moved to the mouth of San Jose Creek, south of the Carmel River. They stayed there until December 10, waiting for the sea expedition which was supposed to join them. Food was running desperately

short. When the ships did not arrive, they decided to risk staying no longer. They set up two crosses--one on the Monterey side of the peninsula, the other at the mouth of the Carmel River--and set off again for San Diego.

While at Monterey in 1769, the diarists made little mention of Indians. Crespi, on Dec. 1 states (Palóu, 1857, VI:354) (7): "We did not see a single Indian hereabouts" (they were then camping at the mouth of the Salinas). On November 30, apparently while camping at the mouth of San Jose Creek, he states (Palóu, 1857, VI:392) (8):

> "This afternoon there came to the camp some ten or twelve gentiles who said that their rancheria was up the valley of the river that empties in the estuary /i.e., up the Carmel Valley/. They brought us their gift of a good ration of pinole and seeds which they distributed among the people and the Commander /Portolá/ replied with a few glass beads."

On December 5, the following statement was written by Crespi, and gives a clear picture of the confused and disappointed state of mind of the party (Palóu, 1857, VI:395) (9):

> "At Point Pinos no port is to be found nor have we seen on all the route more unpopulated country than in this neighborhood nor people more rough than are to be seen in this diary, considering to the contrary the voyage of Commander Sebastian Vizcaino; that Monterey was well populated with the very best of gentiles, although this is easier to get confused than a port as famous as was Monterey in past centuries;..."

Costansó's diary for that day reads very similarly (Teggart, 1911, p. 120) (10):

> "We must also say, that land more depopulated than those situated in the aforementioned latitudes principally at the end of the Sierra de Santa Lucia, we have not seen in all the journey, nor people more rough, nor more savage, than its natives: Where is then the populous/land/ which those of old so greatly emphasized, and the extreme docility of its inhabitants".

It should be noted, Wagner (1923-27, VII: fn. 170) to the contrary not withstanding, that the Portolá party saw some Indians near Monterey Bay, although very few.

Another land expedition went up the coast from San Diego in 1770, while another was sent by sea. The land expedition arrived at Monterey May 23, 1770; the San Antonio arrived on May 31, and stayed until July 9. The mission of San Carlos Borromeo was established, somewhere near the site of the present Royal Presidio Chapel in Monterey, and a presidio built. Palóu (1787, p. 103) states (11):

> "On the same day when possession was taken of the port.. the Mission was founded with the appropriate name...The gentiles did not let themselves be seen in those days, because naturally they were terrified by the multitude of artillery and musketry shots that the troops let off; but in a little while they began to come near..."

The first local Indian, a five-year-old boy, was baptized on December 26, 1770.

Costansó, in his official diary covering both Portolá expeditions, gives the following description of the Indians (Costansó, 1950, pp. 63-64; von Hemmert-Engert and Teggart, 1910, pp. 64-67) (12):

> "The natives of Monterey live in the hills, the nearest about one and a half leagues from the beach. They come down sometimes and go out fishing in little rafts of reeds. It seems, however, that fishing does not furnish their chief means of sustenance, and they have recourse to it only when hunting yielded little. Game is very plentiful in the mountains, especially antelopes and deer. These mountaineers are very numerous, extremely gentle and tractable. They never came to visit the Spaniards without bringing them a substantial present of game, which as a rule consisted of two or three deer or antelopes, which they offered without demanding or/even/asking for anything/ in return/. Their good disposition has given the missionary fathers well-founded hopes of speedily winning them over to the faith of Christ."

This description forms an interesting contrast to Costansó's remarks on the first expedition.

In 1771, it was decided to move the Mission from the shores of Monterey Bay to the banks of the Carmel River, some five miles to the south. There was a better water supply there, and the fertile land of the Carmel Valley to cultivate. Also, doubtless, Serra did not relish the idea of his mission being in the middle of a military establishment, particularly with the irascible Catalan, Pedro Fages, in charge of it. The move was started at the beginning of August, and not completed until the end of December. Palóu (1787, p. 474) says of the new site (13): "In the neighborhood of the mission there are various rancherias of gentiles, that after the founding of the mission began to frequent it, and their reduction soon began..."

After a severe initial struggle with problems of supply, the population of the mission began to grow (see Appendix 1, Table 1). However, it never became one of the largest of the missions. The number of neophytes living at the mission never seems to have exceeded seven or eight hundred.

Vessels began stopping at the new port. The packet San Antonio came every year with supplies. Several Spanish voyages to the Northwest coast stopped there; in 1775, Juan de Bodega y Cuadra was there from August 29 to September 11 (Palóu, 1857, VII:243, 247). The first foreign visitor to Monterey was Lapérouse, in the ships Boussole and Astrolabe, from the fifteenth to the twenty-fourth of September, 1786. The account of his voyage contains considerable material on the Indians at the mission.

Lapérouse was followed by others on voyages of discovery: Alejandro Malaspina was there in 1791 from September 12 to September 25. Although he apparently questioned the Indians at the mission with the aid of two native interpreters, the publication of his papers (Malaspina, 1885) gives little of the material he obtained. The publication was by no means complete, and there is doubtless much more material from this voyage in the archives of the Dirección Hidrográfico in Madrid.

In 1792 the voyage of the Sutil and Mexicana stopped in Monterey from September 23 to October 22, on its way to the straits of Juan de Fuca. George Vancouver stopped there from November 26 of the same year to January 14, 1793. He returned the next November, but stayed only the first three days. In 1794 he returned again, and stayed from November 6 to December 2. In 1796 a Frenchman, Péron, stopped in Monterey from November 31 to December 8.

After Péron's visit, there is a long gap when no major voyagers seem to have visited Monterey. Humboldt gives an account of Monterey in his description of his voyage of 1803, but it is clearly quoted from the voyage of the Sutil and Mexicana, and was probably obtained from the Mexican archives. The only reliable information located for this period is the reply written by Father Amorós in 1814 to the 1811 questionnaire sent to the missions by the Spanish government.

The next voyager to stop at Monterey was Captain Beechey, in the ship Blossom. He was there from the first to the fifth of January, 1827. The account of his voyage gives little information concerning the Indians of San Carlos Mission. In the same year, the ship Héro, commanded by Duhaut-Cilly, stopped in Monterey from March 9 to March 27. She returned in 1828, and stayed from the third to the thirtieth of May.

In 1834 Mission San Carlos, along with all the other Spanish missions in California, was secularized according to the decree of Governor Figueroa. Already poverty-stricken, with few Indians left, it began to fall into ruins.

In 1936 Ruschenberger visited Monterey from the twenty-fourth to the thirtieth of October. In 1837, the French frigate Vénus, commanded by Abel du Petit-Thouars, was amchored at Monterey from October 18 to November 14; one of her seamen died there and was buried at Carmel Mission. In 1839, the Artémise, under Laplace, was there from August 24 to September 5. The illustration of Mission San Carlos in the account of Laplace's voyage shows it already

in a state of some disrepair, but by no means in ruins. T. J. Farnham visited the mission on April 25, 1840; he describes it as "ruined" and almost forsaken. 1842 saw two travellers come to Monterey: Sir George Simpson of the Hudson's Bay Company, from January 15 to 19 and Eugene Duflot de Mofras.

By the 1840's there were few Indians left in the vicinity of Monterey, and those that remained were, as far as can be gleaned from the literature, completely missionized. The last pagan Indian in the area under the jurisdiction of Mission San Carlos had been baptised long before, on September 17, 1808 (Culleton, 1950, p. 168). In later years the sources speak little of the Indians; they are more interested in recording the quaint customs of the Spanish Californians.

Terminology

The name applied to the Costanoan speakers of Carmel Mission is Rumsen or Runsien. It appears to have been the name of one of several Costanoan villages within the area taken by Carmel Mission for proselytizing purposes. There is no indication that it was the principal village of the area, nor, indeed, that there was any super-village organization of any description. It has been, and is here, used simply to distinguish between the Costanoan and Esselen speakers at Carmel Mission.

The earliest explorers gave no name to the Monterey Indians. None of the Vizcaino accounts make any mention of tribal or village names. Neither do the accounts of the two Portolá expeditions. The first indication of any subdivision of the Indians of the region appears in Fages' account (Fages, ms, p. 52). He divides them into those of the port and the mountaineers of the Santa Lucia range, which appears to correspond to the linguistic Costanoan-Esselen division. The only tribal or village name mentioned by him is Zanjones, six leagues on the road to San Diego, who sometimes troubled travellers (ibid., p. 51). They were Esselen.

The first writer to apply general names to the Indians of the region was Lapérouse, or rather de Lamanon, who wrote the linguistic notes on the Indians of Mission San Carlos. He states (Lapérouse, 1797, II:289) (14): "Monterey, and the Mission of San Carlos which depends on it, includes the country of the Achastliens and the Ecclemachs." And (ibid, p. 290) (15): "The country of Ecclemachs stretches out at more than twenty leagues to the east of Monterey; the language of its inhabitants differs absolutely from all those of their neighbors.." Achasta was another Costanoan village near Carmel Mission (Pillin, ms. 1, p.6), from which the term Achastliens is clearly derived.

The term Runsien appears to have been used first in the account of the voyage of the Sutil and Mexicana. Here it is stated (Navarrete, 1802

p. 164) (16): "The Indians who come to this Mission, are those of the Runsien or Eslen tribes which are the principal ones, or of those of the Ismuracanes or Aspaniaques..." and (p. 167) (17): "From the information that our Missionaries have been able to acquire concerning the customs of the two nations, Eslen and Runsien, who occupy all of northern California /sic/, it seems that the former is the most numerous, and that the individuals of both are wandering and scattered..."

Malaspina also used the term Runsien. Although he visited Monterey before the Sutil and Mexicana expedition, his account seems to have been written after the account of that voyage was available, since he refers to it. His usage of the term must therefore be considered later than that in the aforementioned voyage. He stated (Malaspina, 1885, p. 443) (18):

> "Three tribes, different and perpetual enemies among themselves, live now united in this mission...These tribes are called that of the Runsien, of the Eslenes and of the Vaysh, to which /plural/ follow towards Santa Clara and San Francisco those of the Ymuracan and Aspaniac..We will carry no further the thread of our comparisons, and he who wishes to continue them, may read in the relation of the voyage to the Straits of Fuca the information that has been inserted there concerning these natives and it conforms to that acquited by us in the campaign of 1791...."

Who the Vaysh were remains unknown. No such name appears in Pilling's list (ms. 1, p.6) of village names, and Harry Downie (personal conversation) had never heard of it in the mission records.

Doubtless more material on these tribes exists in the archives of the Dirección Hydrográfico in Madrid, with the rest of the documents from Malaspina's expedition. The 1885 publication was only partial, and the remainder of the material is not at present available.

In reply to the 1812 questionnaire it is stated (Amorós, 1950, pp. 468 and 477):

> "Seven Indian tribes live at this mission. They are the Excelen and Egeac, Rumsen, SargentaRuc, Sarconenos /p. 468: Sanconeños/, Guachirron, and CalendaRuc. The first two are from the interior and have the same language or speech, which is totally distinct from the other five, who also speak a common language."

And (pp. 468 and 478):

> "These seven tribes speak two languages: the one is Rumsen, the other Excelen; they differ entirely."

Excelen and Egeac were Esselen villages. Of the five Rumsen "tribes" listed by Amorós, Rumsen, SargentaRuc, and CalendaRuc are given by Pilling (ms. 1, p.6) as Costanoan villages, as Rumse-n, Sirhin-ta-ruk, and Kalinta-ruk. "Sanconenos" and Guachirron (Wacharo-n) are given as Costanoan villages of unknown location.

As noted above, "Sarconenos" is given in the Spanish version as "Sanconeños". Since "-eños" is a Spanish suffix meaning "dwellers of", the name of the village would not be Sanconenos, but Sancon-; certain final syllables are dropped before this suffix. It is possible that the village referred to is Los Zanjones (Spanish for "the deep ditches"). Culleton (1950, p. 271) identifies this with Ensen, an Esselen village. The same writer (1950, p. 174) considers it unlikely that Father Amorós knew either language; if this is true, it would probably explain the confusion, if such it is. Culleton (1950, pp. 206 and 271) seems to regard Guachirron as part of GalendaRuc, on the lower Pajaro River.

The Rumsen language is a member of the Costanoan group. It appears to have been so considered by Powell when the group was first established by him (Powell, 1891, pp. 70-71). His opinion seems to have been derived from suggestions by Latham (1853, p. 78; 1856, pp. 83-84) that a group of languages near San Francisco Bay, called by him Costano, were related to Rumsen or Ruslen (which, however, was placed in a "Salinas" group, with Esselen and apparently Salinan) and the Soledad dialect, and also to Mokelumne. A further source for the Costanoan group was Gatschet's statement (1877, p. 157):

> "Mutsun--This name, of unknown signification, has been adopted to designate a family of dialects extending from the environs of San Juan Bautista, Cal., in a northwestern direction up to and beyond the Bay of San Francisco and the Straits of Karquines, in the East reaching probably to San Joaquin River. It is identical with the language called Runsien or Rumsen..We can distinguish the following dialects: - San Juan Bautista;..Mission of Carmelo, near the port of Monterey; the Eslenes inhabited its surroundings. Santa Cruz,...La Soledad Mission;../and/ Costaño,..."

Gatschet suggested that these languages were related to Mokelumne and Miwok; Powell in 1877 (1877, pp. 536-537) followed this, but in 1891 (1891, p.70) repudiated it. The notion was, however, not killed; it was still available to Kroeber in 1904 (Kroeber, 1904, p. 69). In 1910 Kroeber again presented the possibility of relationship between Costanoan and Miwok (Kroeber, 1910, p. 259). This was followed in 1919 by the establishment of the Penutian family by Dixon and Kroeber (1919, pp. 48-102). It then consisted of Costanoan, Miwok, Maidu, Wintu, and Yokuts, and has since been extended to include certain languages outside California. Hence, the Rumsen language is now considered a member of the Costanoan group of the Penutian family.

Region

The Rumsen, for the purposes of this paper, are those people of Costanoan speech who inhabited the area served by Mission San Carlos Borromeo del Rio Carmelo, and who were in course of time incorporated into that mission. Such a definition implies the area of this group; however, it is defined in terms of the mission, and not in terms of native groupings. It has, therefore, no real significance from the point of view of the aboriginal ethnographic situation, and for this reason no attempt has been made in the present paper to define its boundaries. There does not appear to have been any native grouping larger than a village. It is now impossible, except in a very few cases, to separate the data on the Rumsen from that concerning the Esselen living at Carmel Mission, let alone to separate the information on specific Rumsen villages. The historical records speak only of the Indians of Carmel Mission, and often they are not even that specific. At the present time, the only possible way to obtain information on Mission tribes that are now extinct seems to be to take them in the groupings formed by the missions, remembering always that these are Spanish, not aboriginal groupings and areas.

In general, the area under consideration is the southern half of Monterey Bay, the Monterey Peninsula, Carmel Bay and a short distance up the Carmel Valley, and down the coast about as far as Big Sur. This area consists of the coastal plains of the Salinas and Carmel valleys, which are separated by a line of hills terminating in the pine-forested Monterey Peninsula; and the seaward slope of the northern part of the Santa Lucia mountains. For such a limited area, the habitat is quite surprisingly varied: well-watered valley floor, steep mountains, pine forest, several types of sea-coast, and the marshy lagoons on both sides of the Peninsula. It still supports a richly diversified flora, and in former times a similarly varied and abundant fauna, several of the larger members of which are no longer to be found. Trees growing there include the Monterey pine and cypress, oaks, willows, both black and white poplars, alders, and maples (Ascención, ms, ff. 74^{v} and 81^{v}; Vancouver, 1798, II:35). Several berry plants are also to be found: blackberries, huckleberries, and strawberries. Piñons grow in the Santa Lucias. The land fauna included bears, wildcats, mountain lions, elk, deer, antelopes, cottontails and jackrabbits, foxes, squirrels, and a great variety of birds. Aquatic fauna included quantities of fish and shellfish, among which should be mentioned sardines, steelhead, mussels, and both black and red abalones as likely to have been important food resources; whales, seal, sea otters, and such aquatic birds as gulls, cormorants, and pelicans, besides beach birds and those of the lagoons. Sea otters were believed to have been hunted out of existence by 1842 (Duflot de Mofras, 1844, p. 394); however, in recent years they have made a comeback in this area, and may often be seen off various coastal points.

Physical appearance, costume, and ornament

Early travellers, except those of Vizcaino's expedition, all seem to

have regarded the Monterey Indians as a physically rather unprepossessing group. They are described as being short or of medium stature, rarely running to fat; dark skinned, with straight black hair, thick black eyebrows, small, deep-set black eyes, prominent cheeks and cheek-bones, a short nose depressed at the root, a large, thick-lipped mouth, and good teeth (Lapérouse, 1797, VI:37-38; Petit-Thouars, 1841, II:112). Rollin gives a series of physical measurements taken at Monterey (see Appendix 1, Table 2). In short, the Rumsen do not appear to have differed significantly from most other Californian groups in their physical appearance. Most writers remark on the lack of intelligence of these Indians; however, such observations of eighteenth century writers are so likely to be heavily weighted with cultural factors that they say less about the observed than about the observers, and consequently have little or no scientific value.

Ascención and Navarrete state that the Indians of Monterey went naked (Ascención, ms. f. 83^{v}; Navarrete, 1802, p. 167). This appears to be an exaggeration, since other writers describe a specific costume; however, Menzies (1924, p. 293) states that the men went naked. It seems likely that at least a large proportion of the men went naked most of the time. When they wore anything, it consisted of a loincloth of otter-skin (Lapérouse, 1797, II:271), and a rabbitskin cape from shoulders to waist, tied under the chin with a string (Lapérouse, loc. cit.; Fages, ms, p. 53). Lapérouse further states that some of the men wore well-woven basketry hats; Merriam (ms., p.14) denies this, although he mentions a head kerchief woven of fine grass, longer than broad, which was worn in the hot sun (see also p. 63).

Female costume consisted of a piece of buckskin or sea-otter skin draped around the hips, falling to the knees, and an apron of braided/?/ tule, apparently sometimes dyed or painted red and white. Over their shoulders they wore a cape of buckskin. Girls under nine years of age wore a girdle only; small boys went naked. Early sources make no mention of shoes, and Merriam states that no moccasins were worn; Petit-Thouars (1841, II:113) states that women wore a piece of cowhide attached to the feet as shoes. (Fages, ms, p. 53; Laperouse, 1797, II:271; Menzies, 1924, p. 293; Merriam, ms. pp. 13-14).

Red ochre was worn as body paint by warriors in time of war. Tattooing was unknown. Ornaments of abalone shell were worn, and ears were pierced for the attachment of ornaments. A necklace of spherical beads was worn; it was made from a white stone splashed with blue that came from Hash-show'-wen (Jamesburg), the home of the Esselen in the mountains southeast of Carmel. It was also used as money. The chief wore a headdress of white seagull down; dancers, of flicker feathers. A nose-stick was not worn. (Laperouse, 1797, IV:39; Malaspina, 1885, p. 371, fn.1; Navarrete, 1802, p. 169; Beechey, 1941, p. 68; Merriam, ms. pp. 14-15).

Laperouse and Menzies state that the hair of both sexes was worn cropped to about four or five inches from the head, this operation being done with a lighted brand (Laperouse, 1797, II:271; Menzies, 1924, p. 293). Petit-Thouars

(1841, II:113) states that the women wore their hair loose over their shoulders, or with a cord tied around the head dividing it into three tufts, one on each side and one at the back. This may represent a change in fashion over the forty-year period between the visits of Menzies with Vancouver, and Petit-Thouars.

Rollin (Laperouse, 1797, IV:38) states that both men and women removed beard and body hair by plucking with bivalve shells or with a stick split at the end. Menzies, however (1924, p. 293), states that the men wore long beards; this sounds rather improbable.

Economy

The economy of the Rumsen was on a hunting and gathering basis. Their diet consisted of acorns, wild oats, buckeye nuts, pinenuts, blackberries, strawberries, mushrooms, and seaweed among vegetable products; steelhead and other fish, abalone, clams, duck eggs, deer, antelope, moles, rats, and squirrels. They did not eat frogs, toads, or owls, these being the only animals they feared (Amoros, 1950, pp. 472 and 482). An unidentified fruit is mentioned by Fages (ms, p. 55) (19):

> "...and another wild fruit of the size of an ordinary pear, which they eat roasted or boiled, although it is a little bitter. The tree that bears it is rather whitish and like the fig tree, not very tall: when it bears fruit it is divested of its leaves."

The method of gathering pinenuts was to light a fire at the foot of the tree; after a few hours, it fell to the ground, and the nuts could be collected by hand without difficulty (Fages, ms, p.55). Wild oats mush was one of the principal foods before the padres came (Merriam, ms, p. 27).

Fishing was done from tule balsas (Costanso, 1950, p. 63). No early descriptions of fishing methods exist; Menzies (1924, p. 293) states that the women collected shellfish, but gives no indication of method. Pilling (ms. 2) states the following:

> "As late as the 1880's, the Indians still fished in the Carmel River for steelhead. It was called salmon then. The old folks used to camp along the river and wait for the steelhead to run. They could hear the fish in the rapids just above the mouth of the Carmel. When they heard the fish they would take their torches and go to the river and spear the fish. The torch was made of long stiff grass tied together at various places, so as not to burn all at once...The torch was kept near the fire ready to use. When the run started, the torch was lit in the fire and the old folks went out into the river. They used a three-prong spear with the prongs set out from the main stem at an angle. The blacksmith at Monterey used to make the spears for the Indians of iron. If they did not use these spears they used pitch forks."

A hunting disguise has been described by several writers (Vancouver, 1798, II:36; Menzies, 1924, p. 284; Navarrete, 1802, p. 164; Laperouse, 1797, II:251). It consisted of a deer-skin, with head and horns intact, the head being stuffed with dried grass to maintain its shape. In this attire the hunter joined a herd of deer, using his left hand as a foot and carrying his bow and arrow in his right. He imitated the movements of the deer, and pretended to browse; according to Navarrete, he imitated the behaviour of the opposite sex from that of the group of animals he was among. This he did so cleverly that Laperouse reports that at thirty paces the hunters of his company would have shot at the Indian hunter if they had not been prevented. When he came close enough to an animal--within two or three yards, according to Vancouver--he discharged his arrow from a stooping position, while the attention of the animal was directed elsewhere. The first or second arrow usually proved to be fatal.

The only hunting weapons reported are the bow and arrow (for description, see Weapons, p.64). With these they were very skilful and patient hunters. They crouched, hid themselves, and crawled near to the game, which they shot at a distance of fifteen paces or less. In this way they killed the smallest birds (Menzies, 1924, p. 294; Laperouse, 1797, II:250). Menzies states:

> "...they seldom missed their aim, when they saw us therefore miss a shot with our Fowling Pieces at a Bird flying, they would then exult in the superiority of their own mode & weapons by shewing us their dexterity."

During the periods of shortage of food supplies at the Mission, the neophytes obtained their own food according to their old methods. When Serra returned from Mexico in 1773, the Indians were all in Monterey at the beach in search of food; however, when the frigate arrived with supplies, they rejoined the Mission once again. (Palou, 1857, VI:608; VII:29; Laperouse, 1797, II:269).

According to Amoros, (1949, pp. 475 and 485), when a hunter killed a bear or a mountain lion, he would extract a claw or a tooth and wear it around his neck, as a token of his bravery. He was then respected.

Pilling (ms. 2) makes the following statement:

> "The chief used to tell the people when they were to go and get what food. At one time, when the acorns were ripe, they went and collected acorns. When the fish were running in the Carmel, they fished. When the duck eggs were ready to be collected, they went over to the marshes in the Salinas Valley and collected duck eggs. When the young ducks were just short of flying they went over again and caught the fledglings. In between times they stayed at the village and collected shell-fish for food."

The above statements apply to, probably, the last quarter of the nineteenth century, well after the end of the Mission period. It seems possible that this function of the chief was a carry-over from Mission days rather than an aboriginal pattern, the chief taking over the funtion of the departed padres. However, the pattern of work seems completely aboriginal: a sequence of gathering seasons with periods of dependence on the most permanent food supply, shellfish, in between. The wide range of collecting area is noteworthy: at that date, a group apparently living in the Carmel Valley had at least some collecting rights in the Salinas Valley. Since this was probably the last organized community of Indians in the area, and there were probably none such in the Salinas Valley, it is impossible to say whether such extended gathering rights were customary in prehistoric times.

Seeds (wild oats) were parched with hot coals on a flat, wide basket, called by Merriam a "snowshoe-shape winnower". Laperouse remarks upon the evenness and skill with which this operation was performed (Merriam ms, p. 22; Laperouse, 1797, II:268). Seeds and acorns were then ground. Laperouse, Peron, Petit-Thouars and Vancouver describe the mano and metate only for this process. Laperouse (1797, II:266) states that grain was crushed on a stone with a cylinder; Vancouver (1798, II:35) describes the apparatus as follows:

> "...two small stones placed in an inclined position on the ground; on the lower one corn is laid, and ground by hand by rubbing the other stone of nearly the same surface over it."

Petit-Thouars merely says that acorns were crushed between two stones (1841, II:117). Peron gives the following account of the milling operation (1824, p. 128) (20);

> "...fifteen to twenty Indians were sitting on their heels, having before them a flat stone two and a half feet long and one and a half wide; with both hands they held another stone, prismatic in form, with which they crushed the grain."

All of these, except for the vague account of Petit-Thouars, sound like metates and manos of the Mexican type which were in use by Spanish and Mexican families in Monterey until at least the last generation. Merriam (ms, pp. 19 and 22) gives words for bedrock mortar ("these and portable ones also used at Sur"); portable stone mortar; metate; mortar or milling basket, i.e. hopper mortar ("= stuck-on basket, because fastened to mortar by means of asphalt"); pestle of stone; and hand stone for rubbing (i.e., mano). All these implements are known archaeologically in the area, indeed, from the mission itself. It is probable that all were in use, in late prehistoric and historic times at least. They may have been used for different purposes; the sources yield no clue on this point.

Once ground, acorn meal was winnowed in the snowshoe-shaped winnower

above mentioned. Wild oats and pinole seeds were sifted in a basketry sieve. Acorn meal was next leached, sometimes in a burden-basket, to remove the tannic acid (Merriam, ms, p.22; Petit-Thouars, 1841, p. 119).

According to Petit-Thouars (1841, II:118-119), leached acorn meal was allowed to ferment somewhat, and then made into cakes which were cooked to keep for winter provisions. The same author (1841, II:113) states that food, presumably including acorn mush, was cooked by stone-boiling in water-tight baskets. It was not seasoned with salt or butter (Laperouse, 1797, II:266). Merriam (ms, p. 19) states that baskets were the only cooking vessels, and (ms, p. 22) that burden baskets were sometimes used to cook mush in. He also mentions an "abalone shell shovel on long wood handle...to carry hot stones from fire to cooking basket." (ms, p. 20). Mush of wild oats was one of the principal foods before the Padres came (Merriam, ms, p.27). Merriam gives the only clue to the way foods were served: he gives words for two kinds of "small mush or soup bowl (twined)", and a "very small/one/ (used for dipper & cup)." This seems to indicate that food was dipped out of the large cooking vessel with a very small basket, and served in small basketry bowls.

Kelp was dried in the sun and roasted in front of the coals before it was eaten (Merriam, ms, p. 28).

When plenty of food was avilable, the Rumsen would eat until they had gorged themselves (Amoros, 1949, pp. 471 and 481). They were described by Rollin as "moderate due to idleness, and gluttons in time of abundance" (Laperouse, 1797, IV:44). This habit of a succession of periods of gorging and starving was probably the result of the succession of gathering seasons mentioned on page 16, above; brief periods of plenty of perishable foods, followed by interims when food was harder to get and more monotonous.

Animal fat was regarded as a great delicacy (Laperouse, II:268).

Architecture

Two kinds of structures are known to have been built by the Rumsen: dwellings and sweat-houses. Dwellings were hemispherical huts, six feet in diamter and four in height. The frame was composed of boughs the thickness of an arm, which were embedded in the ground, and came together at the top to form a vault. This was covered with tules or broom (Merriam, ms, p. 16); Laperouse (1797, II:262) states eight or ten bundles of "straw" were used, and that two or three such bundles were kept in reserve near the hut in case of need. He says that more than half of the structure remained open, whatever the weather. However, he visited the Mission in September, when the weather is usually warm and dry. Further, in another place (1797, II:269) he says that when all the inhabitants were absent, the hut was closed with one bundle of straw. Merriam (ms, p. 16) states that the structure came to a point above, and was big enough for two families. Navarrete (1802, p. 165) mentions a foundation circle of stones or adobes, but his description of the structure is so vague that it is

difficult to know how much weight to give this statement. The only furnishings mentioned for these huts are skins laid out around the fire to sleep on (Laperouse, 1797, IV:45).

Merriam (ms, p. 20) states that these houses were large enough for two families. Laperouse (1797, II:262) states that in 1786 seven hundred and forty Indians, including children, were living in some fifty of these huts; that is, an average of about fifteen per hut. This seems to corroborate Merriam's statement.

During the mission period, the Indians were extremely difficult to dissuade from their aboriginal form of dwelling. According to Laperouse (1797, II:263), the Indians said that they liked the fresh air, and that when the house became infested with fleas it was a simple matter to burn it down and build a new one. To judge from the accounts of later travellers (Taylor, 1871, p. 136), this must have been no mean advantage. Further, the wild Indians are said to have been constantly on the move, and hence found it advantageous to have quickly-built dwellings, (Laperouse, 1797, II:263; Vancouver, 1798, II;34; Navarrete, 1802, p. 165). After the close of the mission period, the Indians lived in adobe huts with tile roofs (Farnham, 1850, pp. 99 and 101).

Menzies (1924, p. 294) says that the groups of Indians seen by him near Monterey Bay built no huts, "but generally kindled a fire in the open air near to where they collected their food and huddled together round it at night, covering themselves with Deer Skins & the Pelts of other Animals." These parties appear to have been groups of gentiles who came down to fish on the Bay. It seems likely that they were from established villages inland, and were merely camping on the Bay, and hence would not go to the trouble of constructing proper houses.

Sweathouses appear to have been semi-subterranean. They are described as a circular hole in the ground, covered with "a sort of bell" (presumably a conical roof, probably thatched) (Navarrete, 1802, p. 166). The entrance was low and narrow, and it was necessary to go through it on all fours; it had no covering. It does not appear to have been in the form of a tunnel, but simply an opening left at the side of the structure. The fire was built next to the doorway, and served as the only means of closing the entrance. No mention is made of the use of steam in sweating. The sweathouse held six to eight persons. (Fages, ms, p. 54; Navarrete, 1802, p. 166; Petit-Thouars, 1841, p. 120; Merriam, ms, p. 16). (For use of sweathouse, see Sweating, p. 76).

Merriam (ms, p. 16) describes what may represent a third type of structure. This is a "brush wickiup...large & square; framework of poles covered with brush and plastered with mud on outside." This description is on the line following that of "domed hut (tule or thatched)...Rook'...". No native name is given for the "brush wickiup". It is therefore not perfectly clear whether the description is part of the description of the rook', or whether it represents a

distinct structure for which Merriam did not obtain the native name. However, since the rook' is described as round, and the "wickiup" as square, and since no description of the domed hut mentions mud plaster on the outside, it seems likely that Merriam's "wickiup" is actually distinct from the domed hut. It may have been some kind of roofless sun- or wind-shelter.

Rollin (Laperouse, 1797, IV:45) says that preferred locations for villages were the edges of riverbanks and the sides of mountains that were exposed to the midday sun. Pilling (ms. 2) says: "The Indians always built high and away from water. They never lived on the creeks." It seems probable that high, sunny locations were preferred, and the banks of at least the Carmel River were avoided, perhaps owing to danger of flooding.

Basketry

According to Merriam (ms, p.22) all Rumsen basketry was twined, and none was coiled. It was all made from Salix nigra (ibid, p.23). Types of baskets included burden baskets, sometimes used also for leaching acorn meal and cooking mush; two kinds of seed paddle, one with a handle like a dipper for gathering oats, and one with a round bottom but no handle; a large flat-bottomed cooking bowl; two kinds of small mush bowl; a very small bowl, used as a dipper or cup; a "snowshoe-shape winnower...for winnowing acorn meal, like Piute winnower but broader at narrow end, used also to roast seeds"; a basket sieve, for sifting ground wild oats and pinole seeds; a baby basket; a trinket basket, to be described later; a small subglobular choke-mouth bowl, 3 to 12 inches in diamter, apparently used for carrying water; a hopper mortar basket, stuck to the base with asphaltum; and a storehouse basket (Merriam, ms, pp. 22-23). Laperouse (1797, II:271) mentions also a basketry hat, worn by the men; Merriam (ms, p. 22) denies the presence of such hats. Since more than a hundred years intervene between the visits of Laperouse and Merriam, it is possible that such hats may have fallen into disuse; on the other hand, it is possible that Laperouse may have confused his memory of the costume of the Indians of Baie des Français, which he visited before Monterey, with that of the Rumsen.

The trinket basket was a small bowl, decorated on the outside with abalone pendants and features--black ones from the crest of the California quail, and others of different colors. It was used to hold sewing materials, and was burned as a sacrifice when people died (Merriam, ms, p. 22); Petit-Thouars, 1841, II:113; Beechey, 1941, p. 69).

The storehouse basket was three to four feet high, and made of peeled willow rods fastened with willow bark. The bottom was covered with cattail leaves, and the top with madrone leaves. It was kept in the house and used for storing split acorn meats. (Merriam, ms, p. 23).

Weapons

The sole Rumsen weapons appear to have been the bow and arrow. The bow was well-made, sinew-backed, from three to four feet long; the bowstring was made of sinew. The arrows were about the same length as the bow, tipped with a neatly-worked flint point, "with jagged edges" (serrated?), which was attached by means of pine-tree resin. Both bow and arrow might be wrapped with sinew cord (probably at the grip and to attach the point). The Rumsen were skillful archers (Laperouse, 1797, II:272; Menzies, 1924, p. 294; Merriam, ms, p.20).

According to Rollin (Laperouse, 1797, IV:58), if the Rumsen used any poison on their arrows, it was not particularly effective. The Spaniards Rollin spoke to knew of no fatal arrow-wounds over a period of several years.

Miscellaneous manufactures

The only kind of boat in use by the Rumsen was the tule balsa (Merriam, ms, p. 21). It is mentioned by Costanso (1950, p. 63) as "balzita", that is, a small raft. It was used for fishing, apparently in the ocean. Judging from the fact that the Spanish do not describe the vessel further, it seems likely that the form was that with which they were already familiar from other parts of the New World to the south: a long narrow raft constructed of two or more bundles of tule, with tapering ends tending to curve upwards. There is nothing to indicate that it was any more or any less elaborate or well-made than the well-known form [the diminutive used by Costanso does not carry the derogatory meaning implied by certain other Spanish diminutive suffixes.] It seems probable that the balsa was paddled rather than poled, since Merriam (ms, p.21) gives a native word for "paddle", but not for "pole".

The fire drill used by the Rumsen was made of elder. Dry manure was used as tinder (Merriam, ms, p. 19).

The Rumsen tobacco pipe was straight, and made of cane (Merriam, ms, p. 20).

Two kinds of thread were in use: one was made of sinew, and was used as bowstrings and for wrapping bows and arrows, but not for sewing. The other was made of nettle, and was used for all kinds of sewing. Cord or rope was also made of nettle. (Merriam, ms. pp. 20-21).

A soaproot brush was "used for hair and other purposes". A carrying band for the head was "used here but name forgot" (Merriam, ms, p. 21).

Political and Social Organization

Comparatively little is known of the political and social organization of the Rumsen. The political unit appears to have been the village or settlement

unit, with little or no wider ties; Amoros (1950, pp. 468 and 477) states that the villages in the gentile state were continually at war with one another. Fages (ms, p. 51) says that war between the Indians of Monterey Bay and of the Sierra (i.e., the Esselen) was frequent. Malaspina (1885, p. 443) states that the Runsien, the Eslen, the Vaysh, the Ymuracan and the Aspasniac (all apparently villages) were in a constant state of war.

The chief appears to have been at least the principal governmental agency; he is the only one mentioned. He could be either a man or a woman (Merriam, ms, p. 32). He appears to have been a man who was respected for his prowess in war and possibly also the hunt. Navarrete (1802, p. 167) says: "...by his greater valor and skill in war he had obtained the subordination and obedience of the rest." Amoros (1950, pp. 474 and 484) makes the following statement:

> "The prominent Indians are the captains or kings. There is one for each tribe/nacion: probably "village"/. They command obedience and respect during their lifetime. This office is hereditary, or, in default of an heir by direct descent, it goes to the closest relative. This chief alone among the pagans could retain or desert a number of unmarried women; but if he had children by one of them, she was held in higher esteem and he lived permanently with her. He retained the privilege of living with unmarried women whenever he so desired. The entire tribe rendered service to him in the days when they were pagans, as well as now that they are Christians. He led the van in battle, supplied the bows and arrows, and encouraged his people. He was, as a rule, a very good archer. Even today they show more respect and submission to their chiefs than to the alcaldes who have been placed over them for their advancement as citizens. The chiefs are always recognized as elders and teachers of their tribes, even in the event that old age forces them to give the chieftainship over to a successor. They wear no distinctive mark of any kind. In the days of paganism, a cloak made from rabbit skins usually distinguished them..."

The sentence chosen by Amoros to illustrate the Rumsen and Esselen languages is also of interest in this regard: "Men who are good bowmen are esteemed and well liked." (Amoros, 1950, pp. 468 and 478). He also states (pp. 470 and 480) "At the close of their seed-harvest, the chiefs of each tribe customarily give a feast, at which they eat, sing and dance."

There seems to be no other evidence to corroborate Amoros' statement that the office of the chief was hereditary. It seems possible that it was not formally so, but that the son of a chief had a slightly greater chance of

succeeding his father than did another youth. All it may mean is that Amoros knew of one or two cases where the son had succeeded the father, and was generalizing from these. It seems more probable that the chief was simply a man who was well respected for his ability as a war leader, and thus became the natural leader of the group.

The duties of the chief appear to have been to lead the group in time of war; to advise and direct in other activities, including the gathering of food (Pilling, ms, 2; see above, p. 15); and, apparently, to give a feast at the end of the gathering season (acorn harvest?). One of the last functions of the Indian "capitán" late in the historic period was to arrange for the feast and festival of San Carlos Day at the Mission (Harry Downie, personal conversation).

Marriage

Evidence concerning the number of partners in marriage is somewhat contradictory. Navarrete (1802, p. 169) says that among both the Rumsen and the Eslen only one wife was permitted. Laperouse (1797, II:270) says that polygyny was permitted, and that a man might marry all the sisters of one family. Amoros (1950, pp. 474 and 484; quoted above) says that the chief alone might retain a number of "unmarried women", but that if one of them bore him children, she apparently became his principal wife. The statement of Laperouse may, perhaps, not apply to the Rumsen in particular; it may well have been hearsay obtained from the priests at the Mission, and refer to groups at other missions. The remarks of Navarrete and Amoros are not irreconcilable; Navarrete was probably considering the group in general, while the chief had a special status. However, it seems a little odd that Navarrete should omit to mention an exception to his blanket statement covering both tribes. It is the kind of detail that a European usually notices rather readily.

Marriage was arranged in the following manner. The parents or next of kin would decide that their son or daughter should marry a certain person. Having reached agreement, the consent of the marriageable child was asked. Marriages were sometimes made contrary to the wishes of one (or both?) partners, but such marriages seldom lasted. When consent had been obtained, the relatives went to see the intended. If he (or she) agreed, the contract was complete, and the man was free to go to eat at the home of his future bride. He procured for her a gift of beads or the like. His relatives contributed a share of this bride-price gift. When the bride was given to the groom, the gift was divided among her kinsmen. (Amoros, 1950, p. 471; Navarrete, 1802, p. 170) (22).

Barrenness and incompatibility appear to have been major grounds for divorce (Amoros, 1950, pp. 469 and 478). It is not clear whether adultery was so considered: Amoros (loc. cit.) states that it was the cause of much war and death in gentile times. Navarrete states that an unfaithful wife was not punished physically, but that her accomplice was cut or beaten with sticks, sometimes to the cost of his life. He says that the Eslen repudiated an unfaithful wife, but

not necessarily permanently, or turned her over to her new lover in return for indemnification for her bride-price. Such a specific statement implies that among the Rumsen, an adulterous wife was not divorced.

Fages (ms, p. 54) states that whenever married persons cohabited, they scratched each other.

Kinship

The kinship system of the Rumsen is fairly adequately recorded. The early sources yield no information on this subject. From their silence, it may be inferred that descent was probably patrilineal, since matrilineal descent would have been more likely to arouse comment from European visitors.

Three lists of Rumsen kinship terms exist: Merriam's (ms, pp. 2-4); Pinart's (Heizer, 1952, pp. 7-8); and Kroeber's (Gifford, 1922, p. 76). Of these, Merriam's is the most complete, while Kroeber's is the most phonetically reliable. Merriam's list was obtained from three female informants; Pinart's from one male; Kroeber's, from one of Merriam's informants. The following is a reconstruction from these three sources of the probable forms of kinship terms used by the Rumsen. The scheme of presentation is that of Gifford (1922); the phonetic symbols are those of Bloch and Trager (1942). A complete comparative listing of all terms given in the sources, in the original orthography, will be found in Appendix 1 (table 3). In both listings, the possessive prefix has been deleted, and replaced by a dash to show that the form given is a combining form. A question mark in parentheses indicates that the form occurs in only one source, without supporting evidence.

Parent class

ápa, - apan(?) F

isuin S, wmn. spk.
inšinš S, mn. spk.

ána, - a·n M

isuin ~ læčut (?) D, Wmn. spk.
kan D, mn. spk.

Grandparent class

pap Gf

ména, --men Fm

ána (?) Mm

meresens Gs

mersens (?) Gd

Sibling class

ták-a ~-s; --takan Ob

tan-a ~-s; --ta·n Cas

taú·sixs (form of Sibilants uncertain) Ysb

Uncle class

éte, --éten U — 'meresena Np

ánakans A — --mers (?), --kan, (?), kaná (--ná?) (?) No

Spouse Class

úrin H — xáwan W

Parent-in-law class

úxin ~ xowom (?) M1 — mars (?) S1

xowom (?) P1 — tíštan (?) D1

Sibling-in-law class

mers (?) ~ hau·nake (?) B1 — tíštan Ss1

A few details of explanation should be added to this listing. The form insins (Son, man speaking), given by Pinart, is supported by the Mutsun form inis given by Gifford (1922, p. 75); likewise kan, (daughter, man speaking) Mutsun ka. Merriam's form úxin (mother-in-law) is supported by the Santa Clara and Mutsun form uxi (Gifford, 1922, pp. 74 and 76); Kroeber's xowom (parent-in-law) resembles no other term in any Costanoan dialect, unless it be Santa Cruz howo (father's father) (Gifford, 1922, p. 74). Pinart's form uxi (aunt) resembles Merriam's Santa Clara and Mutsun forms for mother-in-law, whereas Merriam and Kroeber agree on anakans for aunt. It is likely that some confusion is involved. Kroeber and Pinart both give only one term for younger sibling; Merriam twice has the same term for younger brother as for younger sister; he has crossed it out both times, and has replaced it by tah'-kah, which is identified as brother. What appears to have happened is that Merriam asked for the term for younger brother, and heard a word which he associated with younger sister. Checking this, he is likely to have asked the informant, "Isn't that the word for 'younger sister'? What is 'brother'?, in reply to which he would get the only term that referred specifically to a brother, and meant "older brother".

The form for niece is rather dubious. Pinart gives "ca-mers o ca-can" for niece; "o" is certainly Spanish "or", and Pinart is here giving two forms, -mers and -can. Merriam gives the form kah-nah', cousin or niece. In every other form where kah- is prefixed, Merriam specifies that it means "my", but not in this case. If kah- is part of the term, it looks very much like Pinart's -can (/kana, kan/). Pinart's -mers appears to be phonologically identical with the forms given by Kroeber for "son-in-law" and by Merriam for "brother-in-law".

The form for brother-in-law presents another problem. Merriam gives mãrs (/mers/), Kroeber gives hauunake. These may well be alternative terms. However, mers is the same form as that given by Kroeber for son-in-law; and Kroeber's

terms for daughter-in-law and sister-in-law are identical with each other. If symmetry is to be expected from the system, mers is probably the term to be preferred.

Apart from the above considerations, which concern simply the forms of terms of kinship, a few details of the system merit particular attention. One is the fact that the terms for nephew and grandson are identical. The same trait occurs in the Mutsun system; and there the reciprocal terminological equation occurs - namely, uncle equals mother's father (Gifford, 1922, pp. 75-76). According to Gifford (1922, pp. 248-249), these equations are two of twelve associated with marriage to the wife's brother's daughter, either polygnyously or as a widower. The same author states (op. cit. p. 252) that this appears to be primarily a Penutian institution, reaching a maximum expression among the Wintun; and points out (op. cit. p. 253) that such equations may result from "diffusion of the marriage institution, the resultant terminology, or both."

A second point to be noted is the merging of terms for siblings-in-law with those for children-in-law. Concerning this, Gifford (1922, p. 180) makes the following statement:

"Among relatives by affinity, those of the parent-in-law classes and sibling-in-law classes are as a rule denoted by special terms...Hence the merging of parents- and children-in-law in siblings-in-law is of peculiar interest. It occurs among the Colorado river Yuman tribes, among the Yokuts, and sporadically with the Rumsen and Southwestern Pomo."

The classification of offspring among the Rumsen is also interesting, although not quite clear. Three or--possibly--four terms were employed: man's son, man's daughter; and probably woman's child, although Merriam gives two terms for this. The Mutsun had such a three-term system; otherwise, only the Yuman tribes possessed it. Four-term systems were restricted to the Southern Athabascans in California (Gifford, 1922, p. 126).

The classification of grandparents involves a somewhat similar problem. The terms for father's father and mother's father were clearly identical, and hence equal grandfather. However, Merriam gives different terms for father's mother and mother's mother; the other two sources give simply "grandmother". In the case of Pinart, this probably means that "grandmother" was all he asked for. Kroeber, however, may have asked for both mother's and father's mother, and received identical terms. Since Merriam's term for mother's mother is identical with that for mother, it is possible that some confusion occurred. Whether or not this is so, the classification was one of two types: two-term, grandfather -- grandmother, or three-term, grandfather --father's mother -- mother's mother. In the first case, Rumsen would be aligned with fourteen other California tribes, including most Penutian groups, in the second commonest Californian system. If the system was three-term, Rumsen would belong to

a group of tribes, all on the borders of the Penutian group, with what Gifford calls a "hybrid" nomenclature. They all occur sandwiched between the Penutian system and a four-term system. The systems of the tribes immediately neighboring the Rumsen are unknown (Gifford, 1922, pp. 128-129, and map 2).

Crime, punishment, and quarrels

According to Laperouse (1797, II:269), theft was unkown among the Rumsen in 1786, despite the flimsy structure of their houses. No home had anything to tempt the cupidity of its neighbors. Navarrete (1802, p. 171) found the same condition in 1792. By 1814, stealing at least from the Spanish had increased to such an extent that Amoros (1950, pp. 473 and 483) placed it first in a list of the "vices" of the Rumsen: they called it "no more than taking." However, on the same pages Amoros states: "They give, lend, and make agreements (23) not as strangers, but as brethren." This statement implies some laxity in the concept of personal property; a laxity which would not be appreciated by the Spanish. There is no mention of native punishments for stealing.

Laperouse (1797, II:275) states that homicide was very rare, even among the wild Indians. It was punished only by general contempt. Navarrete (1802, p. 171) states that it was regarded almost with indifference; he contrasts the Eslen, who punished homicide with death. If a man was killed by a group attack, it was supposed that his fate was deserved, since he had attracted so many enemies (Laperouse, 1797, p. 275). Adultery was punished on the person of the male partner (see above, p. 67) (Navarrete, 1802, p. 170).

Duplicity and lies were not so common in the gentile state as they became after missionization. The neophytes lied brazenly in 1814 (Amoros, 1950, pp. 473 and 483).

If two men quarrelled, they fought each other with the bone spatulas used in the sweat bath, which they carried at all times. As soon as blood was drawn, no matter how little, the battle was over, and the contenders reconciled, even if it were over a major affront (Fages, ms, p. 53).

Life Cycle

The sources have little to say concerning the course of pregnancy. Rollin (Laperouse, 1797, IV:54) states merely that women suffered little inconvenience during gestation. This statement appears to be obstetrical rather than cultural. Nothing is known concerning the presence or absence of pregnancy tabus.

Childbirth was easy, and labour short, Old women acted as midwives. As soon as the child was born, the old women cut the umbilical cord and bathed the child in cold water. The mother immediately went and washed herself in the sea or in a stream. On coming out of the water, she sat on a hot stone and was covered with skins, and sweated until the stone grew cold; she then plunged once more into

cold water. This procedure was sometimes repeated on several following days (Laperouse, 1797, IV-54).

Amoros describes a somewhat different post-natal sweatbath (1950, pp. 472 and 482). A hole was made in the floor of the hut; a fire was built in it and rocks heated there. When they were hot, they were covered with green herbage, to form a sort of mattress. The mother and new-born child lay on this; the woman sweated profusely, and the child was kept warm. This was done for six or seven days, after which the mother was asvigorous as if she had not given birth.

When accidents occurred (in the sweatbath? or in the course of delivery?), the midwives treated the affected parts with a fomentation made from a decoction of emollient seeds like linseed, called passelle (Laperouse, 1797, IV:55).

Abortions were not very rare. The woman behaved in the same way as in the case of a normal delivery, unless she suffered hemorrhage. In this case, she remained in bed, and cold fomentations were applied to the genitalia (Laperouse, 1797, IV:55-56). Difficult deliveries or abnormal presentations usually resulted in the death of both mother and child (Laperouse, 1797, IV:54).

Infants were given the breat until they were eighteen to twenty months of age, normally. However, the period had no definite limits; sometimes it was very short (Laperouse, 1797, IV:56).

Rollin (Laperouse, 1797, IV:56) and Petit-Thouars (1841, II:113) have left brief descriptions of cradling practises. The infant was wrapped in skins, the limbs being aligned along the body and held there by strips of leather. He was then placed in "a/piece of/ tree bark, of a size proportionate to the child, and in the form of a /mission?/ tile" (Rollin) or "a kind of basket made from tree bark" (Petit-Thouars), to which he was held by bands or strips of skin. This cradle was almost certainly a basket; Merriam (ms, p. 22) gives a Rumsen word for "baby or papoose basket". It is not perfectly clear whether this cradle was carried on the mother's back; it probably was. A child might also be carried piggy-back on the hips, held there with a strip of untanned leather (probably buckskin) (Petit-Thouars, 1841, II:113). This statement sounds as if it might apply to an older child rather than to a very young infant.

Children were much loved by their parents, and were never struck except when they showed meanness in childish squabbles. As the children grew older, however, they cared little for their parents. They scarcely recognized their father, although their attachment to their mother lasted longer. Girls would help their mother all their lives (Amoros, 1950, pp. 469 and 478; Laperouse, 1797, II:272).

There appears to have been little formal instruction of children. Boys were taught the use of the bow and arrow; girls probably learned to perform household tasks from their mother, since they are said to have assisted her (Amoros, 1950, pp. 469 and 478).

Merriam (ms, p. 31) gives a native word for a puberty dance. Apart from this, no accounts of puberty ceremonies have been found in the literature. As in every other case where information is lacking, this does not necessarily mean that they did not exist.

Marriage arrangements have already been discussed in another connection (see p. 66 above).

When old age made it impossible for a man to support himself by hunting, he was provided with food by the rest of the village. The old people were treated with respect (Laperouse, 1797, II:272). Old women might become midwives (Laperouse, 1797, IV:54). At least one important religious function was also open to them; they could also become shamans (see below) (Amoros, 1950, pp. 469-470, 479).

According to Laperouse (1797, II:273) the dead were usually cremated. However, Fages (ms, p. 53) and Amoros (1950, pp. 473 and 482) both state that burial was the custom; and Merriam (ms, p. 30) denies the presence of cremation. There was no special cemetery. A hole was dug, and the deceased placed therein. An unweaned child was buried with its deceased mother if the family was unable to care for it. Clothing, beads, and seeds were placed with the corpse. (Fages, ms, p. 53; Amoros, 1950, pp. 473 and 482). Merriam (ms, p. 29) states that abalone shells were used to dig the grave, and that the dead were buried sitting up, wrapped in skins. Amoros (1950, pp. 470 and 480) states that all the belongings of the deceased were destroyed; clothing and goods were burned; in mission times, animals were killed and plants uprooted, all in order to eradicate the memory of the departed. However, Navarrete (1802, p. 172) states that this was specifically an Eslen custom, and that the Rumsen divided the property among the relatives of the deceased.

Mourning involved cutting the hair--if no knife was available, it was burned off a little at a time--smearing the body and face with ashes; and loud and profuse weeping. At the funeral of Father Serra, the weeping and wailing of the neophytes drowned out the singing of the choir (Palou, 1787, p. 280). Old women smeared their faces with pitch, the effects of which lasted for months. At the funeral of a chief, the whole tribe /village?/ gathered together to mourn around the corpse, and the ceremony lasted sometimes for four days (Amoros, 1950, pp. 470 and 480, 473, and 482-483; Navarrete, 1802, pp. 171-172; Laperouse, 1797, II:271).

There was a strong tabu on the name of the dead, as is usual in California. If a child was orphaned at an early age, no one would tell him the name of his

parents. A person was offended if the name of a dead relative was mentioned in front of him,"Your father is dead" was a severe insult (Laperouse, 1797, II:271-272; Amoros, 1950, pp. 470 and 480). According to Harry Downie (personal communication) traces of this tabu still persist among the descendants of the Indians. They dislike talking about the dead people.

Warfare

Warfare among the Rumsen villages and with the Esselen was almost continuous, if the early accounts are to be trusted (Malaspina, 1885, p. 443; Amoros, 1950, pp. 468 and 477; Laperouse, 1797, II:272; Fages, ms, p. 51). According to Fages (Ms, p. 51), the major cause of war was transgression of collecting and gathering rights. When the coast Indians went into the mountains to gather acorns, the mountain Indians (Esselen) fought them; and vice versa, when the mountain Indians descended to the coast, Amoros (1950, pp. 469 and 478) states that in the gentile state, much war and killing arose from the infidelity of the women. The reference is not entirely clear, and may refer simply to severe corporal punishment for adultery. Amoros (1950, p. 473) (24) and Navarrete (1802, p. 168) state that the continual state of war was due to the persistent memory of old injuries and resentments, which made small transgressions sufficient cause for war. Wars, though frequent, were short, and casualties few; as soon as two or three had fallen, the others retreated (Navarrete, 1802, pp. 167 and 169).

Surprise is said to have been the main strategy. Sometimes, however, battles were fought by appointment. The time and place were set, and the chiefs advised their subordinates, who came with bow and arrows and "leather jackets" (cuera: perhaps some form of leather armor), painted with red ochre and wearing feathers. The women and children often accompanied the war party, taking care to keep their distance from the battle, so that they were on hand for victory celebrations and yet were ready to flee should it be necessary.

In order to intimidate the enemy, each side tried to make the other hear its preparations. When battle commenced, the warriors formed up into two lines, and advanced towards each other singing military songs "mixed with strange screams". When the lines came close together, they started shooting arrows. To further intimidate the enemy, before his eyes, atrocities were committed on the first victims (Navarrete, 1802, pp. 169-170). Laperouse (1797, II:272) states that although prisoners and enemy dead were not ordinarily eaten, if a chief or a very brave man was killed, a small part of his body might be consumed in homage to his valor and to increase that of the eater. He further states (pp. 272-273) that scalps were taken, and that eyes were removed and preserved as tokens of victory; the last sounds dubious, from both a practical and an ethnological point of view.

Two signals of a desire for peace have been reported. Fages (ms, p. 53)

gives the obvious one of unstringing the bow. The two Portola expeditions came into contact with the other: feathered sticks and arrows set upright in the ground. Crespi (Palou, 1857, VI:441) states that when the 1770 expedition arrived, he, the lieutenant of the Voluntarios de Cataluna, and a soldado de cuera went to examine the cross that had been set up in 1769 on Monterey Bay. They found it surrounded by arrows and feathered sticks, a row of sardines, a piece of meat, and at the foot of the cross a little heap of clams. No such offerings were found at the cross at the mouth of the Carmel River (op. cit., p. 443). Palou (1787, p. 106) states that when the neophytes began to speak Spanish, they told the padres that the Indians had been very frightened of the great sign left by the Spaniards, that at night it shone and appeared to grow until it reached the skies. That it should not harm them, they offered it food. On seeing that the food was not eaten, they offered it their arrows and feathered sticks, to show that they wanted peace with the cross and the men who had placed it there.

The 1769 expedition had already experienced this sign of peace somewhat to the north of Rumsen territory. Crespi (Palou, 1857, VI:356) says that, on the way north from Monterey, the advance explorers had come across a rancheria of some fifty souls. The people did not know the Spanish were coming, and were thrown into confusion. Some ran for weapons, some screamed, the women burst into tears. The soldiers did their best to reassure them. The sergeant dismounted, and approached with signs of peace. The Indians would not let him go to the rancheria; they made signs that he should stop. The Indians all seized their arrows and threw them into the ground, together with other darts (banderillas--darts used in bullfighting) and plumes which they had just brought from their houses. They then retired. The sergeant deduced that this was a sign of peace, and went up to and seized some of the arrows and other things. This pleased the Indians greatly; they applauded, and with signs invited the Spanish to eat.

This incident, although it did not take place in Rumsen territory, occurred not far away and within Costanoan territory. It seems highly probable, therefore, that the known Rumsen action with regard to the cross had a similar basis. It seems, however, to have religious overtones. In both cases the reaction was to one of extreme fear; the one, probably, to men on horseback, and the other to the supernatural behaviour of a wooden cross. It is clear from both accounts that some other kind of feathered stick, besides arrows, was used. As Kroeber has noted (Heizer and Whipple, 1951, pp. 55-56), feathered wands as offerings have also been reported from the Chumash and Maidu, and one is irresistibly reminded of Southwestern prayer-sticks.

Relgion

The sun appears to have been one of the principal deities of the Rumsen. It was believed that the sun had once been a man, and that he had had the power to take away their lives (Navarrete, 1802, p. 170). Offerings of tobacco smoke were blown towards the sun, the moon, and certain beings who were believed to dwell in the sky, all of which affected their needs. The offerer said, "There

goes this wisp of smoke that you may give me a good day tomorrow." A handful of pinole, flour, or seeds would also be thrown towards the sun, moon, or sky, with the words "I send you this so that another year you may give me more abundance (Amoros, 1950, pp. 470 and 479).

Certain old women were believed to have control over the fertility of the plants. They received gifts for this. When the year was a poor one, it was because the old woman was angry, and she had to be placated with further gifts. In good years, she was contented and approving, and everyone humored her (Amoros, 1950, p. 470).

It was believed that when someone died, he went to the place where the sun set. There, there was a man who received the dead. The dead sometimes returned to visit their relatives in dreams, which frightened them very much (Amoros, 1950, pp. 475-485). The Esselen believed that the dead turned into owls, which bird they held in great veneration (Navarrete, 1802, p. 170).

Medicine

Among the Rumsen, medical practice was in the hands of shamans. These were old people; it is not clear whether they were men or women, or could be either. To relieve pain, the shaman sucked on the aching part, and produced from his mouth a stone, which he said had been inside the sufferer, and was the cause of the illness. For this, the shaman was paid. Other old people sang and danced before the patient, and also received pay for it (Amoros, 1950, pp. 469-470, and 479). Merriam (ms, p.32) states that all doctors had the power of witches. He also (p. 31) gives a native word for the medicine man's dance. Putting these two sets of statements together, it appears likely that there were two kinds of shaman among the Rumsen: a sucking shaman, and the other a dancing one. The statement of Amoros implies that the two functions were vested in different persons. Both had supernatural powers.

In this connection, it is interesting to note that Rollin (Laperouse, 1797, IV:50) while at the mission saw a woman afflicted with epilepsy. The attacks usually lasted two hours. The normal duration of an epileptic fit is from five to twenty minutes; a two-hour seizure would be highly exceptional. On the other hand, two hours would be a reasonable length of time for a shamanistic performance.

Besides such performances, several other, more "practical" medical techniques were in use. Sweating (q. v. p.76) was a favorite remedy. Techniques for the reduction of dislocations were known, but were somewhat inefficient. Fractures were set, held in position with a bandage, and immobilized in a casing of bark (? This may be a basket, since it occurs in the same work in which the baby basket was referred to as a piece of bark) held in place by skin cords; the patient rested until consolidation was complete. Wounds and

ulcers, in simple cases, were left to heal by themselves. In severe cases, certain herbs were applied, either whole or crushed. If the pus produced by ulcers caused pain or wasting of the affected part, it was bathed with a lotion made from emollient plants or seeds. Hemorrhage was stopped with the aid of tampons made from animal hair, to which gentle compression was applied with the aid of pieces of skin held in place by cords, like bandages. The wad of hair was permitted to remain until it fell off owing to suppuration, after which the rest of the cure was left to nature.

Swellings containing fluid were left alone, unless they became inflammatory in nature. In this case, emollients were applied, either topically or in fomentations. The Rumsen knew nothing of methods for reducing hernias (Laperouse, 1797, IV:57-59).

The pain in an aching limb was sometimes relieved by binding it fast. Blood-letting was also a frequent practice: the aching part was punctured with a jagged flint, and the wound scraped, so that blood flowed freely. A root like a parsnip was used as a remedy for bloody (probably bacillary) dysentery; it was beaten to a powder, and administered as a drink with a little water. Sea water or amole sap were used as emetics; the latter was also taken as a purgative. No use was made of hot springs (Amoros, 1950, pp. 471-472 and 481-482).

Throat and chest ailments were treated with drinks of infusions of herbs; the herbs were afterwards crushed and applied to the epiglottis or to the site of the pain. Fevers were treated by making the patient vomit by forcing the finger back in the throat, and by sweating (Laperouse 1797, IV:49-50). Tobacco and abalone shells, mixed together and powdered, were given as an emetic (Merriam, ms, p. 28).

Sweating

The sweatbath appears to have been an important institution to the Rumsen, as it was throughout California. The structure of the sweathouse has already been described. It was used daily by the men; the women did not use it, although they also sweated after childbirth, but not in the sweathouse. According to Fages (ms, p. 54), the men sweated three times daily, in the morning, at midday, and at night.

The sweathouse held six or eight persons; the fire was near the door. Inside the sweathouse, the men scraped themselves or each other with shells or with bone spatulas, which they carried at all times, and used to scrape off sweat when marching and also as weapons in personal quarrels. To while away the time, various games were played. After about an hour of sweating, the men emerged, and plunged into the river (Fages, ms, pp. 53 and 54; Navarrete, 1802, p. 166; Amoros, pp. 472 and 481-482; Petit-Thouars, 1841, II:120-121; Merriam, ms, p. 16).

According to Rollin (Laperouse, 1797, IV:52-53, 57), sweating was a

favorite remedy, and, together with a decoction of sudorific plants, was regarded as a specific for syphilis. However, the form in which he describes it is not the normal sweathouse. A hole was dug in the sand about a foot deep and two wide, as long as the patient was tall. A fire was built throughout the hole and on the displaced sand. When it was all heated the fire was put out and the sand replaced superficially, to make the heat even. The patient removed his clothes and lay down in the hole. He was covered to the chin with heated sand. When the sand cooled and sweating diminished, he went and bathed in the sea or a nearby stream. This process was repeated until a cure was effected. It seems likely that this form of sweatbath was strictly medicinal, whereas the sweathouse had social functions as well, and was used in time of health.

It may be worth while to quote, in translation, Rollin's description of the plant which was drunk as a decoction in association with the sweatbath with a view to curing syphilis (Laperouse, 1797, IV:54 (25):

> Calix: four parts, ovoid, of the same size as the corolla, inserted in the fruit; it falls with the flower;
> Poly-petallic corolla: Petals four, small, entire oval, inserted in the receptacle;
> Stamens: Eight, inserted in the receptacle, of the same size as the corolla; fillets pulpy, furrowed or concave on one side, and convex on the other, wings rough, anther simple;
> Pistil: Fruit obrond (meaning not located), hairy pentangular, divided into five segments, enclosing an oblong seed; the hairs of the pericarp are very apparent, although very fine;
> Habit: I should judge that it must be a shrubby tree, at the most, of medium height; the stalks are angular, bushy, knotty, and covered with a sticky varnish; the insertion of the lateral branches alternates, and they are quite close to each other; the leaves smell, petiolate, bilobed, opposed, smooth underneath; flowers axillary, sometimes terminal, pedunculate, solitary, and sometimes geminate.

This plant was known t the Spanish as gouvernants.

Amusements

A number of Rumsen games have been reported. The hoop and dart game is described by Laperouse (1797, II:273); the native name for it is given by Rinart (Heizer, 1952, p. 13), and is recognizable as the same as that given by Laperouse (Pinart, tikirši; Laperouse, takarsia). The field was a space ten fathoms square, cleared of grass and surrounded with brush. The ring

was three inches in diameter; the darts were five feet long, and the thickness of an ordinary cane. The game was played by two men. Each tried to throw his dart through the hoop as it rolled, which gained two points; or to have the ring, when it stopped, fall on the dart, which gained one point. The game consisted of three points. It was a fast game, since the ring or the darts were always in motion.

A stick guessing game is described by Laperouse (1797, II:273-274); Fages (ms, p. 54); Lyman (1924, pp. 222-223); and Merriam (ms, p. 34). The object of this game was to guess which of the opponent's hands the object was in. According to Laperouse and Fages, the object was one stick; Merriam, a single marked shell or bone; Lyman, a particular one of two sticks. In order to distract the attention of the guesser, the player made many gestures, keeping his hands and body in motion the whole time; he sang, as did the onlookers, according to Fages and Lyman. Lyman states that the hands were held under a blanket. Laperouse states that four persons played it at once, apparently in teams of two--one hiding the stick, the other making distracting gestures--and that a profound silence was observed. If the guesser guessed right, he won a point and the right to hide the stick; if not, he lost a point. Five points made the game. Bets were laid on the outcome of the game; a skin, a handful of seed, a quiver, a glass of wine; Laperouse states that among the gentiles, the favors of the women were the stakes. Later in mission times, clothing was staked (Amoros, 1950, pp. 475 and 485; Lyman, 1924, p. 223). In the American period, the Indians were passionately fond of gambling, and would continue from Saturday evening to Monday morning (Lyman, 1924, p. 223).

Lacrosse was also played by the Rumsen; it was called xils (Heizer, 1952, p. 13) or ils (Merriam, ms, p. 33). Since Pinart's x probably represents an unvoiced glottal spirant, (or possibly a stop) which is very easy to miss, these two renderings undoubtedly represent the same word. Merriam (ms, p.33) states that the stick was curved like a golf stick; at the beginning of the game the ball was buried in the ground, and the first player struck it out with his curved stick.

Another game, called trălk or tsălk (Merriam, ms, p.34) was played with twenty sticks. These were split branches, flat on one side and curved on the other. They were thrown down, and if ten came flat side up, a count was made.

Pinart (Heizer, 1952, p. 13) mentions an unidentified ball game. Since the native word given by Pinart looks like no other name of a game, it is probably a different game from any described here.

Music

The musical instruments of the Rumsen consisted of the alder flute, the split-stick rattle (made of the wood of the California laurel, Umbellaria), the

bone whistle, and a rattle made of two cocoons (Amoros, 1950, pp. 474 and 484; Merriam, ms, p. 31). The hollow-log drum was unknown (Merriam, ms, p. 31).

Amoros (1950, pp. 474 and 484-485) states that the split-stick rattle was used to beat time for chants, which had the same rhythm whether joyous or sad. A chant of joy or vengeance would be sung to the same rhythm; the words consisted simply of naming the cause of their joy, or shouting disparaging words against the enemy. These chants were very simply: "Acorn a^{a}.... a^{a}....Acorn," or "Maimed one o^{o}....o^{o}.... Maimed one," or the like.

Kroeber (1910, pp. 258-259) gives eight Rumsen songs. Of these, four are dance songs; three of these appear to have mythological reference, and the other refers to a woman's white face paint. The longest of these runs as follows:

ka istun xaluyaxe	I dream jump
ka mas ictunine	I you dream-of
werenakai	rabbit
tceicakai	jackrabbit
eksenakai	quail

"I dream that you jump, I dream this of you, Rabbit, Jackrabbit, Quail." According to Kroeber, this was sung by Rat to the three animals mentioned, who danced.

Of the four other songs, two are charms--one to bring a husband home, the other a hunting charm; one is the quaint love-song:

hayeno	come!
ha-me ka rut·ano	you I mean
ha-purps tcokolate	hat chocolate-colored

"Come! I mean you with the chocolate-colored hat." The other song is stated to have been played by a blind man on his flute. A girl was attracted, came to him, and became his wife. The song is a very simple one:

piina watena tot·i	there goes meat

The story sounds like a folktale, but could well be true.

Merriam (ms, p. 31) gives Rumsen words for several dances. These are: medicine man's dance; devil's dance ("used bone whistle in this dance"); bear dance; coyote dance; dove dance; and puberty dance. They are not described.

One further note remains to be added with respect to music. On board ship in the vicinity of 55° North latitude, Crespi makes the following remark concerning some Indians approaching the ship in canoes (Palou, 1857, VI:645) (26);

> "From a good distance from the ship we heard them sing, and from the tune we knew they were gentiles, since those of San Diego and Monterey use the same song."

This statement is probably not to be taken too literally; it is to be remembered that the harmonies of Indian music were unfamiliar to Father Crespi's ears, and hence songs that were actually very different might well sound alike to him.

Calendar

The year was measured from one acorn harvest to the next. It was counted off in moons; the Indians would say that it was four moons until the harvest. The years of their age were not counted, nor the years since the death of a relative; owing to the tabu on the name of the dead, Father Amoros found that the Indians disliked such questions. Particular periods were remembered as when there was war, or when the sea was angry, or when a ship came (Amoros, 1950, pp. 472 and 482).

Appendix 1.

Table 1. Mission Statistics

Date	Population of mission	Source
Dec. 26, 1770	first convert	Palou, 1787, p. 105.
1773	154	Palou, 1857, VI:452 Palou, ms, f. 156[v]
1775	151	Fages, ms, 51
1784	ca. 700	Palou, 1787, p. 278
1786	711	Laperouse, 1797, II:253
1792	ca. 700 ca. 800	Menzies, 1924, p. 284 Vancouver, 1798, II:34
1803	700	Humboldt, 1811, I:326-327
1827 (Jan.) (March)	260 306	Beechey, 1941, p. 71 Duhaut-Gilly, 1835, II:150
1834	500 /!/	Duflot de Mofras, 1844 I:520
1836	almost abandoned	Ruschenberger, 1838, p. 507
1837	abandoned	Petit-Thouars, 1841, II:80, 117
1840	a few families only; a family of half-breeds are caretakers	Farnham, 1850, pp. 99-100
1842	40 no converts, no priest, a man and wife are caretakers	Duflot de Mofras, 1844, Simpson, 1847, p. 203

Table 2. Physical measurements (after Rollin) (Laperouse, 1797, IV:60)

	Men			Women		
	Feet	Inches	Lines	Feet	Inches	Lines
Average height	5	2	6			
Great diameter of head		9	-		8	5
Small " " "		5	4		5	3
Length of upper extremities	2	1	9	2	1	-
" " lower "	2	9	-	2	6	-
" " feet		10	-		8	6
Width of chest	1	1	-		10	9
" " shoulders	1	7	-	1	2	8
Height of vertebral column	1	11	-	1	8	6
Circumference of pelvis	2	6	8	2	6	-
Distance from one superior anterior spine to the other					8	5

"These proportions were measured in the following manner: for the upper extremities, from the head of the humerus to the end of the middle finger; for the lower extremities, from the head of the femur to the heel, and from the heel to the great toe; the width of the chest, from one superior humeral articulation to the other; the height of the vertebral column, from the first cervical vertebra to the sacrum; the great diameter of the head, from the superior angle of the occipital to the symphisis of the jaw, and the small diameter, from one parietal boss to the other." Owing to the peculiar nature of Rollin's measurements, they are of more historical than scientific interest.

Table 3. Kinship terminology

Kroeber	Merriam	Pinart	meaning	Kroeber	Merriam	Pinart	meaning
Parent class:							
apa	Ah'-pahn	--appan				--insins	S, man spk.
	Ap'-pah	appa	F		--e'-soo-win	--isuin	S, wmn. spk.
						--can	D, man spk.
ana	An'nah,	--aan			Lă-choot	--isuine	D, wmn. spk.
	--ahn	--ana	M	iswin			Ch, wmn. spk.
Grandparent class:							
	Pahps,					mersens	Gs, man spk.
	pahp		Mf				
	Pahps,			meresens			Gch, wmn.spk.
	pahp		Ff				
pap		--pap	Gf				
	Men'-nā		Mm				
	An'-nah		Fm				
men		--men	Gm				
Sibling class:							
taka	Tah'ks,	--tacan	Ob		Tah'-kah*	--tausins	Yb
	tah'kah						
					Tow'-o-six,	--tausins	Yss
tana	tahns,	--taan	Oss		Ta-ow'-siks		
	tow'-six,						
	tă-ow'-six			tauusis			Ysb
Uncle class:							
	st'tä,			meresens		--mērs ens	Np
	--st'-tan		Fb				
	et'tā, --st'-ten		Mb			--mers, --	
	ahn'-nah-kans		Fss			--can	No, man spk.
	ahn'-nah-kans		Mss				
					Kah-nah'		No or cousin,
ete		--etten	U				wmn. spk.
anakens		--uji	A				
Spouse class:							
urin	--oor-rín	--urin	H	xawan	--how'-wan	--xauan	W

* Tow'-o-six, Tă-ow'-sike deleted in two places, and replaced by Tah'-kah, identified as brother.

Table 3--continued.

Kroeber	Merriam	Pinart	meaning	Kroeber	Merriam	Pinart	meaning
Parent-in-law class:							
	oo^{ch,}-in		M1	mers			S1
xowom			P1	tictan			D1
Sibling-in-law class:							
	mārs		Hb		Tish'-tan		Hss
	mārs		Wb		Tish'-tan		Wss
				tictan			S1
hauunake			B1				

Endnotes

(1) "...y se Sallaron cerca de una cierre muy alta y blanca y porlas faldas toda bermeza Poblados de muchas /74v/ arboledas llamase esta sierra de santa lucia al aqual bienen a Reconocer de ordinario los naos que bienen de las yslas filipinas..."

(2) "La terra muy Poblada sin numero de yndios de que binieron en diferentes Besses cantidad de ellos á nuestro Real. paresse ser gente manssa y apassible; dicen por senas abar en la tierra ademtro munchas Poblaciones. El sustento que estos yndios comen mas cotidiano de mas del pescado y marisco as bellota y otra fruta mas gruesse que castana, que es lo que podimos entender de ellos."

(3) "Ay yndios aVnque rreselos de tratar con nosotros. digo que los dichos yndios se Vinieron de paz al pareser es gemte Buena y nos trujeron marisco y hacian mucha dilig[a] por lleuarnos a su pueblo que senalauan estar tierra ademtro,..."

(4) "Esta todo este puerto cercado de Rancherias de yndios afables, y de buenos naturales y bien dispuestos y amigos de dar lo que tienen, alli nos trujeron Pieles de Osos y de leon y de benados usan arco y flocha y tienen su modo de gobierno tenian much gusto de quenos estubieramos de asiento en su tierra andan des nudos aeste puerto..."

(5) "...y es buen Puerto para socorro de las Naos de China por ser la tierra primera que reconocen, quando vienen a la Nueva Espana:..."

(6) "En 4 de Octubre/1783/ en la Iglesia de esta Mision de San Carlos de Monterey bautice solemnemente un hombre de mas de un siglo de edad natural de la rancheria de Sargenta ruc casado cuya mujer aun vivia su nacimiento fue por estos contornos y dice se acuerda cuando antiguamente aportaba por aqui la Nao de China que trataba con estos gentes dandoles abalorios por pieles de nutria y en una ocasion dejaron en la punta de Cipreses uns pipa o tonel para que se aprovesharse del fierro de los aros, llamabase en su gentilidad Pechipechi y era tenido de los suyos en mucha veneracion pusele por nombre Juan Francisco fue su padrino Manuel Buitron cabo de la escolta y para que conste lo firme. Fr. Matias Antonio de Santa Catarina"

(7) "No vimos por estos contornos gentil alguno;..."

(8) "Esta tarde vinieron al real unas diez ó doce gentiles que dijeron tenian su rancheria dentro de la canada del rio que desagua en el estero. Trajeron su regalo de buena recion de pinole y semillas que se repartió entre la gente y correspondió el senor comandante con unos abalorios."

(9) "En la Punta de Pinos ningun puerto se halla ni hemos visto en todo el camino tierra mas despoblada que la de estos contornos ni gente mas bronca como se vé en esta diario, ponderando lo contratio el viage del comandante Sebastian

Vizcaino; que Monterey está muy poblada de gentiles sumamente buenos, sunque esto es mas fácil de trastornarse que un puerto tan famoso como era en los siglos antecedentes Monterey;..."

(10) "Hemos de decir tambien, que tierra mas despobladas que las situadas por las alturas expresadas maiormente al salir de la Sierra de Santa Lucia, no las hemos visto en todo el viage, ni gente mas bronca, ni mas salbage, que sus naturales: Que es pues de lo populoso que tanto ponderan los antiguos, y de la suma docilidad de sus moradores?"

(11) "En el mismo dia que se tomó posesion del Puerto...se fundó la Mision con el propio nombre...Los Gentiles no se dexaron ver en aquellos dias, porque desde luego les causó espanto la multitud de tiros de artilleria, y fusileria que se disparon por la Tropa; pero á poco tiempo empezaron á acer-/104/carse..."

(12) "Los Naturales de Monterey viven en la Sierra: los más cercanos á la Playa distan da ella como legue, y media, bajan á vezes, y salen á pescar en Balzitas de Enea, pero no debe ser la pesca su principal mantenimiento, y sólo recurrirán á alla quando les ayudare poco la caza que abunda mucho en lo interior de la Sierra, sobre todo la del verrendo, y venado. Son estos Serranos muy numerosos, en extremo dociles, y mansos; nunca salian venir á visitar á los Espanoles sin llevarles buen regalo de caza, que comunmente se componía de dos o tres Venados, ó Verrendos, que /64/ ofrecían sin exigir, ni siquiera pedir cosa alguna: su buena índola ha dad á los Reverendos Padres Misioneros bien fundadas esperanzas de Conquistas brevement á la Fe de Christo."

(13) "...en las cercanías de la mision hay varias rancherias de gentiles que desde luego de fundada la mision le empezaron á frecuenter y empezó en breve su reduccion,..."

(14) "Monterey, et la Mission de S. Carlos que en dépend, comprennant la pays des Achastliens et des Ecclemachs."

(15) "Le pays des Ecclemachs s'étend a plus de vingt lieues a l'Est de Monterey; la langue de ses habitans differe absolument de toutes celles de leurs voisins;..."

(16) "Los Indios que ecuden a este Mision, ya sean de las tribus Runsienes ó Eslenes que son las principales, ó de las de los Ismuracances ó Aspaniaques.."

(17) "Por las noticias que had podido adquirir nuestros Misioneros acerca de las costumbres de las dos naciones Eslen y Runsien que ocupan toda la California septentrional, parace que aquella es la mas numerosa, y que los individuos de ambos viven errantes y despersos;..."

(18) "Tres tribus diferentes y perpetuamento enemigas entre sí, existen ahors unidas en esta misión...Denomínase estas tríbus, la de los Runsien, de los

Eslenes y de los Vaysh, á las cuales siguen hacia Santa Clara y San Francisco las de Ymuracan y Aspaniac...No lleveremos más adelante el hilo de nuestras comparaciones, y el que guste continuarlas, podrá leer en la relación del viaje al Estrecho de Fuca las noticias que se han insertado allí de estos naturales y son conformes á las adquiridas por nosotros en la campana de 1791..."

(19) "...y otra fruta silvestro del tamano de una Peru regular que cemen azada y cosida, aunque amarga un poco. El Arbol en que se dá es un tanto blanquisco y al mode de la Higuera, no mui alto: Quando lleva fruta queda todo depojado de sus ojas."

(20) "...cuinze a vingt Indins étaient assis sur leurs talons, ayant devant eux une pierre plate de deux pieds et demi de long et un et demi de large; ils tenaient a deux mains une autre pierre de forma prismatique avec lacquelle ils écrassient le grain."

(21) "...por su mayor valor y destraza pars la guerra se han grangeado la subordinacion y obediencia de los demas."

(22) Geiger's translation of the words of Amoros (1950, p. 481) in this connection proved too inaccurate to use. A completely new translation was therefore made.

(23) contratan: Geiger translates this as "borrow". The word has no such meaning.

(24) "...y las guerras en que vivian era por el espiritu de venganza que nunca se les olvida la injurris que les hayah hecho." Geirge (Amoros, 1950, p. 483) translates this as follows: "Lasting feuds were due to a spirit of vindictiveness which did not permit forgiveness on an injury done them." However, Father Amoros' reference is clearly to his earlier statement (p. 468): ".. en el estado de Gentilidan ordinariaments vivían en guerra" ("in the gentile state they /the villages/ were usually in a state of war"), and not to personal feuds. Moreover, olvidar means to forget, not to forgive.

(25) "Calice: Quatre párties ovoïdes, de même grandeur que la corolle, inserees sous le fruit; il tombe avec la fleur;

Corolle polypétale: Quatre pétales, petits, entiers, ovales, insérés sur le receptacle;

Étamines: Huit, insérés sur le receptacle, de même grandeur que la corolle; filets charnus, sillonés ou concave d'un côté, et convexes de l'autre, ailes velues, anthere simple;

Pistil: Germe obrond, velu, quinquangulairs, divisé en cinq loges, renfermant une semence oblongue; les poila du péricarpe sont très-apparens, quoique tres-fins;

Port: J'ai jugé que ce devait être un arbrisseau, au plus, de moyenne grandeur; les tiges sont anguleuses, touffues, noueuses, et enduites d'un vernis

gluant, l'insertion des branches latérales alterne, et elles sont assez près les unes des autres; les feuilles petites, pétiolées, bilobées, opposées, lisses en-dessous; fleurs axillaires, quelquefois terminales, péduneulées, solitaires, et quelquefois géminées."

(26) "De bien apartados del barco los oimos cantar en en /sic/ el tono conocimos ser gentiles, pues usan el mismo canto que los de San Diego y Monterey."

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V. NOTES ON LARGE OBSIDIAN BLADE CORES AND CORE-BLADE TECHNOLOGY IN MESOAMERICA

Thomas Roy Hester

In this paper four very large obsidian polyhedral blade cores from archaeological sites in Mesoamerica are described. The small depleted (exhausted) obsidian blade cores abundant at most Mesoamerican sites have been discussed at length in the regional literature (Kidder, Jennings and Shook 1946; Coe 1959; Epstein 1964; MacNeish, Nelken-Terner and Johnson 1967; Hester, Jack and Heizer 1971; Hester, Heizer and Jack 1971). These worn-out specimens represent the terminal phase of a blade production process which began with the removal of blades from much larger, roughly-shaped cores. These large cores (termed "macrocores" by P. Tolstoy, n.d.) have received little attention, although they, too, are a potential source of information on core-blade technologies in Mesoamerica. It is these cores which represent the initial activities in blade manufacture, activities about which little of substance has been published.

The macrocores are discussed in the following pages; one is from southeastern Mexico, and the others are from sites in Guatemala.

The Papalhuapa Specimen

This large core (Fig. 1,A) collected from the obsidian workshops near the site of Papalhuapa, Guatemala, has been previously described by Graham and Heizer (1968:104). However, a more detailed account of the core attributes seems warranted. Dimensions of this specimen are given in Table 2.

The Papalhuapa core is pyramidal in shape. The striking platform is a broad flat surface, created by the splitting of a larger obsidian nodule (concentric force rings are evident on the platform surface). Prior to the detachment of blades, the platform was prepared by the removal of several short arc-shaped flakes on the periphery of the platform (see Fig. 1,A). There is also a scratched area on the platform and some crushing along the platform edges. The sides of the core immediately below the platform are marked by a series of small flake scars. These scars apparently result from the trimming of the platform edge before a blade was removed. In such trimming activities, the overhanging platform edge (caused by negative bulbs of percussion left by prior blade removals) was removed and the edge straightened.

Near the edge on one side of the platform there is a shattered area with an intact cone of percussion. Pond (1930:49) states that such a cone could have been formed by a direct blow on a flat obsidian surface. A similar shattered spot is found nearby. In both instances, these shattered areas appear to result from attempts at blade removal in which the hammerstone or other percussion tool delivered a blow 10 to 15 cm. inside the platform edge. One side of the core bears considerable battering, perhaps indicative of secondary use as an anvil.

There are scars indicating the removal of eight large blades (see Table 1) and a number of smaller flakes and blades, including two adjacent hinge fractures. Six triangular flake scars are present (7.6 to 10.4 cm. in length). Large blades such as those derived from this core were used at Papalhuapa for the manufacture of large bifaces and biface preforms (see Graham and Heizer 1968:108).

The distal end of the core is wedge-shaped. Several small flakes emanate from this end and there is slight crushing. Two major flake scars are present, one representing the removal of a blade 8.7 x 2.0 cm., and the other, an irregular flake, 7.6 x 4.0 cm. These may have been detached during the initial core-shaping procedures.

Blade scar length(cm.)	Blade scar width (cm.)
19.6	5.6
17.8	3.3
17.4	4.0
14.8	4.3
14.3	5.0
13.2	6.3
15.4	3.4
16.9	3.9

Table 1. Dimensions of major blade facets on the macrocore from Papalhuapa, Guatemala.

The Villahermosa Specimen

Another very similar macrocore is on display in the "Sala Azteca" of the Museo del Estado, Villahermosa, Tabasco (Fig. 1,B). According to museum records, the core was found in the "central Mexican plateau". Dimensions of the core are given in Table 2. It, too, has a pyramidal shape. The striking platform is a flat flake surface similar to that of the Papalhuapa specimen and formed by the splitting of an obsidian nodule. However, it is more extensively facetted. Three large flakes and two smaller flakes have been removed from the platform periphery and portions of the periphery are heavily battered. One

side of the core, near the distal end, retains a small area (10.0 x 6.5 cm.) of nodular cortex which survived the shaping of the core. There are eight large blade scars on the sides, six of which extend from the striking platform to the distal end; maximum widths of the scars vary from 2.5 to 7.5 cm. Ten other flake scars are present, mostly triangular in shape. There is one hinge fracture 13.0 cm. in length, another 4.0 cm. long, and a series of three overlapping hinge scars, 4.0 cm. in length. The distal end of the core is lightly battered.

The Quirigua Specimen

A third macrocore (Fig. 1,C) has been published by Stromsvik (1941:81; Fig. 32,d). This specimen was found in a lidded pottery box, packed in fine bluish-clay and placed beneath Zoomorph G at the site of Quirigua, Guatemala. The Quirigua core is not described in Stromsvik's text. However, the core has been recently located in the National Museum of Guatemala. The following descriptive data are based on photographs and observations by Marion P. Hatch and E. M. Shook; dimensions of this macrocore are given in Table 2.

The Quirigua macrocore has a shape quite similar to the previously-reported specimens. It has a flat flake striking platform. Trimming flake scars are evident around the platform edge. Ten large blades have been detached from the core, most extending from the platform to the distal end. Several smaller blades and flakes were also removed. The distal end is wedge-shaped and shows crushing.

A small sample of the Quirigua macrocore was provided by the National Museum of Guatemala, and was subjected to rapid-scan X-ray fluorescence analysis by Robert N. Jack of the University of California, Berkeley. Jack's analyses indicate that the macrocore is made of obsidian from the Ixtepeque source near Jutiapa (approximately 80 miles southwest of Quirigua). A geologic description of the obsidian flows in this region has been published by Williams, McBirney and Dengo (1964).

The Tiquisate Specimen

A fourth macrocore (Fig. 1,D; Table 2) has been found at the site of Tiquisate on the south coast of Guatemala. It is now stored in the National Museum of Guatemala. I am indebted to Marion P. Hatch and E. M. Shook for the following descriptive comments and for the photograph of the specimen.

In shape, the Tiquisate macrocore resembles the other specimens, although it is considerably larger. The striking platform is flat and smooth (i.e. a single facet), with trimming flakes removed along the periphery. There are eleven blade scars on the core, most of which extend from the platform edge to the distal end. A remnant of nodular cortex is preserved on one side of the specimen.

As in the case of the Quirigua macrocore, the personnel of the National Museum of Guatemala allowed the removal of a small piece of the Tiquisate specimen. X-ray fluorescence analysis revealed the presence of trace elements distinctly characteristic of the El Chayal obsidian source 70 miles to the northeast (for a description of the El Chayal obsidian quarry, see Coe and Flannery 1964).

Discussion and Interpretations

With this brief review of four Mesoamerican macrocores, it is apparent that these pieces in their initial form share a number of characteristics: (1) they are pyramidal in shape; later in the blade removal process, their shape often changes to cylindrical, conical or bullet-shaped; (2) striking platforms are flat flake surfaces, modified only along the peripheries; the extensive platform alterations (such as truncation, facetting, grinding and scratching) noted by Hester, Jack and Heizer (1971) were apparently performed when the cores were greatly reduced in size (there is an area of scratching on the platform of the Papalhuapa specimen); (3) a number of large blades (eight to ten) had been detached; such pieces could have served as blanks for biface manufacture, as they did at Papalhuapa; (4) the sides of the cores, just below the platform edge, were trimmed in order to remove overhang created by negative bulbs; these trimming activities were necessary throughout the life of the core; (5) the distal end of each shows crushing or battering.

These macrocores were probably always manufactured at obsidian quarry-workshops, such as Papalhuapa, El Chayal, and Cerro de Navajas (Holmes 1900, 1919 ; Breton 1902; Coe and Flannery 1964; Charlton 1969; Graham and Heizer 1968). To initiate the process, a large obsidian nodule was split, or "quartered" (see Pond 1930; Leakey 1960, 1965; Howell 1970), and the resulting flat flake fracture plane was used as a striking platform. Holmes (1900:413) states that such roughed-out macrocores were "...roughly cylindrical...and averaged four or five inches in length and two to four in diamter". It seems likely, based on the data presented here, that newly-formed macrocores were more often pyramidal in shape and considerably larger than the size range indicated by Holmes. If, during the shaping process, the nodule was ruined or found to be flawed, it was rejected. (Holmes 1900: Fig 45; 1919, Fig. 97).

Once the macrocores had been shaped, they were ready to be used in the production of large blades. Holmes (1900:413; 1919:22) did not think that the blades were detached at the quarry sites. However, R. F. Heizer informs me that at Papalhuapa there are numbers of macrocores present, as well as an abundance of large blades and bifaces made on blades. It was Holmes' belief that the macrocores were carried to occupation sites and the blades made when needed. This would almost certainly be true in instances, such as at Papalhuapa, where occupation sites were very near the obsidian source; the data provided by the Tiquisate and Quirigua cores indicate that long-distance transport of macrocores was also practiced. In the case of the Quirigua specimen, its transport

from the quarry at Ixtepeque might have had some ceremonial significance, since Stromsvik (1949) reported that it had been carefully cached. The ritual use of cores in Mesoamerica is widespread (Kidder 1947:20).

Michels (1971:266) has offered the hypothesis that quarried nodules of obsidian "...were brought to market and sold by the miners themselves. Other craftsman [sic], skilled in the preparation of cylinder cores and in the pressure-flaking technique of blade production, would very likely purchase a portion of these for further processing and resale". Michels' hypothesis is strictly applied to the green obsidian of the Valley of Mexico, the source of which was not easily accessible. On the other hand, the gray obsidian of that region was less difficult to obtain, and Michels (p. 267) postulates "...separate collecting efforts of many individuals aimed at satisfying their own needs". It seems reasonable to assume that no single model of macrocore distribution (and the subsequent manufacture of blades from them) is applicable to all parts of Mesoamerica.

I would like to offer some brief comments on the techniques used in detaching blades from macrocores. All four of the specimens described here have large, deep negative bulbs of percussion and battering along the edges of the striking platforms. The presence of such attributes suggests that direct percussion (perhaps with a hard hammer; J. Desmond Clark, personal communication) was used. Certainly the intact cone of percussion on the Papalhuapa specimen would reflect such a technique (Pond 1930:49). Honea (1965:32) believes that cores worked by direct percussion methods were often rested on an anvil, and it is possible that the use of an anvil could have produced the crushing noted on the distal ends of some macrocores.

Crabtree (1968:457-458) has experimented with hammerstone percussion and was unable to produce regular, parallel-sided blades. He has examined the Papalhuapa specimen and believes that it may have been worked by indirect percussion with a punch (D. E. Crabtree, personal communication; for a discussion of the punch technique, see Ellis 1940:32; Crabtree 1968; Bordes 1969; Bordes and Crabtree 1969). However, Crabtree has acknowledged that the Papalhuapa macrocore could have been made by direct percussion, "...with much skill and a soft hammerstone" (personal communication).

It is entirely possible that some macrocores may have been worked by impulsive pressure with a T-shaped crutch (Ellis 1940:48-49; Crabtree 1968). Indeed, some Spanish accounts of impulsive pressure techniques, as in the Aztec blade-making process, suggest that very large cores were used (see a translation of Torquemada's 16th century account in Hester, Jack and Heizer 1971; see also Fletcher, 1970). A recent translation by Feldman (1971:214) of an account by F. Hernandez suggests that impulsive pressure was used for blade-making on smaller cores ("medium-sized pieces" from which "small thick flakes" were removed; Feldman 1971:214). According to data presented by Honea (1965), the impulsive pressure technique (as well as indirect percussion)

would not have produced the massive negative bulbs which can be observed on the macrocores.

The striking platforms of these macrocores are horizontal, forming angles of 70^{o} - 90^{o} with the sides. Bordes and Crabtree (1969:5) have reported that a slanted, oblique striking platform is most efficient for cores being worked by indirect percussion. However, Crabtree has more recently stated (personal communication) that when the punch is held vertical to the platform, a conical core will result.

As the discussion above points out, there is no concrete evidence which indicates that one blade-removal technique was used to the exclusion of all others. Perhaps this question can be resolved through continued experimentation, such as that reported by Crabtree (1968).

There are probably many macrocores extant in museum collections in the United States and Mesoamerica. For example, Holmes (1900:413) notes: "...the largest nucleus that has come to the writer's attention is now preserved in the Field Columbian Museum and is about eight inches long and six inches in diameter". A recent communication from D. Collier indicates that a somewhat larger macrocore (found at Texcoco) than the one described by Holmes is on display in the museum (now the Field Museum of Natural History). Our knowledge of core-blade technologies in Mesoamerica would be measurably increased if data on additional macrocores were presented. More importantly, analyses are needed of the variety of workshop debris at obsidian quarry sites where these large cores are found. Such systematic, quantitative studies (none of which exist at present) would deal with the total workshop assemblage, thereby enabling us to define the processes involved in the manufacture of these cores and in their subsequent use.

Acknowledgments

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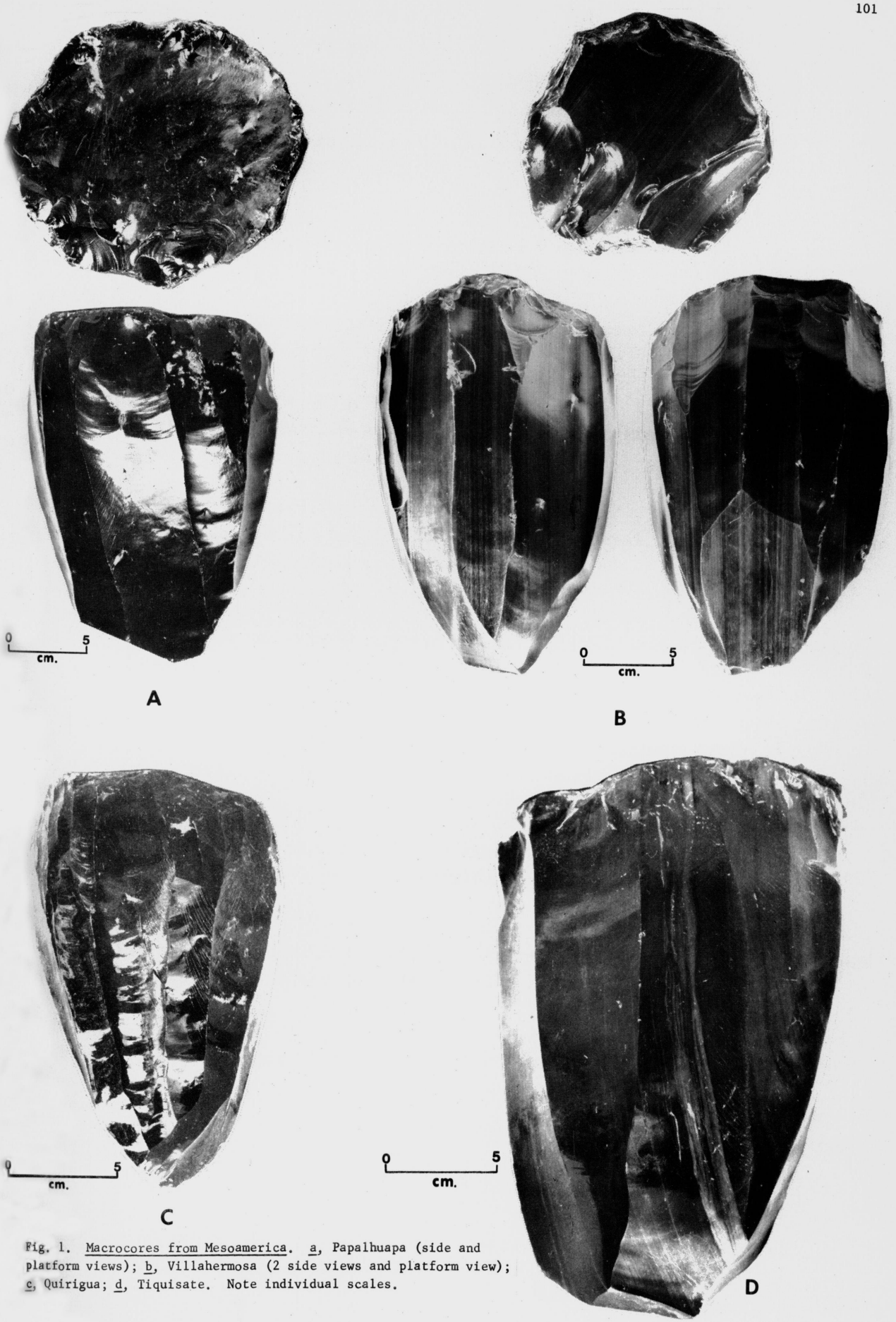

Fig. 1. Macrocores from Mesoamerica. a, Papalhuapa (side and platform views); b, Villahermosa (2 side views and platform view); c, Quirigua; d, Tiquisate. Note individual scales.

	Papalhuapa	Villahermosa	Quirigua	Tiquisate
Core Height (proximal-distal distance)	19.2	22.0	16.0	24.6
Maximum width of core	16.3	17.0	12.7	20.0
Maximum diameter of striking platform	16.0	15.0	12.2	20.0
Minimum diameter of striking platform	13.3	13.0	12.0	15.5
Weight (lbs.)	11.0	[13.0]est.	4.9	27.0

Table 2. Dimensions and Weights of Mesoamerican Macrocores. All linear measurements are in centimeters.

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VI. PROBLEMS IN THE FUNCTIONAL INTERPRETATION OF ARTIFACTS: SCRAPER PLANES FROM MITLA AND YAGUL, OAXACA*

Thomas Roy Hester and Robert F. Heizer

We have recently studied a series of unifacial stone artifacts from the Post-Classic sites of Mitla and Yagul in the Valley of Oaxaca, Mexico.[1] These specimens are of a distinctive form, termed "scraper planes" or "push planes", found in various parts of the New World, particularly in arid regions of Mexico and the western United States. They occur in abundance at both Mitla and Yagul and have been reported from other parts of the Valley of Oaxaca (Lorenzo and Messmacher 1966). At Mitla, the specimens often occur in the earth and rubble hearting of the stone faced walls. The wall fill was presumably obtained from nearby superficial midden deposits of approximately the same age as the ruins, and thus we believe that the stone artifacts described here probably date from the Post-Classic. Examples of the Mitla specimens were first described by Holmes (1897), who called them cores, although he noted (p. 286) that flakes derived from them had not been modified or used as tools. A more recent discussion of Mitla "scraper planes" was published by Williams and Heizer (1965:46, Pl.7). During a visit to these two sites in the winter of 1970, we were able to examine a number of the artifacts and decided to analyze a series of them in an attempt to learn more of their manufacture and use.

THE SAMPLE

Eighty-one specimens were studied; 54 are from Mitla and 27 are from Yagul (Figs. 2,3). The series from both sites are quite similar, although those from Yagul are more crudely made and show less evidence of use.

Most are plano-convex in cross section, ovate in outline, and unifacially chipped. The majority have been formed by the splitting of a cobble, with the resultant fracture surface used as a striking platform to remove flakes, by percussion, from the convex face. Six specimens (4 from Mitla,

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2 from Yagul) are made on thick flakes, the ventral surfaces of which served as platforms for the removal of flakes from the dorsal side. For a discussion of the manufacturing techniques of similar tools from Lake Mohave, see Barbieri (1937: 101-102).

The dorsal surface is usually dome-shaped and is referred to in this paper as the "dome" (see Fig. 1 for descriptive terminology). The ventral surface or base is termed the "planar surface"; use-wear is often found around the edge ("planar edge") of this surface. The planar surface is generally flat, consisting of a single large flake scar (i.e., a single facet). Four specimens (all from Yagul) have two or more facets on the planar surface. In seven instances, specimens have two opposing (or sometimes adjacent) planar surfaces. These are similar to the "multifacted scraper planes" reported by MacNeish et al. (1967) from the Tehuacan Valley of Mexico. Two specimens (both from Mitla) have wedge-shaped domes and markedly convex planar surfaces.

The flake scars on the dome are often large and extend to near the crest; cortex remnants are usually present. On many pieces, there is step flaking along the planar edge (see Fig. 1). Some of the tiny step flakes are attributable to use-wear. Such use-flaking has been referred to as "nibbling" by Hole, Flannery and Neely (1969) and Hester (1970). Larger step flakes (those exceeding 5-7 mm. in length) may result from either resharpening activities or the shaping and trimming of the planar edge. On four examples, flakes have been detached from the ventral or planar surface (using the planar edge as a striking platform), probably part of a resharpening technique (cf. Shafer 1970: Fig. 1,c). Burins on scraper planes such as observed on specimens from coastal California (Heizer and Kelley 1961) are absent from the Oaxaca examples studied by us.

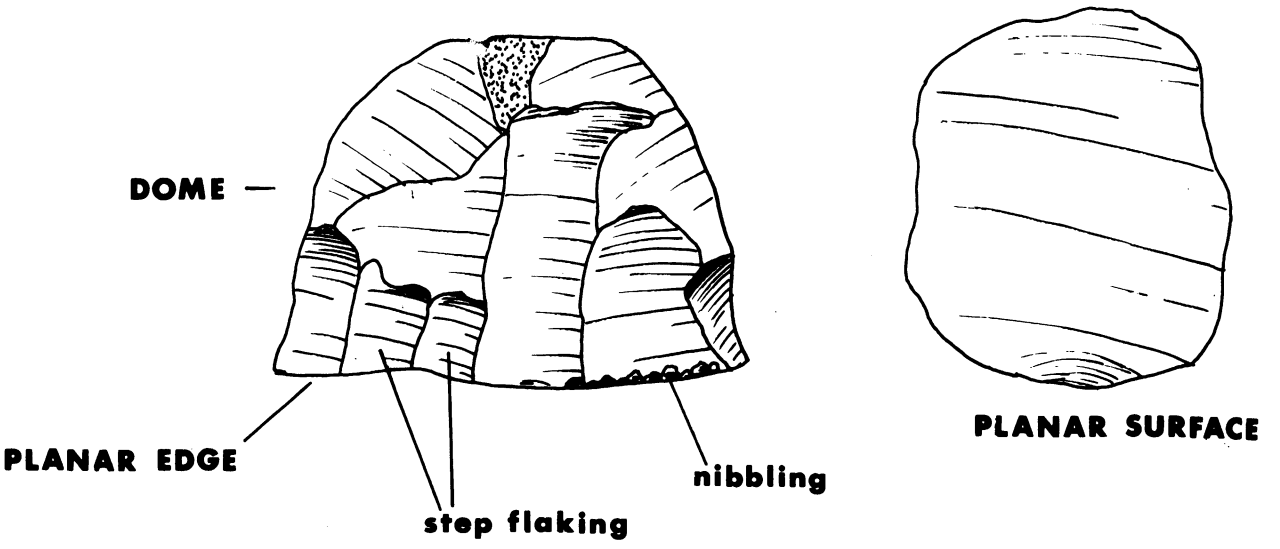


Fig. 1. Descriptive Terminology for Scraper Planes.

The raw materials used in the manufacture of these artifacts at Mitla include silicified tufaceous sediment (dominant), rhyolitic ignimbrite, and silicified sediments derived from ignimbrite. At Yagul, several specimens are made of gray-black andesite, several of banded rhyolitic lava, and others of silicified ignimbrite, impure limestone and rhyolitic ignimbrite.[2]

Dimensions and weights of the study series are given in Tables 1 and 2.

WEAR PATTERN ANALYSIS

The planar edges of all specimens were examined under high magnification (30X to 75X), using a binocular microscope with an independent light source. Dulling of various kinds was observed on almost every specimen. Much of the dulling is light and scattered, although in certain instances it extends along one-half of the circumference of the planar edge. In all cases, light dulling had to be detected microscopically. Twenty-nine specimens (14 from Mitla and 15 from Yagul) show heavy dulling, observable on the macroscopic level. Such dulling appears as a broad band of attrition (seen as a rounded or slightly beveled planar edge) usually restricted to one-half or less of the circumference. Under magnification, 13 specimens exhibit deep striations or grooving (perpendicular to the planar edge; see Fig. 3,b) in association with heavy dulling.

Other forms of use-wear include nibbling (4 specimens, all from Mitla) crushing or battering of the planar edge (Mitla, 3; Yagul, 4) and polishing of the edge (1 example, from Mitla). In one instance, a specimen from Mitla is abraded over much of the dome. The domes of four other specimens (all from Mitla) exhibit light to heavy battering.

EDGE ANGLE ANALYSIS

Semenov (1964) and Wilmsen (1968a) have suggested a direct correlation between the angle of the tool working edge and the function of the tool. In order to supplement our wear pattern data, we measured the edge angles of all specimens. Results are given in Tables 1 and 2. Initially, we used a polar coordinate graph to measure the angles (cf. Wilmsen 1968b), but we later found that these measurements could be more easily determined with equal accuracy with a goniometer.[3]

Edge angle values for the Mitla series (those specimens with a single planar surface) ranged from 50° to 125°. Sixty-seven percent of these values were between 60°-80°. Yagul edge angles show a similar wide range, from 55° to 100°, although 56% were between 65°-80°. In the samples from both sites, numerous specimens have edge angles in excess of 90°. These steep edges were created primarily by pronounced recession of the planar edge caused by nibbling

and by the removal of larger step flakes (i.e., resharpening and trimming flakes). Edge angle values for specimens with two planar surfaces are indicated in Tables 1 and 2.

DISCUSSION AND INTERPRETATION

In our attempt to ascertain the function of these artifacts, we had several types of data with which to work. First of all, Holmes (1897) had pointed out that flakes removed in the manufacture of these objects apparently had not been used in any manner; thus they do not seem to be cores or exhausted nuclei in the usual sense. In addition to the presence of use-wear, which suggests that they are tools, they appear to be made according to a predetermined form or mental template (cf. Rogers 1929:459). They are reminiscent of the "hoof core" planing tools of Great Britain described a century ago by Gillespie (1877), a form which has also been noted among aboriginal groups in Australia (Mitchell 1949: Fig. 16; Gould 1968:171).

There are numerous publications which have described similar artifacts in the New World and many of these papers have proposed functional identifications of these tools. The rubric "scraper plane" has been most often used, and has been applied to specimens in California (Rogers 1929, 1939, 1966; Amsden 1937; Barbieri 1937; Heizer and Lemert 1947; Treganza and Malamud 1950; Treganza and Bierman 1958; Heizer and Kelley 1961; Creutz and Moriarty 1963; Kowta 1969), Baja California (Arnold 1957), Arizona (Hayden 1954), Chiapas, Mexico (MacNeish and Peterson 1962) and the Tehuacan Valley of Mexico (MacNeish et al, 1967). Additional distributional data are given by MacNeish et al (1967:39).

A number of these publications provide opinions for their functional interpretations. For example, Arnold (1957: 253) believes that Baja California scraper planes were "...designed primarily for use...against some relatively soft materials (wood?) and that they were also employed in a variety of other ways". His specimens were "smoothed" along the planar edges.

In their discussion of scraper planes from the Topanga lithic assemblage, Treganza and Bierman (1958:56) note: "The nature of the wear indicated hard usage, such as would result from repeated contact on an unyielding surface". Amsden (1937:61) observes that the Lake Mohave scraper planes "...meet the requirements of a fleshing tool, in addition to having a fore edge sufficiently well sharpened and thinned to serve as a skinning knife". The scraper planes found by Rogers (1939) in the Playa complex were attributed to use as "fleshing planes in dressing hides" (p. 29). However, Rogers (1939:50) also presented ethnographic data which suggested the use of scraper planes for processing Agave leaves. This latter interpretation is accepted by Kowta (1969:52), although he suggests that these tools might have had other uses in the exploitation of Agave, such as the shaping of chisel-ended digging sticks (Ibid.,p.54).

While it is probable that scraper planes served a wide variety of functions in different environmental situations, our major interest is determining their use at the sites of Mitla and Yagul. Our sample shows a wide range of use-wear, such as light and heavy dulling, crushing, battering and nibbling, none of which by itself can be linked to a specific function. However, 36% of the specimens have heavy dulling (often as a band of attrition along the planar edge) and associated striations. Such a wear pattern, according to Witthoft (1955:20), can be caused when a stone tool comes into sustained contact with another stone. We postulated two tasks which might involve these conditions: (1), the tools were used to shape and/or trim the building blocks found in the structures at Yagul and Mitla; or (2), the tools were used to process some type of thin, pliable material which had been placed on a stone backing or anvil and that during the course of such work, the tool edge repeatedly rubbed against the resistant backing. We feel that if their function had involved the first task (i.e., shaping of building stones) much more evidence of rough use, such as massive crushing and battering of the edge and more extensive planar surface wear, would be observed on the tools. When considering the second possible task, we reviewed some of the functional interpretations found in the literature. We were particularly interested in the hypothesis that this tool form was employed in Agave exploitation, particularly the shredding and de-pulping of the leaves. Since the fibers of this plant (which occurs in abundance in the Oaxaca Valley) could be used in making cordage and rope, the proliferation of these tools at Mitla and Yagul might be related to the local manufacture of rope, perhaps to aid in the construction activities (such as dragging building blocks, tying poles used in scaffolding, etc.). This has been previously suggested by Williams and Heizer (1965:46).

Rogers (1939:50) recorded the use of scraper planes in the working of Agave leaves in southern California:

> "A Southern Diegueno informant has described them as being used in the preparation of fiber for cordage. The green leaves of various species of agave were placed on flat rocks or the backs of metates and the pulp pushed out with these heavy planes, which were manipulated exactly as a carpenter's plane".

MacNeish et al (1967:36) have reported two scraper planes recovered from dry caves in the Tehuacan Valley which had plant fibers adhering to the planar edges. Unfortunately, these fibers cannot be specifically identified (R. S. MacNeish, personal communication).

To test the proposition that these tools were used in the working of Agave, we conducted an experiment. Leaves of an Agave (an unidentified species from Mexico) were secured from the Botanical Garden of the University of California, Berkeley. Tools used in the experiment were taken from the Mitla series; those selected had earlier been examined microscopically and bore no evidence of use. In addition, two modern scraper planes of obsidian were made for the experiment.

The major part of the experiment centered on the use of one tool (Cat. No. 3-24143; see Table 1 and Fig. 4), which was used to work a single large Agave leaf (length of leaf, 3.5 feet; basal width, 7 inches). The leaf was placed flat on the macadam surface on the west side of Kroeber Hall. The operator held the implement at a slight angle to the leaf (see Fig. 4,b), although it sometimes rested flat against the leaf, and pushed the tool in a forward motion (i.e., away from the operator). Using this technique, it was found that the tool was extremely effective in peeling away the tough epidermis of the leaf, and thereby exposing the underlying fibers. At the same time the epidermis was being shredded (Fig. 4,a) the tool also pushed out much of the pulp contained in the leaf (note the dark stains caused by the pulp expulsion on the macadam surface; Fig. 4,c). We also discovered that some of the pulp could be expelled by pounding the exposed fibers with the dome of the tool (this, incidentally, caused light battering marks such as those found on four specimens collected at Mitla). As the shredding of the leaf proceeded, the exposed fibers began to spread, causing the planar edge to occasionally come into contact with the road surface. This was particularly true as the process neared completion and the long, white fibers became increasingly free of pulp and even more separated (see Fig. 4,c,d). The working of this large leaf consumed almost 45 minutes. No doubt with more practice less time would be needed. Afterwards, the tool was examined, and it was apparent that the edge had become heavily dulled. This was confirmed by microscopic examination. A slight beveling of the planar edge was also noted. During the leaf-working process, the dulling of the planar edge was felt as the tool became much less efficient, requiring frequent stops to clear the edge of accumulated fibrous material. We have no doubt that the dulling was caused by the occasional contact of the working edge of the tool against the backing (road surface). The wear observed as a result of the experiment closely duplicates the heavy dulling noted on the archaeological specimens. We attempted to use one of the freshly-made obsidian scraper planes to work a leaf. However, the planar edge was much too sharp, and it cut deeply into the leaf, with the result that the fibers were severed and the tool "jammed" in the leaf.

Thus, our experiment showed that tools such as those from Mitla and Yagul can be effectively used in a push-plane motion to decorticate and express the pulp from Agave leaves. The close correspondence of the use-wear on the tool used in the experiment and those with ancient wear patterns provides the best indications of the function of the tools at Mitla and Yagul. However, we should not rule out the possibility that they could have been put to other tasks, as the types of minor wear (nibbling, crushing, etc.) were not replicated in our experiment.

Wilmsen (,968a:157) has suggested a variety of functions for tools with edge angles of 60°-75°. As we discussed earlier, the majority of our samples from both sites have edge angles roughly in this range. The uses listed by Wilmsen are the working of wood, bone, and skin, and heavy shredding. We can state that our experimental tool (with an edge angle of 75° worked quite well

in the latter task. We did not experiment with working wood, bone or skin with these tools.

We should also point out that there are other methods and other implements, which can be used to process Agave (cf. Castetter, Grove and Bell 1938). An excellent example is provided by Michels (1971:265). He used an obsidian "scraper rasp" (a steeply beveled end scraper) to separate the pulp from the fiber of maguey leaves. Osborne (1965:48) has carried out a similar experiment, in which she used a claystone side scraper to process a yucca leaf; apparently, the scraper became dull and had to be resharpened during the work. Osborne also experimented with tools made from deer and mountain sheep humeri. An account by Lothrop (1929) records the use of a handled wooden scraper for the purposes of fiber extraction in a Guatemalan hennequen industry. Morris and Burgh (1954:61) used a notched deer rib to pulp yucca leaves, and cite an account of the Pima using a deer scapula to scrape the pulp from roasted maguey leaves.

It was earlier suggested (Williams and Heizer 1965:46) that the large numbers of scraper planes which occur generally in the zone of the Mitla ruins, and which constitute a conspicuous cultural element (along with potsherds) in the earth-fieldstone wall heartings of the Mitla buildings, might be taken as an indication that in prehistoric times there was an extensive local manufacture of maguey cordage and rope which served as aids in the construction activities at Mitla. This suggestion rested on no solid basis and was merely an inference. We have looked for evidence in Conquest and early colonial period documents for a substantial production of Agave cordage in the Oaxaca valley but have not found any. We cannot say whether this lack of early documentation signifies absence of such an industry or mere neglect to note a prosaic local activity in which the conquistadors and ecomenderos saw little of economic interest for themselves.

But the archaeological fact remains that scraper planes are present in very considerable numbers at Mitla, and we are inclined to believe on the basis of our technological examination of a number of these, together with our single successful experiment in extracting fibers from an Agave leaf, that the scraper planes were anciently used to extract cordage fibers. But in stating our hypothesis we are no further forward as regards actual evidence of what the scraper planes were used for in pre-Conquest times. We can offer a possible means of reaching a fairly firm conclusion to the question.

It is a known fact that many, perhaps most, plants have a particular affinity for certain trace elements which they extract from the soil and concentrate in their structure. We assume that Agave behaves in the same manner, but we do not know which trace elements are selectively favored in this plant. If in the past great quantities of Agave were processed, the pulp and liquid residues would presumably have become part of the occupation soil zone and would be associated with rejected scraper planes. Since the wall heartings

at Mitla are apparently midden soils, in this case we might expect to find in this earth a very high level of those particular trace elements for which Agave has an affinity. Such a demonstration still would not offer absolute and final proof that the Agave was manipulated with scraper planes, but it would strengthen the case for the association of Agave and this particular tool.

There is nothing very original in this approach except for its proposed application to illuminate the Mitla question. One of us (Heizer 1960: 99-100) proposed over ten years ago this kind of "chemical archaeology" as a possible way to show that maize may have been grown and eaten even though no palpable remains of the plant had survived. It has also been suggested that this approach might tell us the antiquity in Central California of dependence upon the acorn as a primary food resource. So far as we know, nothing has ever been done in the way of testing these propositions, but it could be done easily by applying the well developed methods used in geochemical prospecting (e.g. Chikisev 1965; Malyuga 1964; cf. Stiles 1951).

We intend to try the geochemical test for Agave identification at Mitla if and when the opportunity permits. In the meanwhile, we would encourage others to make this test if they wish, because we are equally as interested in exploring the potentials of the method as in any particular results which might be obtained.

Notes

1. Specimens studied for this paper are in the collections of the Lowie Museum of Anthropology, University of California, Berkeley.

2. We thank Dr. Howel Williams (Professor Emeritus, Department of Geology and Geophysics, University of California, Berkeley) for making the petrographic identification of the specimens.

3. Crosby (1967:102-103) reports some problems encountered in using a goniometer to measure tool edge angles, especially the accurate measurement of low angles. She suggests the use of a carpenter's template former.

Cat. #	Height	Max. Diam.	Min. Diam.	Weight	Edge Angle
3-24160	88	107	97	1014	100
3-24149	65	81	75	520	75
3-24176	86	82	62	639	75
3-24175	66	133	108	786	60
3-24141	94	83	83	851	105/85
3-24159	90	100	87	869	105
3-24171	72	98	78	752	100
3-24174	74	79	62	447	85
3-24162	65	75	72	470	80
3-22680	36	78	71	194	70
3-22868	46	61	48	181	60
3-24167	46	74	54	208	70
3-24173	42	82	58	261	85
3-24151	42	93	70	349	75
3-24165	72	76	66	424	90/75
3-24145	49	62	40	190	70
3-24147	50	78	60	259	80
3-24170	48	84	57	315	80
3-24149	64	79	75	520	60
3-24156	57	71	52	320	70
3-24146	43	66	57	195	80
3-24143	44	74	66	266	75
3-24142	41	78	67	244	70
3-24169	64	78	73	418	100
3-24144	51	90	61	353	90
3-24164	48	79	62	263	95
3-24152*	23	80	76	141	50
3-24161	26	68	57	98	60
3-24153	51	58	53	179	75
3-24158	59	80	47	266	125
3-24150	48	73	53	216	80
3-24166*	22	75	64	124	50
3-24163	54	72	53	235	60
3-24154	55	frag.	frag.	frag.	60
3-24168	58	75	45	169	60
3-24148	61	53	46	159	80/65
3-24172	44	82	57	242	80/75
3-24155	70	78	65	419	100/80
3-24157	53	71	58	283	65/100
3-22862	49	64	58	193	65
3-22860	53	78	64	220	70
3-22865	47	62	52	209	80
3-22863	62	81	69	323	75
3-22858	47	93	64	362	70
3-22861	50	63	60	260	70
3-22867	41	61	48	171	95
3-22681	25	63	51	109	70
3-22864	68	81	78	459	70
3-22866	49	63	50	182	90
3-22857	27	55	52	88	70
3-22859*	26	61	59	97	80
3-22854	30	76	59	139	60
3-22679*	36	75	70	154	70
3-22853	31	71	63	147	75
Mean	54.0	79.4	62.9	319.8	75.8

Table 1. Dimensions, Weights and Edge Angles of Scraper Planes from Mitla. Dimensions are in millimeters and weight in grams. Asterisk (*) denotes specimens made on flakes. The mean edge angle indicated here was calculated for 48 specimens (specimens with 2 planes excluded).

Cat. #	Height	Max. Diam.	Min. Diam.	Weight	Edge Angle
3-24118	45	67	58	203	70
3-24139	44	78	68	220	80
3-24117	30	66	61	170	65
3-24136	64	78	61	526	75
3-24131	38	78	60	196	60/65
3-24116	47	93	70	290	60
3-24124	54	66	51	261	85
3-24129	50	83	65	369	75
3-24135	53	106	96	522	65
3-24114	46	108	76	380	65
3-24138	70	84	77	601	75
3-24119	65	75	60	315	80
3-22862	52	77	74	317	80
3-22855	37	71	60	196	55
3-24127	84	86	74	655	95
3-24120	51	76	75	291	75
3-24123	35	81	71	260	70
3-24122	35	68	66	154	85
3-23137	72	122	100	895	90
3-24133	92	100	90	855	85
3-24115	75	97	88	803	90
3-24130	86	97	77	793	100
3-24128	84	94	81	783	95
3-24126	74	75	63	510	90
3-24125	77	90	90	671	70
3-24134	65	86	75	470	80
3-24132	23	80	76	230	75
Mean:	57.3	84.5	77.7	442.1	78.5

Table 2. Dimensions, Weights and Edge Angles of Scraper-Planes from Yagul. Dimensions are in millimeters and weights in grams. The mean edge angle indicated here has been calculated for 26 specimens (the specimen with 2 planes was excluded).

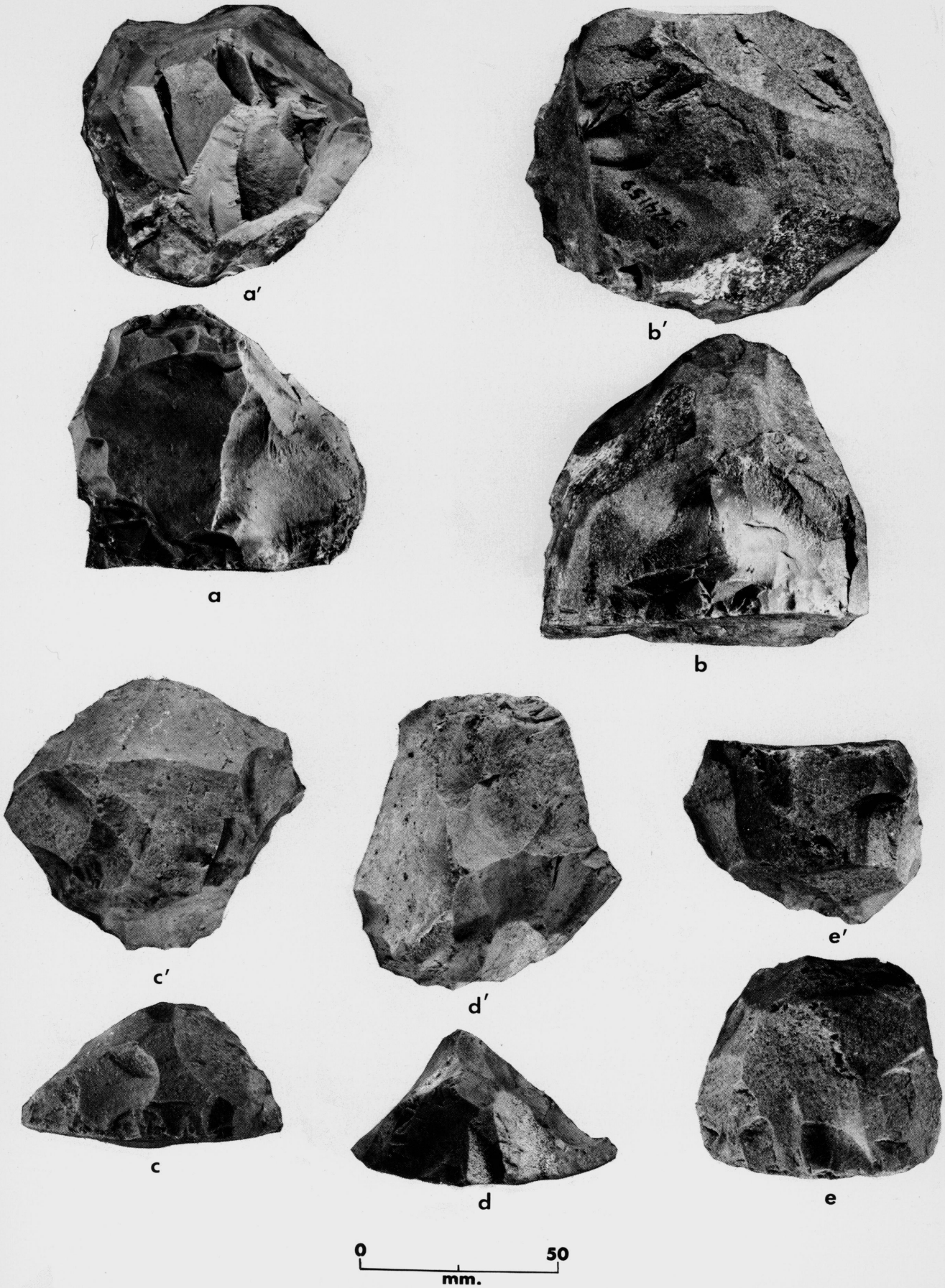


Fig. 2. Scraper Planes from the Valley of Oaxaca. a-e, frontal views, showing working edge; a'-e', corresponding dome views. a,a' (Cat. No. 3-22864); b,b' (3-24159); c,c' (3-22680); d,d' (3-22679); e,e' (3-24145).

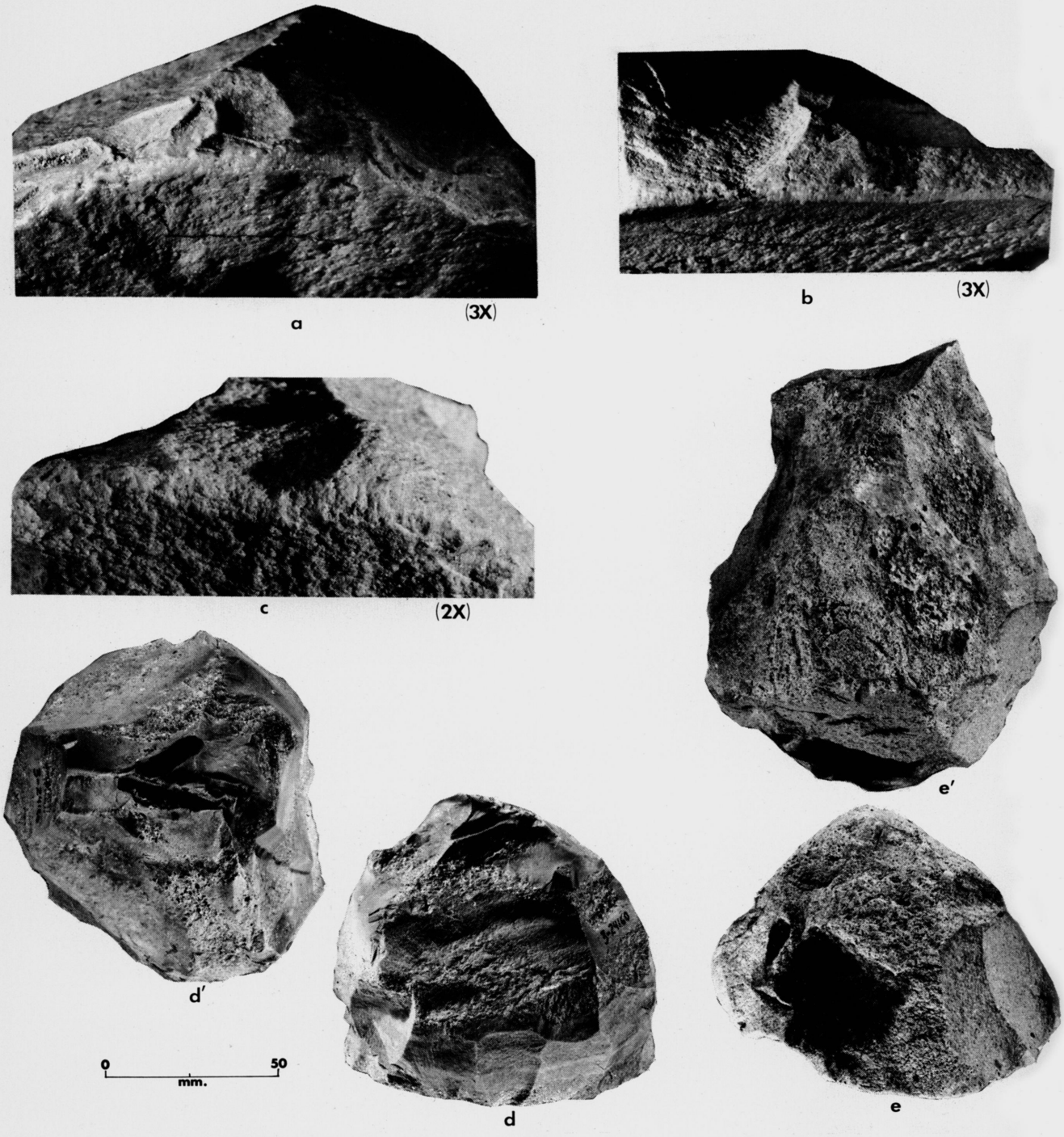
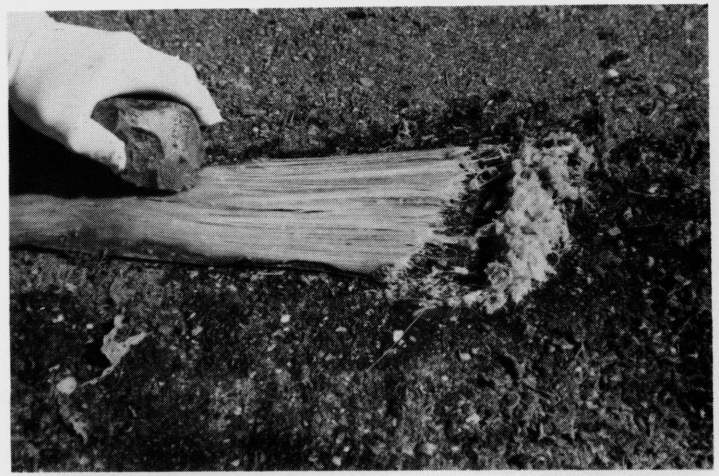
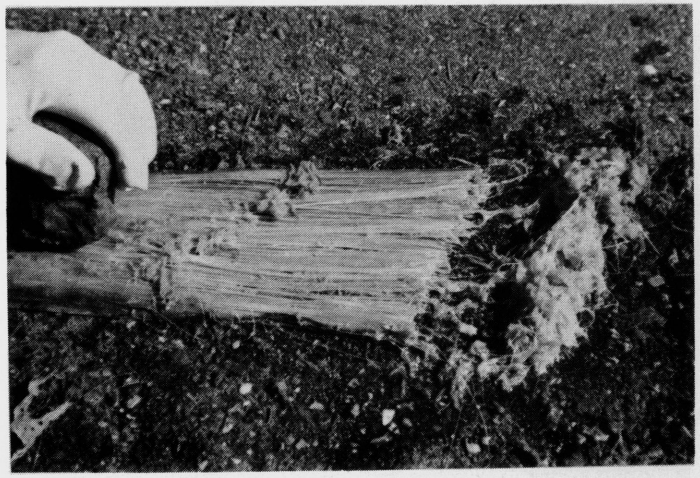


Fig. 3. Scraper Planes from the Valley of Oaxaca. a-c, low power photomicrographs of wear patterns on working edge of three scraper planes (linear magnifications are indicated); a, 3-22680 (dulled edge); b, 3-22679 (dulling and associated striations); c, 3-24175 (dulling and associated deep striations); d, d', frontal and dome views of 3-24160; e, e', frontal and dome views of 3-24175.

a



b



c

d

Fig. 4. Experimental Use of Scraper Plane. a, decortication of Agave leaf; b,c, expressing the pulp from the leaf; d, leaf end after completion of experiment.

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