

ETHNOGRAPHIC INTERPRETATIONS

7-11

BY

A. L. KROEBER

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INTRODUCTION

NUMBERS 7-11 directly continue *Ethnographic Interpretations 1-6*. "Yurok National Character," number 7, speaks for itself; it attempts to formulate the psychology of a culture. Number 8, on Athabascan glottochronology, necessarily involves the whole far-flung stock, but within California it concerns peoples adjacent to the Yurok. The study deals with procedures bearing on the classificatory and time aspects of a group of related languages. Also ethnolinguistic is number 9, on recent spreads of languages as inferable from their relative uniformity; the primary accent here is on the variable of space, the time being so brief as to count as a virtual constant. The frame of consideration begins broadly, but focuses chiefly on the Monache, Yokuts, and Miwok peoples of the San Joaquin Valley. "Problems on Boscana," number 10, deals with versions and interpretations of this most famous of ethnographic documents to emanate from the Franciscan missions in California. The Desert Mohave of study 11 are a legendary people in the far southeast of California. The slippery problem concerning them is to separate culturally invented patterns of fictitious history from memory of actual events.

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7. YUOK NATIONAL CHARACTER

THE YUOK were a small tribe or nationality of Indians in northwestern California along the lower Klamath River and adjoining sea coast. They numbered about 2,500 when first reached by Americans in 1850 and had had no previous Spanish contacts. They were surrounded by five tribes with similar customs and institutions, all somewhat less populous, aggregating perhaps 6,000 additional persons, by my estimates. Recent computations by Cook (1956) increase the population of this little ethnic world somewhat short of doubling it. Tribes beyond the nuclear six possessed a simpler and less accentuated culture, and the Yurok had few contacts or relations with them.

In 1900, those Yurok who were 70 years old had been adults when the native culture was first impinged on. Their knowledge of that culture was therefore first hand. In fact, at many points the cultural practices were still unchanged, which is why it was selected for study. At the same time, transformation of the Yurok into disadvantaged and second-class Americans was going on as the result of contact with our infinitely more massive society—a transformation that has been accelerated since 1900. This process proved traumatic for many Yurok. It is not gone into here.

The undisturbed, pre-1850 native culture seems to have been largely in static balance. It has been described in systematic ethnographic outline in the first four chapters of the *Handbook of Indians of California* and in special monographs. The pages that follow are an attempt to separate out and precipitate an ethnographer's impressions and his experiences with living or remembered Yurok who were still leading a predominantly native life, and to formulate these in personality terms. It is perhaps an unachievable task, but one that ethnographers will continue now and then to attempt, probably without ever satisfying psychologists.

This is a descriptive account, where Erikson's monograph of 1943 is explicative. The present tense must be construed as a narrative one, referring to a century or more ago.

The Yurok are an inwardly fearful people, cautious and placatory. Before other persons, pride often covers up their fear, but in the face of nature, taboo, or fate, they are timorous and propitiating. Moreover, they are suspicious of motives, quick to become jealous, and, by their own accounts, given to envy.

They are touchy to slight, sensitive to shaming, quickly angered. Their restraints of prudence break with a jerk, and they are then likely to explode into reckless violence of speech and anger. They hate wholeheartedly, persistently, often irreconcilably. They scarcely know forgiveness; their pride is too great. If unlimited acting-out of hostility is unfeasible, they take refuge in the negation of blotting out an opponent's existence by complete nonintercourse, within which they continue to nurse their hurt. Native law recognizes the perseverance of this by stipulating that settlement for injury include abrogation of the condition of nonintercourse, though common experience with one another has taught the Yurok to more than half expect continued hate after settlement and formal reconciliation.

They are acquisitive, but even more avaricious and retentive. They are particularly greedy for treasure, whose possession brings prestige and which is never

given away gratis except in partial return of bride price. But they are not greedy for food; liberality with food brings credit, and restraint in one's own eating is an index of good breeding and success. Acquisition of property by tenacious production is recognized, but its actual rewards are probably modest. The largest transfers of property follow on misfortune: they consist of fees paid shamans for treating the sick and of compensation paid for death or injury of close kin. (The full marriage of daughters also brings wealth, which however is diminished by return of a partial unstipulated equivalent.) The greatest reliance for acquisition is on the supernatural and one's own will—a willing through abstinence and deprivation, and by sheer pertinacious wishing, weeping, insistence, and proclaiming—devices psychologically effective because they direct and focus volition. In short, the Yurok depend more on concentration of mind to acquire wealth than on extraverted activity.

That they are litigious, resourceful in finding and countering claims, and stubborn in maintaining rights follows from their sensitive pride, their defensive avarice and aggressive acquisitiveness, and the prestige they attach to wealth. The cultural counterpart is a complex and refined system of law almost wholly resolvable ultimately into claims for property.

Perseveration characterizes not only the hates of the Yurok but all their affects, and the strength of their perseverations tends to deepen with increasing age. They become intensely attached to places, to habits, to their property; the feeling is more one of longing, regret, and nostalgic remembrance than of active satisfaction. Affection and sadness intertwine with it. Serenity is rare in their lives; rooted attachment ever present. They recognize the danger of such concentration when it is misdirected, as toward a pet or supernatural animal; this brings its own tragedy in such consequent loss of normal love objects as death of one's children.

In terms of long-recognized temperaments the Yurok are most often melancholic, punctuated by cholera. They look upon the sanguine man as a rash fool, the phlegmatic one as a sort of base clod.

Abstemiousness marks the Yurok attitude to life, verging at times on asceticism. They believe in the virtue and power of deprivation—strong restraint in eating, in sexual pleasure, in any indulgence. They do not urge limitation of intense wishing for wealth, for it is through punishment, through self-deprivation and self-pity, that fulfillment of such wishing is heightened. Money is believed to flee the house in which it is subjected to the indignity of witnessing the loosening of restraint on sexual pleasure; therefore proper intercourse takes place outdoors. As for the men's sweat-house, a woman may not even enter it; when a woman shaman novice properly dances in it, she is in trance and therefore sexually clean, and the structure is partly dismantled for the occasion.

Puritanism in loving, in genital exposure, in decency of speech or gesture is as characteristic of the Yurok as is the puritanism of thrift and capitalism. Like puritans they feel sin as well as shame, expiating it by public confession on threat of loss by death of one of their loved ones—itsself a puritan's ordeal of choice.

A multitude of specific, minute "don't" taboos of most miscellaneous and highly special character fill the interstices of the larger fundamental restrictions. These endless little restraints are relieved and illumined by the occasional contrasting

flash of a traditional belief expressing a daydream of extreme indulgence—such as eating a whole salmon in one ritual sitting, or cohabiting ten times in a night—but characteristically such feats are also regretfully thought of as unattainable.

With deprivation goes limitation. The Yurok love a small, snug, known, unchanging world, and in imagination often contract their universe, even short of the bounds of their actual knowledge. But they identify with this shrunken core of a universe intensely and passionately. It follows that they are incurious and really antipathetic toward what may lie beyond, toward all that is not of immediate and familiar affective concern. And they want their world stable; they dread its tilting and slipping, its shaking by earthquake, its flooding, its invasion by famine or by epidemics advancing like clouds. It is the warding off of these threats that is the formal motivation of their greatest rituals, the "World Renewals."

The Yurok like to fit and adapt themselves into their world. Their houses have the center sunk into the ground, the gables low, the eaves still lower; the sweat-houses fit in even deeper; both often nestle under a tree or crag or slip into a natural slope; there is no effort at all to make buildings stand out boldly. The dwellings are named, most often from their location: by the trail, at the elderberry or bay tree, in the middle, farthest uphill; sometimes by function: where they dance or dress for it; never in commemoration of a person or particular event. On the contrary, people derive their usual designations and appellations—their actual, individual names are private and are avoided rather than used—from their village settlement, among people at a distance; from their particular house, among those nearer and acquainted with it. Coupled with the house name is their marital status: married into, married to a woman born in, living with a woman of, divorced from, widowed in. This double device identifies persons socially as well as geographically, but it is the precise familiar society and the intimate location that are discriminated—not placement in any broad general scheme.

The lack of anything abstract or universal is evident also in the directions recognized by the Yurok, which are those of the flow of water at any given spot of the slope, to or away from water, whether this side or across stream. As the drainage of the area, including the main river, is tortuous, the directions, such as "up-river," sometimes happen to agree with solar or cardinal directions such as "east" but often disagree radically with them. The Yurok have not tried to compromise or reconcile the two possible systems but have simply discarded the abstract or cardinal method in favor of the localized, particular, and qualitatively more concrete one.

In manners, the Yurok, in spite of their aristocratizing values, make a show of democratic affability. They are perceptive, courteous, normally sensitive to the sensitivities of others, grave but able to smile, basically reserved—never telling all.

Individuals vary considerably in neatness. They bathe frequently, especially as part of the use of the sweat-house, and for all major ritual purification. Among them tidiness with objects runs toward emptiness. The terrace before the house is swept clean; the formulist for some rituals is called "he who brushes it out," and in all ceremonies he does keep clear the dance strips and the incense hearth. Sweat-houses are bare; and a well-brought-up, industrious housewife has enough

baskets for orderly storage of her provisions and effects. But it is hard for them to throw away anything; in the less well-kept homes discarded utensils, worn-out baskets, and maggoty dried food tend to accumulate in heaps and clutter the space needed by persons. Lack of both axes and saws gives a ragged appearance to the eaves of houses, for the projecting plank-ends weather unevenly; and the indoor fuel sheds, in which the split and crooked limbs of broken firewood cannot easily be stacked, also look disorderly.

There is considerable sense of workmanship and technological standard. Texture of artifacts is highly appreciated, especially in such hard materials as stone and antler, less in wood, but again in baskets and dressed skins. Except for the caps which women wear and which are an index of their being well-dressed, basketry is definitely sober and unshowy, without feathers, shell, or other attachments, its patterning limited mostly to a single band, and that negative—a paler overlay on root-brown—but evenness and fineness of stitch, surface, and contour are prized. There is also an evident feeling for texture of fur and feathers in the treasured dance regalia; the feathers, and sometimes pieces of fur, lie flat and smooth rather than bristling or radiating. In the best of the highly valued large display flints and obsidians, even and ripple-like flaking is achieved.

With all their feeling for material and surface, the Yurok decorate very simply, chiefly with triangles and diagonal quadrilaterals, never representing anything; that impulse seems wholly lacking. Even for objects—elkhorn spoons and purses, cylindrical dance baskets which sometimes almost force a symbolic interpretation on us—the Yurok consistently deny any “meaning” of shape. Their ritual face-and-body paints, their dance postures are arbitrarily traditional, never mimetic.

Only in straight magic, usually practiced individually and often in secret, does a definite mimetic symbolism frequently enter. And verbal symbolism has some occurrence in myth and legend—image, metaphor, occasional brief similes.

The Yurok do not scalp, decapitate, or spoil the body of a slain enemy. In fact, on the beach they drag his body above the surf for his kinswomen to claim. This may be because if the corpse is injured or lost, the punitive damages to be paid are increased. But even so, this fact implies discrimination under strong passion. They concentrate the aim of their hate on actual hurt instead of diffusing it in a flood of expressive gestures. The wish to hurt the living man may become overpowering, but in spite of their perseveration of affects, the wish, once accomplished, does not continue to indignity toward the corpse. In fact, if any mutilation of the body is practiced, it is by the bereaved women, who may run arrows into the dead body of their kinsman hoping that their magic will bring the same fate on his killer; they will mutilate their own murdered sons or fathers to achieve revenge for them.

A somewhat reminiscent dignity is extended to women, in spite of the extreme defilement residing in menses and parturition and the generic impurity of sex. Women claim and tend with impunity the bodies of their slain kinsmen. The full-married mistress of a prosperous house is a person of importance. The bride-price paid for her redounds to her credit as well as to her husband's, and she contributes equally to their children's social rank, which is definitely bilateral. She can inherit dance treasures, though she is disadvantaged in using and maintaining them. If of good family, she inherits from her mother a complete display dress outfit, and

from one or both parents intangible property such as spells and formulae, of which even a widow cannot be robbed. And finally, with few and second-rate exceptions, it is only women who are eligible to become curing shamans. Therewith they have in their control perhaps the greatest opportunity in the Yurok world to regular, consistent acquisition of wealth.

I might add that among the only slightly acculturated aged Yurok I knew or met in the first decade of the century, the men often seemed to me bitter and withdrawn, and some were of terrifying mien, but the old women made an impression rather of serenity.

I have long thought and still believe that the Yurok adhere to the classical anal temperament first recognized by Freud. My friend Erikson diagnoses rather an oral type of personality; he is a psychoanalyst gifted with extraordinary perceptiveness and therefore perhaps more interested in the subtle and veiled nuances than in obvious qualities of character. His imaginative comparison of the Yurok concepts of the body, its orifices, and food, of houses and sweat-houses and their doors, of river, salmon, and weir, is stimulating and illuminating. These are findings in a dimension which it is not for an ethnographer or culture historian to believe or deny or to use, though he can recognize them as valuable, as I do. As for the oral constituents, there is no doubt that the Yurok wealth-acquiring behavior connected with the sweat-house strongly enacts infantile attitude and behavior, and I have myths that emphasize the similarity even more. And a great deal of the thousand-headed traditional practices of magic is certainly at least puerile in the symbolic fusion of its concept and affect. I do not therefore see any quarrel between recognition of anal and oral components of Yurok personality; each is presumably true on its own level and degree.

To those further concerned with the psychoanalytic aspects of the culture, two recent articles by S. H. Posinsky (1956, 1957) will be of interest.

As for the type of neurotic behavior among ourselves which customary Yurok personality most suggests to me, it is compulsion.

8. REFLECTIONS AND TESTS ON ATHABASCAN GLOTTOCHRONOLOGY

INTRODUCTORY CONSIDERATIONS

HOIJER'S INTERESTING PAPER ON "The Chronology of the Athapaskan Languages" (1956) applies the Swadesh lexicostatistical or glottochronological method to data of a speech stock the knowledge of which Hoijer controls more intensively than anyone else. The paper is therefore of prime importance. It is also startlingly low in the time depth it reconstructs for the original break-up of Athabascan—only about 1,300 years ago. Hoijer himself finds the figure surprising. A year earlier (1955, p. 93), I had expected "at least 20 centuries of separation, quite probably up to fully 30," in spite of the Athabascan languages' possessing "well-marked consistency."

The low figure resuscitates an old doubt of the method of glottochronology, of which most linguists have never wholly rid themselves. The technique assumes that the rate of change of languages is essentially or ideally more or less constant, enough so to make it profitable to assume constancy as a working hypothesis until it has been confirmed, modified, or disproved by being tested in a large variety of cases. But even among languages of the same stock it does not follow a priori that these would all alter at a uniform rate, irrespective of geographical, populational, and cultural factors. Figures from interstock comparisons would have to be accepted even more guardedly, since it is conceivable that languages of markedly diverse structure might vary in their "natural" rate of change. For instance, languages built up overwhelmingly of monosyllabic elements, as the Athabascan ones are, might change faster or slower than others. I would guess slower, on the theory that monosyllables are more likely to be hard residual cores. I shall not try to substantiate this intuitive semiconviction, but suggest it by way of illustration. Others may favor different factors as impelling toward either rapid or slow change.

But obviously if one set of languages were twice as resistant to alteration as another set, and the same formula were applied to them, the resistant set would emerge from computation seeming only half as "old" as the second set. The Athabascan and Uto-Aztecan groups have been recognized as stocks about equally long and with an analogous and approximately equal spread; it probably has been tacitly assumed that their principal component languages diverged about equally. It was on this generic parallelism, and on Swadesh's computation that Nahua, Papago, and Southern Paiute had diverged about 27 to 31 centuries ago, that I based my expectation that the Athabascan split-up would probably prove to be datable up to 30 centuries back. Hoijer's 13 centuries thus comes as a shock; and the possibility that the Athabascan languages are inherently more resistive to change suddenly confronts us with a certain insistence.

However, let us analyze Hoijer's results more closely before we grapple with this possibility, which might go far to undermine comparative glottochronology, at least as applied between different stocks.

EFFECT OF WORD LIST USED

Hoijer used, so far as the data allowed, the first or preferred 100 of the 200 words recommended by Swadesh in 1955. But adequate lexical data being scant, he se-

lected 78 of the first 100 and 22 from the second or supplementary list. This would, statistically, make no difference within the Athabascan stock, but it might lead to partially unconformable results on comparison with other stocks for which Swadesh's unaltered first or double list had been used. Presumably the supplementary words are less constant, and the use of one-fifth to one-fourth of them would make the Athabascan lists more labile, and therefore seemingly older than computations based wholly on the 100 words of the preferred list. The degree of warping of comparability is unknown. It would presumably not be very great, but probability of some warping would be expectable.

Hoijer's figures show no visible evidence of distortion due to his choice of vocabulary, nor any extreme aberrations. Two of the three geographical blocks have obvious internal homogeneity; in most cases adjacent languages within a block are also more similar than those not in contact. It is thus evident that in the main the differentiation of the Athabascan languages and dialects was slow, progressive, orderly, and in general conformity with what their geographical distribution would lead us to expect.

EFFECT OF INCOMPLETE VOCABULARIES

The orderliness, however, makes it possible to test the three or four languages with incomplete word lists for their conformability as compared with those represented by a full 100 words. If their conformity with geographic position is perceptibly less, that would inferably then be owing to their less full sample.

For eleven Athabascan languages, Hoijer had his full 100 words available, but for Mattole only 99, for Kato 94, for Galice 89, and for Beaver as few as 82. Eighty-two is just a different sample of a language than 82 + 18; it is bound to have less reliability; it is less likely to hit the truth as given by the whole language. The question is, how much less? Fortunately we can get some approximate idea of how much nonconformity is introduced with incompleteness of sample, by examining total context of results for Athabascan.

By context I mean trend or pattern or grouping in Athabascan as discernibly related to objective constants. Such a constant would be geographical position or distances between languages. Hoijer deals with six North Athabascan languages, five Southern or Apachean, four Western or Pacific. These three blocks are well separated on the map. We should therefore ordinarily expect a language to show greater similarity to others in its block than to those in remote areas. It would be in present or recent contact with those spoken near by, but expectably only in ancient contact with those at a distance. This would not necessarily have to be true, but if the distance-dissimilarity correspondence were notably disturbed, we would infer that special factors had been at work to bring about the irregularity. For instance, to take an extreme but unreal case: if Navaho showed more retention similarity with Carrier, Chipewyan, and Hare in the far north than with the neighboring Southern or Apachean languages, we could reasonably infer that its speakers had left the north much later than the Apaches had, and, after an independent migration by themselves, had still later settled in proximity to the Apache where these were already established in the south.

REARRANGEMENT: PLACE OF GALICE

To reveal whether or not less conformity exists for the short-word-list languages, it is convenient to arrange all the languages dealt with in a single tabulation in which members of the same block are kept together and are arranged, so far as possible, in some simple geographical order.

Hoijer has kept the blocks separate, but he was uncertain whether to class Galice, collected by Melville Jacobs, with the Northern or the Pacific block. He grouped it as Northern, "somewhat arbitrarily," he says, recognizing that there was "perhaps as much justification" for classing it with Pacific.

On purely geographical grounds Galice belongs in the Pacific division. Galice Creek is a tributary of Rogue River in southern Oregon. The Rogue River Athabascans, also known as Tututne, seem to shade into the Tolowa of Smith River, the next major stream to the south in northwesternmost California. Even such non-Athabascans as the Yurok know that Tolowa speech is distinct from Hupa, though related to it, but I have never heard mention of any material break in either speech or culture between Smith and Rogue rivers and suspect that Galice will prove to be not much more than dialectically different from Tolowa. At any rate, Galice was situated within the strip of Coast Range territory that extended from Umpqua River in Oregon to the headwaters of Eel River in California as a solid Athabaskan continuum except for the Yurok interruption on the lower Klamath. All the geographical facts thus demand placing Galice in the Pacific division.

RETABULATION

Next, for conspectus, I have brought Hoijer's five partial tables together into one (table 1). I have also filled this out to make two triangles symmetrical along the upper left to lower right diagonal, in short, to constitute a rectangle. This rectangle merely repeats each percentage figure once, and linguists, having an unusually high value for elegant economy of presentation, usually restrict themselves to a triangular half rectangle. This, however, means that the figures for all but one language have to be read around a corner when seriated, whereas in a rectangle one simply reads down a column, or for that matter, across it, along a line. This is so much easier for novices (besides allowing means to be added where desirable) that the redundancy of the rectangle may be pardoned for its greater ease of comprehension of the relations expressed by the table. I might add that if groups of numbers are replaced by graphic symbols, forming a diagram which elucidates comprehension by the eye, a symmetrical rectangle is almost a necessity.

In table 1, then, I have grouped together, first, six of Hoijer's North Athabaskan languages, then five Southern, then four Pacific, including Galice. The northern languages are arranged roughly in a north-south order; the southern, in west-east order. Within these blocks, one or two languages at or near the geographic center of the division average the highest means of retention percentages, from which the others slope down to the geographic margins of the block. These central and high-retention-mean languages are Chipewyan and Beaver in the north, Chiricahua and Navaho in the south.

In the Pacific division, the order is geographically linear from north to south, without any culmination in the middle.

In short, all the data in table 1, except the final row of means at the bottom, are taken from Hoijer's text; only the rearrangement is mine.

DEMONSTRABLE EFFECT OF INCOMPLETENESS ON RETENTION RATIOS

It is at once apparent that within the Pacific division the geographical order is not the order of linguistic seriation. Kato, as the most southerly, should have diverged from photo-Athabaskan first, and then have pushed on farthest, and Galice latest and least far; whereas in computed retention ratio Kato and Galice are closest together of the four Pacific languages.

TABLE 1
ATHABASKAN RETENTIONS CONSOLIDATED AND REARRANGED

	Ku	Ha	Chip	Bea	Sar	Carr	S.C.	Nav	Chir	Jic	Lip	Gal	Hu	Matt	Kato
Kuch.....	..	81	77	73	65	70	66	70	71	67	66	65	60	58	65
Hare.....	81	..	76	77	68	70	68	69	72	68	68	67	61	62	65
Chip.....	77	76	..	82	76	77	72	77	76	74	74	74	67	62	71
Beav.....	73	77	82	..	78	73	71	76	76	72	71	77	68	63	74
Sars.....	65	68	76	78	..	71	65	68	68	68	66	70	62	61	64
Carr.....	70	70	77	73	71	..	68	73	71	69	70	71	65	62	65
S.C.....	66	68	72	71	65	68	..	89	91	87	84	72	61	63	66
Nav.....	70	69	77	76	68	72	89	..	94	89	87	72	64	65	68
Chir.....	71	72	76	76	68	71	91	94	..	92	91	73	64	65	67
Jic.....	67	68	74	72	68	69	87	89	92	..	91	70	63	65	65
Lip.....	66	68	74	71	66	70	84	87	91	91	..	69	60	62	64
Gal.....	65	67	74	77	70	71	72	72	73	70	69	..	70	66	74
Hupa.....	60	61	67	68	62	65	61	64	64	63	60	70	..	67	67
Matt.....	58	62	62	63	61	62	63	65	65	65	62	66	67	..	70
Kato.....	65	65	71	74	64	65	66	68	67	65	64	74	67	70	..
Mean.....	68.1	69.4	73.9	73.6	67.8	69.6	73.1	75.7	76.5	74.3	73.1	70.7	64.2	70.8	67.5

Since Galice and Kato are also the two Pacific languages whose ratios are derived from incomplete lists, this deficiency is evidently the cause of their high similarity. The words missing are of course not the *most* basic ones, such as say eye or water, but somewhat less basic ones, and therefore more likely to be replaced by noncognate ones. This elimination from the data of part of the less stable words makes the ratio depend unduly on the residue of more stable terms and thus brings the retention percentage up, thereby *reducing* the time lapse as computed by formula. This is the opposite effect from that theoretically expectable from use of an off-standard vocabulary and is evidently a stronger influence. That is, use of incomplete lists compared with complete ones introduces a greater aberration, and in the opposite direction of shortening durations, than consistent use of somewhat remodeled full lists.

Mattole, with only 1 word missing, is not appreciably affected in its percentages. But Kato is 6 words short and Galice 11, and as there is only one item which they jointly lack, their ratio is built on only 84 (100 - 6 - 11 + 1) instead of 100-word comparisons.

Eighteen words are lacking from the sample vocabulary of Beaver in the north. The Beaver-Galice and Beaver-Kato lists both consist of only 79 pairs. Their retention ratios are, respectively, 77 to 74, as against 74 and 71 for Chipewyan-Galice and Chipewyan-Kato. Since Chipewyan has the highest mean ratio of the northern languages, but Beaver surpasses it in these two particular combinations, it seems as if Beaver's short word list had again raised its retention ratio.

Moreover, within the Northern block, Chipewyan behaves more stably in its correspondences, Beaver more variably. Thus:

CHIPEWYAN	BEAVER
Kuchin.....77	Kuchin.....73
Hare.....76	Hare.....77
Carrier.....77	Carrier.....73
Sarsi.....76	Sarsi.....78

It therefore seems that all three of the languages represented by incomplete lists have had their retention ratios affected, generally in the direction of the ratio being raised. A raised ratio results in computation of a reduced age. The 724, 823, and 724 years found for the separation of Beaver, Galice, and Kato from one another are therefore short of the presumably true age; we do not know by how much. In any event it is clear that the computed age of separation of these three languages is *relatively* too low, compared with intra-Athabaskan ages generally.

REDUCTION BY OMISSION OF SUSPECT RATIOS

Table 2 is a reduction of table 1 by simply omitting the three suspect languages. It has less spread but greater reliability. It also intergrades more evenly or continuously, especially within each of the three blocks.

NORTH ATHABASCAN, REDUCED

Of the Northern block in table 2, Chipewyan pretty consistently has the highest retention percentages with the Apachean and Pacific languages. This would indicate that its history has been the most conservative, that it has on the whole changed least since the split-up of proto-Athabaskan commenced, and that when groups began to drift south both inland and along the Pacific Coast, they probably started from a form of Athabaskan nearer to Chipewyan than to any other now represented in our data.

It is tempting to connect the relative conservatism or stability of Chipewyan with its approximately central position within the Northern block. It would there be most consistently in contact with fellow-Athabaskan dialects, whereas the more marginal dialects would have more contacts with foreign stocks. Kuchin and Hare form a unit northwest of Chipewyan. Carrier and Sarsi and presumably Beaver form a less close-knit unit lying west and on the whole south of Chipewyan. It is however to be noted that Sarsi on the edge of the Plains has a consistently lower similarity to the Apachean dialects than has Carrier, which is situated between the mountains near the coast, and that Carrier runs lower than Chipewyan although Chipewyan lies, on the whole, farther north at present than Carrier.

SOUTH ATHABASCAN, REDUCED

The Southern or Apachean block is pervaded by greater homogeneity than the Northern or Pacific. The retention ratios fall between 84 and 94, corresponding to ages of only 400 to 150 years by the assumption which posits 80 or 81 per cent normal retention within one language in 1,000 years. It would seem dubious whether we have here anything much more than five well-marked dialects of a common Apachean language. Hoijer comments that the figures for the ten Apachean interrelationships do not accurately reflect the split between the western and the eastern (Jicarilla, Lipan) Apachean dialects, which latter are set off by proto-

TABLE 2
RETENTION RATIOS OF 12 LANGUAGES HAVING FULL WORD LISTS

	Ku	Ha	Chip	Carr	Sar	S.C.	Nav	Chir	Jic	Lip	Hu	Matt
Kuch.....	..	81	77	70	65	66	70	71	67	66	60	58
Hare.....	81	..	76	70	68	68	69	72	68	68	61	62
Chip.....	77	76	..	77	76	72	77	76	74	74	67	62
Carr.....	70	70	77	..	71	68	72	71	69	70	65	62
Sars.....	65	68	76	71	..	65	68	68	68	66	62	61
S.C.....	66	68	72	68	65	..	89	91	87	84	61	63
Nav.....	70	69	77	72	68	89	..	94	89	87	64	65
Chir.....	71	72	76	71	68	91	94	..	92	91	64	65
Jic.....	67	68	74	69	68	87	89	92	..	91	63	65
Lip.....	66	68	74	70	66	84	87	91	91	..	60	62
Hupa.....	60	61	67	65	62	61	64	64	63	60	..	67
Matt.....	58	62	62	62	61	63	65	65	65	62	67	..
Mn with 11.....	68.3	69.4	73.4	69.5	67.1	74.0	76.7	77.7	75.7	74.4	63.1	62.9
Mn 4 N.....	73.3	73.8	76.5	72.	69.5							
Mn 4 Ap.....	87.8	89.8	92.	89.8	88.3		
Mn 1 Pa.....	67.	67.

Athabascan *t becoming k. This change is conspicuous, but is also an isolated feature, and experience shows that a classification reflecting multiple factors, as the retention ratios undoubtedly reflect, is expectably more significant of the total situation just because it is more complex than a single shift.

The highest intra-Apachean ratio is Chiricahua-Navaho, 94; the next, Chiricahua-Jicarilla with 92, extends across the t-k line split. Hoijer's lexical samples show that though the differences are not very great, Chiricahua has the highest mean of resemblances: Chiricahua 92, Navaho and Jicarilla both 89.8, Lipan 88.3, San Carlos 87.8. Chiricahua has also been geographically most nearly central in the past century and perhaps longer; the languages with the lowest mean retentions, Lipan and San Carlos, are also the farthest west and southeast. These two also show the lowest ratio between themselves within Apachean, namely 84. They have certainly had no serious contacts with each other as far back as we can identify them in history. Hoijer notes that all five languages have been in "more or less" close contact for the last two or three centuries, which fact may account in part for the generally low divergences. This opinion is strengthened by the fact that divergence within the area increases with geographical separation.

As regards similarities outside Apachean, Chiricahua and Navaho both form a close unit and without exception outrank the three other dialects in their extra-Apachean retention ratios.

It is probable that ancestral proto-Apachean, when thus separated from other Athabascan, is best represented by modern Chiricahua-Navaho, and that the three other dialects, lying to the west, north, and east, diverged away from the central nucleus which remained less altered. The $t > k$ isogloss then was superimposed on this more generic process.

It seems evident that comparative retention percentages cut rather fine, that they reflect geographical positions, and that this fact in turn means that divergence is generally accelerated by distance and separation but is slowed by maintenance of contact with intelligible languages.

PACIFIC ATHABASCAN

There is not much to say about the relations within the Pacific division in our table 2 because it is there reduced to two languages, Hupa and Mattole. Also, these are the two middle ones in the linear distribution. In fact, if we count Hupa as including Chilula and Whilkut as dialects, Hupa and Mattole are separated only by a tongue of Nongatl on lower Eel River and its Van Duzen Fork. Thus it is surprising that they show so low a retention percentage as 67, corresponding to nearly 1,000 years by formula.

CORRECTION OF SEPARATION PERIODS

Theoretically, as I have said, the use of incomplete word lists for a few of the languages tends to produce a seeming decrease of differentiation—*increase of retention ratio*—because the missing words are likely to lie in the less basic and more variable parts of the vocabulary. An artificial decrease of differentiation, however, brings with it a fictitious decrease of age since separation. To put it in reverse, the languages have probably been actually separated longer than calculation indicates; this means that the computed chronology, which surprised Hoijer and the rest of us by its over-all recency, is not really quite so recent as the calculations say.

I have recalculated Hoijer's table VI from my reduced table 2 of 12 languages only. This gives only 45 interrelations instead of 70, as shown in table 3, which incorporates Hoijer's VI. His number of cases is given first in each double column, headed "15." The number of cases yielded by my reduced table 2 is headed by "12." It is apparent that in each interdivisional double column the longer time ranges remain unimpaired, the medium ones are reduced somewhat, the shortest are heavily reduced.

This result does not affect what Hoijer says of the relative history of the three divisions, all of which I accept. It merely lengthens somewhat the average time spans during which the three divisions evolved.

ESTIMATED CORRECTION OF RETENTION RATIOS

It occurred to me to make the experiment of correcting the reduced table 2 by consistently adding an arbitrary increment to all the retention ratios of the lan-

guages represented by incomplete word lists, to see whether or not these corrections would "improve" table 1, so as to bring its internal coherence and fit of correspondence with geography more or less to the level of table 2.

I chose a percentage increment of 6 for Beaver, which had the greatest number of lacks in its word list, of 5 for Galice, of 4 for Kato which had the fewest omissions, and of 10 for the three interrelations of these three. The result is shown in table 4. The internal orderliness of the table and its consistency with geography are certainly improved even by this somewhat blindly intuitive stab at correction.

It would have been better yet if the increment had been a straight 5 per cent for each of the deficient languages. This would have brought Beaver up to 67.2

TABLE 3
INTERBRANCH DATES

Ages	No/Pa*		Ap/Pa		No/Ap	
	15	12	15	12	15	12
1,400.....
1,300.....	1	1
1,200.....	1	1	1	1
1,100.....	7	6	4	4
1,000.....	6	1	7	5	4	4
900.....	2	1	2	..	8	8
800.....	2	10	7
700.....	2	9	4
600.....	4	2

* Columns headed "15" from Hoijer's table VI; "12," short-list languages omitted.

in its mean percentage or above Carrier, and Kato down to 61.6 or below Mattole. However, all the other means would have been slightly altered, and it did not seem worth risking new errors and involvement in further and further approximative corrections. The principle seems established that by sufficient testing and fitting of increments, the irregularities in ratios due to irregularity in available lexical material can be pretty well compensated for.

At any rate, the experiment shows approximately how great the error is that was introduced by incompleteness of word lists. In this case, it led to excess of a tenth to a fifteenth. If, on the other hand, such correction had smoothed out certain irregularities, but new and consistent ones appeared, it could be inferred that present-day geographical conformity had been interfered with by events in the history of the particular language or languages involved in the irregularity. I am confident that, in spite of my arbitrary corrections, or rather because of them, table 4 better represents the actual situation, the "true" relations, obtaining in Athabascan than does table 1 which shows Hoijer's ratios for fifteen languages. It is probably less true than the purged table 2, but fifteen languages are a much better sample of the Athabascan stock than are twelve. The gain in illumination by correction is particularly great in the Pacific division because four languages give us six internal relations as against only one from the two Pacific languages in table 2.

MEANS OF RETENTION RATIOS AND THEIR SIGNIFICANCE

I have added at the bottom of table 4 the averages (means) of the retention ratios of each language, Beaver, Galice, and Kato being in their corrected values. There is first the mean of 14 ratios with all the other languages; and second, the mean ratio which each language has with the other languages of its own block or division—Northern, Apachean, Pacific.

It is worth while comparing the first set of means with the second sets. Obviously the means within a division would expectably be higher than the all-Athabaskan means. The size of the difference might be significant.

TABLE 4

INTERRELATIONS OF 15 ATHABASKAN LANGUAGES WITH CORRECTION^a FOR BEAVER, GALICE, KATO

	Ku	Ha	Chip	Bea	Carr	Sar	S.C.	Nav	Chir	Jic	Lip	Gal	Hu	Matt	Kato
Kuch.....	..	81	77	67	70	65	66	70	71	67	66	60	60	58	61
Hare.....	81	..	76	71	70	68	68	69	72	68	68	62	61	62	61
Chip.....	77	76	..	76	77	76	72	77	76	74	74	69	67	67	62
Beav.....	67	71	76	..	67	72	65	70	70	66	65	67	62	57	64
Carr.....	70	70	77	67	..	71	68	73	71	69	70	66	65	62	61
Sars.....	65	68	76	72	71	..	65	68	68	68	66	65	62	61	60
S.C.....	66	68	72	65	68	65	..	89	91	87	84	67	61	63	62
Nav.....	70	69	77	70	73	68	89	..	94	89	87	67	64	65	64
Chir.....	71	72	76	70	71	68	91	94	..	92	91	68	64	65	68
Jic.....	61	68	74	66	69	68	87	89	92	..	91	65	63	65	61
Lip.....	66	68	74	65	70	66	84	87	91	91	..	64	60	62	60
Gal.....	60	62	69	67	66	65	67	67	68	65	64	..	65	61	64
Hupa.....	60	61	67	62	65	62	61	64	64	63	60	65	..	67	63
Matt.....	58	62	67	57	62	61	63	65	65	65	62	61	67	..	66
Kato.....	61	61	62	64	61	60	62	64	68	61	60	64	63	66	..
Mn Ath.....	67.1	68.4	72.9	66.2	66.8	68.6	72.1	74.7	75.5	73.3	72.1	65.	63.2	62.6	62.6
Mn Nor.....	72.	73.2	76.2	70.6	71.	70.4									
Mn Ap.....	87.8	89.8	92.	89.8	88.3				
Mn Pac.....	63.3	65.	65.3	65.7

^a Corrected from table 1 by: Beaver, -6; Galice, -5; Kato, -4; interrelations of these three, -10.

The six Northern languages run only from 2 to 5 percentage points higher internally than their all-Athabaskan ones. The smallness of this difference conforms to expectability, because the Northern languages occupy much the largest territory, and what is presumably the ancient all-Athabaskan habitat. Also, in spite of population thinness, the Northern division is likely to include the largest number of languages. Further, these are most heavily represented in our sample—six languages as against five and four.

It will be observed that for Kuchin and Hare, the two most northerly languages, the interval is nearly 5 percentage points; for Chipewyan, Beaver, and Sarsi around 3 to 4; for Sarsi, to the south and the only Northern language in the open plains, not quite 2. Kuchin and Hare are quite evidently somewhat specialized within Northern, which seems best represented generically by Chipewyan at present. The ratios of Kuchin and Hare are still high with Chipewyan—77 and

76 as compared with 81 *inter se*—but then drop off with the other Northern languages to almost the same average as with Apachean, around 67. It is this specialization of Kuchin and Hare that makes the difference between their total and Northern means greatest.

The Apachean difference between Athabascan and intra-Apachean means is quite high, simply because the Apachean languages are so similar that their internal retention ratios are much the highest anywhere in Athabascan. Their *lowest* retention is 84 as against the otherwise highest of 81 manifested by Kuchin with Hare. Again it is clear that the ancestors of the present Apachean division left the north and moved south as an essentially homogeneous unit; they then accomplished their modest diversification in the south at a relatively recent time in Athabascan history.

RETENTION MEANS IN PACIFIC ATHABASCAN

The situation in the Pacific division is difficult to interpret. The interval between internal Pacific and all-Athabascan means is very low, only 2 or 3 per cent, and for Galice it is even negative—higher for Athabascan generally (65) than for the mean of Galice's three Pacific interrelations (63.3)! Of course all Galice ratios are artificially corrected—and downward, so this anomaly does not mean too much. Also, Hupa, which is uncorrected, shows an average ratio with Northern (63) almost as high as does Galice (65).

The real trouble to understanding the significance of the Pacific ratios is that they all run so uniformly even, internally and externally. Thus:

	57-59	60-64	65-69
24 ^m ratios with Northern.....	2	15	7
20 ^d ratios with Apachean.....	0	12	8
6 ratios in Pacific.....	0	3	3

I can conceive three possible explanations for the Pacific languages being almost evenly diverse both from one another and to the outside. The first of these explanations is that the Pacific division branched off as a unit, like the Apachean, but so much longer ago that it has diversified far more. The second explanation has several languages splitting off and taking the same general path southward but at different times. The third would attribute the high diversity of the Pacific languages among themselves not only to the time elapsed since separation and to the distance traversed, but also to conditions encountered on the way while reaching their final habitats.

The last explanation has in its favor the broken, locally varied terrain inland from the Pacific Coast; the presumably already dense population encountered there, which would further restrict rapid progress and would lead to some migrants settling down while others pushed on; and the fact that they must all have encountered a great variety of linguistic contacts. All these factors together would produce considerable differentiation but in multiple and various directions.

These three explanations are not mutually exclusive. They may have been si-

multaneously and concordantly effective. Yet I would see the third and most complex explanation as the one easiest to overlook, and yet likely to account for the factors of greatest strength.

UNCERTAINTIES OF ABSOLUTE DATING

It is evident that Hoijer's comparative word lists throw much light on the classification of the Athabascan languages and therewith on their relative time depths. But there is no point in all these relative perspectives that connects with a fixed datum in time that would allow us to convert the relative into even an extrapolated or estimated absolute chronology.

Thus, Hoijer, basing on the Lees formula, computes 419 years ago as the oldest date of intra-Apachean diversification in the American Southwest. This is A.D. 1537, three years before Coronado's arrival at Zuni. Yet the archaeologists, rightly or wrongly, have been pushing the invasion of the Southwest by the ancestral Apache-Navaho further and further back. Harold Gladwin's 1957 *History of the Ancient Southwest* has them arrive around A.D. 1000, about 957 as against 419 years ago. Either a lot of the archaeological interpretation is baseless so far as Athabascans are concerned, or Hoijer's computations are badly erroneous because the basic glottochronological formula which was given him by Swadesh and Lees is invalid in the Athabascan case. Even if we consider, instead of intra-Apachean differentiation, the computed separation of Apachean from Northern, we are far from having 900 to 1,000 years to operate in. The Navaho and Chiricahua separation from Chipewyan and Beaver computes out as only 628 to 660 years ago, say around A.D. 1300 for the beginning of the long journey south.

Let us make one more effort to extricate ourselves from the dilemma. I know of no absolute dendrochronological or other date in the Southwest that can be positively, nonspeculatively connected with Athabascan speech. But I would like to essay matching some interstock glottochronological computations against simpler lexical comparisons.

COGNATES IN A TEN-WORD CHECK IN ATHABASCAN

I have extracted from Hoijer's word lists the forms which express ten concrete and simple meanings in his fifteen languages. The meanings are so simple that there are no blanks or vacancies. The point is to see how many separate, noncognate forms occur among these 150 words.

Semantically, the ten denotations include one animal, four parts of the body, four major natural objects or substances, and one numeral. I cite with each of these an abstracted "average" Athabascan form, based on inspection and intended to be only approximate. It is in no sense based on technical linguistic reconstruction making use of phonemic shifts since proto-Athabascan. These are the ten meanings and their abstracted average forms:

dog	h ^a	sun	ža, ša, sa ; or perhaps i for a
bone	-c'in	water	to, tu
ear	-ža (ga)	stone	ce
eye	-naa, -daa	fire	kon [†]
nose	-č ⁱ š	three	taa (ge)

Any Athabascanist will recognize these forms, though he might prefer to express their average by somewhat differently chosen phonemes.

How many of the 150 word forms are noncognate with the 10 basic forms? The number of noncognates is obviously a measure of diversity, and an objective, although limited, measure.

I reckon five probable or possible noncognates. These are:

Dog in Chiricahua. The other Apachean languages have the stem hi^n , compounded with an element $-čaa'y$. Chiricahua shows this added element in the form $-žaa$, but does not have pan-Athabascan hi^n . Its full form is $ke-žaa$. The $ke-$ is presumably noncognate with hi^n .

Nose, usually $-čičš$, is $-n-wo^n$ in Beaver, $-i^n-γo^n$ in Hare, $miišš$ in Galice. The $-šš$ of the last may be cognate with $-čičš$ (compare Carrier $-n-cis$, Jicarilla $-čičš$), but I do not know the value of $mii-$, so count the Galice form as a possible cognate. Beaver and Hare $-wo^n$ and $-γo^n$ look related, and may be cognate through wo^n with basic $čičš$: cf. Hupa $-n-čwīW$.

Sun is basically something like $ša$ or $ža$, which is lacking in San Carlos. The four other Apachean languages add to it $-go-naa'ai$ or an obvious variant. San Carlos expresses sun simply by $yaa'ai$; the usual $ša$ has been lost.

Thus we have in 150 cases 2 in which another element in a compound has replaced the basic stem, and 3 instances in which the basic stem is replaced by a form that may be a variant cognate or a noncognate. In 145 cases out of 150 only clear cognates occur; 7 out of 10 meanings are expressed throughout only by variants of a single stem.

THE TEN-WORD CHECK IN MAYAN

A convenient comparison for the same ten denotations can be made for Mayan from Otto Stoll's *Zur Ethnographie der Republik Guatemala* (1884), by using the tabulated vocabularies on his pages 48-69. Seventeen languages are represented, including Huastec as the only one not contiguous with the rest. Only the Mam list contains serious gaps; 3 of our 10 words are lacking from it.

I cite in Stoll's orthography, giving one, two, or occasionally three forms for each separate stem, followed by the number of languages in which that particular stem occurs. I also number the different stems 1, 2, 3 when they are clearly noncognate, but 1, 1a, 1b, 1c when they may be much-differentiated cognates. Not being a comparative Mayanist, I have had to go by inspection; but when in reasonable doubt about forms, I have included them under possible cognates. "H" denotes Huastec.

dog	1 tz'i, chian, 13; 2 pek (pico H), 2; 3 nichu, ajicho, 1.
bone	1 bak, bakél, beklék (H), 16.
ear	1 chiquín, xiquín, xic, xutzúm (H), 17.
eye	1 vuach, vuich, 10; 1a uti, jut, 2; 1b ich, 2; 1c sitj, sat, 3.
nose	1 ni, 7; 2 tz'am, 6; 3 ju, vuúj, 4.
sun	1 k'in, k'i, k'ij, 11; la aquicha (H), 1; 1b k'akál, k'agu, 2; 1c sak'e, 1.
water	1 jaá, ja, a, jab, 17.
stone	1 tun, ton, tunich, tujúb (H), 8; 2, abaj, 7; 3 cup, 2.
fire	1 k'a'k, 14; 2, xam, xamál, 2 (Quekchi, Ixil).
three	1 ox, oxib, ixiém, óxvual, uxpé, 17.

This listing yields 4 words (bone, ear, water, three) derived in all languages from a common stem; 2 (eye, sun) in which all the forms may be cognate but this is not evident; and 4 (dog, nose, stone, fire) in which there are two or three obviously noncognate stems. The total number of occurrences of clearly noncognate stems, beyond the most common stem of each denotation, is 22 out of 165 (10 words in 17 languages less 5 gaps). If we count the cases where there may or may not be cognateness (the 1a, 1b, 1c instances), we find 11. The maximum total comes to 22 + 11 = 33 out of 165, as against Athabaskan 5 out of 150 and 3 of these merely possible noncognates. It is clear that the frequency of noncognates is much higher in Maya than in Athabaskan, although Maya consists geographically of one compact block with only a single, not very distant outlier in Huastec.

The difference can also be expressed in terms analogous to those of conventional glottochronology. Fifteen Athabaskan languages have 105 interrelations for each word, making 1,050 for 10 words. Five clearly noncognate plus possibly noncognate words each enter into 14 interrelations, making 70 dissimilarities in 1,050 cases, or 6.7 per cent, cognates constituting 93 per cent. But in Mayan, I compute 547 dissimilarities in 1,271, or 43 per cent, with clear cognates numbering only 57 per cent. I do not go on to convert these ratios into centuries because strictly they are not quite the same as the retention ratios we have dealt with before. But there can be no real doubt that, so far as the two comparable little samples have validity, either the fifteen Athabaskan languages have diverged from their ancestral mother language far more recently than the seventeen Mayan ones, or that their words have been much more resistive to fundamental changes including replacement by noncognates.

This conclusion tallies roughly with Swadesh's computation that Yucatec (Maya proper) and Huastec have been separated for 32 centuries, as against Hoijer's 13 for Athabaskan.

THE TEN-WORD CHECK IN UTO-AZTECAN AND ROMANCE

I have made a comparison also of the words of the same ten meanings in seven Uto-Aztecan languages: Nahua, Cáhita, Papago, Hopi, Southern Paiute, Tübatulabal, Luiseño. Ear, eye, sun, three show only obvious cognates throughout; bone, nose, fire show 2 stems; water, stone, 3; dog as many as 5; altogether 21 apparent stems for ten denotations. Seven languages have 21 combinations per word, 210 for the 10 words. Of these I compute 139 to show cognate forms; 71, obvious or probable noncognates, or 66 and 34 per cent respectively. The proportion of noncognates is again much higher than in Athabaskan, but approaches the 43 per cent of Mayan. The Uto-Aztecan differentiation age as given by Swadesh is also close to but somewhat below the Mayan. It is 2,700, 2,900, 3,100 years respectively between Papago-S. Paiute, Papago-Nahua, and S. Paiute-Nahua, as against 3,200 years for Huastec-Yucatec. The retention ratios for the three Uto-Aztecan pairs are 32, 29, 27 per cent as against 26 per cent for Huastec-Yucatec, still according to Swadesh.

It begins to seem as if the 10-word special sample test gave results that run parallel to the regular glottochronological formula. To be sure, it is not usual to corroborate results from large samples by smaller ones; but this small sample

consists of a picked elite of words generally supposed to be particularly resistive to displacement by noncognates.

The same 10 words in Buck's 1949 synonym dictionary of Indo-European for the four Romance languages there dealt with—Italian, French, Spanish, Rumanian—show cognates in every instance except that Spanish has *perro* for dog instead of a derivative from *canis*. This is one case in 40, which results in 3 noncognate matchings out of 60, as against 57 cognate ones. This is 5 per cent, pretty close to the 6.7 per cent found for Athabascan. I do not venture to fix a precise time point for the beginning of the differentiation of "Latin" into the four Romance languages, but if we estimate it as occurring somewhere between A.D. 250 and 500, there is a historical duration of 17 to 14 centuries for Romance as against a computed Athabascan one of about 13 centuries. There is at least a hint of corroboration here for the suggested time lapse of Athabascan.

Incidentally, although there is only one noncognate in the 40 Romance forms, two of the ten meanings have moved on to new forms in the passage from Latin to Romance. For the idea of "fire," *focus*, "hearth," has replaced *ignis*; for "stone," the derivatives are not from *lapis* or *saxum* cited first by Buck, but from the third-place *petra*.

SUMMARY AND CONCLUSIONS

The following inferences can be drawn as to glottochronological method and theory.

1. It seems likely in principle though as yet undemonstrated that a shift in vocabulary meanings used will affect counted ratios of cognate retentions and therewith the centuries computed as elapsed. It is to be expected that Hoijer's use (because of deficiencies in lexical data available to him) of 78 words of Swadesh's first or preferred list and 22 from his second will yield ratios and times somewhat different from those that would have eventuated from using the whole first list of 100. I should expect the retention rates to be somewhat too low, the durations somewhat too long.

As regards the internal Athabascan situation, use of the first or the second standard list will presumably give about equal results of about equal validity. The error in both cases would be only statistical. But with an altered list, Athabascan returns may not be offhand compared with results obtained with the standard list from other stocks.

2. The incomplete lists with which Hoijer had to content himself for three languages prove definitely to affect results in the direction of raising retention ratios and therefore shortening computed periods elapsed. This is empirically evident on comparative inspections of columns of figures, or even of the means of their retention ratios. The expectable cause of the distortion is that it is the less stable and durable words that are also the more likely to be omitted from vocabularies. Thus the reduced sample actually treated by the formula has been preselected in favor of stability, and the retention comes out too high.

The approximate average excess in the retention ratios can be estimated by inspection, and I have ventured to correct Hoijer's fifteen-tribe tabulation and classification accordingly. It gives a better internal congruence for Athabascan

than his uncorrected figures. At least I have little hesitation in predicting that whenever lexical lists of identical size become available, the results will be closer to my arbitrarily corrected tabulation than to Hoijer's uncorrected one.

Of course with a stock having the geographical spread of Athabascan, twenty or twenty-five vocabularies would give a much sounder picture than fifteen. With fuller context for each value, irregularities not only become more visible but are more readily construed as attributable either to a fault of the sample or as due to historic events such as origin from one rather than another subgroup, from alien influences, separated position, or different types of linguistic structure. It is fortunate that Hoijer was able to include even fifteen languages, and it would be unfortunate to have to reduce this number to twelve because of shortages in a few word lists.

3. The classification of the Athabascan languages so far compared conforms so well with their geographic distribution as to leave a strong impression that their separation and spread were on the whole an orderly and rather even-paced process.

The one exception to date is Apachean, whose relative recency of separation and slight degree of diversification since separation suggest that the ancestors of all modern Apacheans may have been a smallish, homogeneous group moving rather rapidly on their migration south. Had their movement been a leisurely or hesitant drift of diverse units, some of them might have been left behind or have experienced accidents of separation or influencing on the way, which would have interfered with their present conformity of degree of relationship to geographical position in the Southwest.

4. The proposition that separation of related populations increases changes in speech whereas continued contact slows change is neither directly borne out nor contradicted by Hoijer's results. But I believe the idea must be maintained in principle as normally having considerable effect.

5. Lees says in his conclusion (1953, p. 127) that "if there are classes of basic-root-morphemes which decay at different rates, the assumption of a temporally constant k [rate of change] for our word list (which presumably contains members of several such classes) is clearly false."

I accept his logic, whether by "decay" he means phonetic wear and tear leading ultimately to nonrecognizability of cognateness, or, a fortiori, if he includes replacement by new stems through semantic shift.

Phonetic decay is not usually a serious factor unless several thousand years have elapsed and only the end products of diversification are known, without intermediate stages and history. Within a range of say three thousand years phonetic decay would presumably produce only a minority fraction of dubious cases: "possible" cognates, "unproved" cognates, and such.

Outright replacement by semantic shift—as English *dog* for *hound*—*canis-kyon*—will normally be the definitely more effective cause of long-range change. This is clear from Hoijer's material, and very clearly so in my Mayan, Uto-Aztecan, and Romance 10-word lists.

6. I am convinced that it is possible to define certain different classes of morphemes exhibiting different rates of change (Lees, 1953 p. 127). I believe also that such differences of rate probably inhere to some extent in those classes of

morphemes that used to be called parts of speech, but that they inhere to a greater extent in groups of words that belong to certain semantic classes, like the body parts that Lees mentions.

This conviction is based on impressions from experience in comparing words with reference to their meaning. It comes out, for instance, in the difference in stability of words for dog as contrasted with body-part terms in the 10-word samples of Athabascan, Mayan, Uto-Aztecan, and Romance discussed above. I infer there is a logic that can validate such differences in replacement rates; what is more useful (though perhaps less titillating) than logic is phenomena, and I hope to return to this subject at another time, with empirical evidence.

7. All this does not mean that the glottochronological hypothesis or method falls and must be abandoned. The method is not a formula that unalterably grinds out dates when a certain series of phenomena has been collected into a hopper. There is probably nothing in human history, culture, or language that works out so simply as that. The glottochronological hypothesis or formula is only an ideal or mean rate. It is always being affected—by the word list chosen, by the intimacy of alien or cognate contacts, probably by the size of the intercommunicating population speaking one language, certainly by geographical position. To think that these factors can be ignored by a finally perfected word list or any such mechanical gadget—that is the fundamental mistake. The sound operational procedure is to recognize that these potentially disturbing factors exist, but therefore to attempt to discover their presence and relative strength in each given situation.

The last results to eventuate with any degree of reliability in these studies are absolute dates, even approximate dates. The ascertainment of relative time depths in stocks of related languages is already fairly dependable; Hoijer's Athabascan contribution is another testimony to that. As between unrelated stocks, dates will of course emerge from computation with equal definiteness, but must be accepted very much more provisionally. It will take a much larger context of findings and consideration of many kinds of pertinent evidence before the tentative interstock dates computed gradually become corrected or confirmed to form a coherent non-conflicting web. On the other hand each new comparative study made with due industry and care will throw additional *statistical* light on the causes, rates, variability, and effects of lexical change. We must accept that progress in attaining sound results will be slow and gradual, but we have every reason to believe that progress will continue and take us far.

There is definite progress already. We can cut some problems fairly fine even now, as I think my dissection of Hoijer's results shows. But we cannot yet cut into some other problems at all, except by guessing. For instance, I do not at all know, any more than Hoijer seems to be sure in his mind, whether his computed times-elapsed can be accepted approximately at face values or may have to be doubled because of a resistability inherent in Athabascan. Yet I am confident that in time this question can and will be answered with essential reliability.

ADDENDUM: HYMES'S AND MILKE'S COMMENT

The foregoing was composed and typewritten in mid-March of 1957. While it lay seasoning, D. H. Hymes published "A Note on Athapaskan Glottochronology"

(1957), which resembles the present examination in also being an analysis and comment on Hoijer's paper.

I agree wholly with Hymes when he says:

Hoijer's original study is the most exact and complete of any so far published for a large family. It gives all calculations... for each pair of languages, all forms for each language, and... which items are found non-cognate for each. This thoroughness makes it possible to evaluate each step, and to discover the effect upon the results of alternative procedures.

Also in agreement is Wilhelm Milke of Soest, Germany, who, in a letter of March 31, 1957, suggests some minor technical corrections and adds that his comments are "possible only because Hoijer, in contrast with other authors, has published his complete material and thereby has exposed himself to criticism." I might add that in my own opinion the Hoijer study, apart from its substantive contribution, seems to promise to become a landmark for this very feature, and that from now on little attention is likely to be paid to glottochronological findings unsupported by the full evidence on which they are based.

Whereas I worked from the total context of Hoijer's results, trying to note any retention rates or ages that seemed geographically or otherwise out of step, in order to find what there was in the material that put them out of step, Hymes has taken the trouble of computing alternative sets of figures. Both of these are based on an assumed retention rate of 86 per cent instead of Hoijer's assumed 81 per cent, but one is based on the same 100 items used by Hoijer whereas the second is based only on those 78 items of Hoijer's which occur in Swadesh's preferred list of 100. Hymes finds that both procedures lengthen the divergence times about 300 years, the first a little more than the second, on the whole. Hymes suspects somewhat greater reliability for the second recalculation. For one thing, it "shows both Hupa and Kato closest to Mattole, next closest to Galice, and least close to Chipewyan," among these five languages, more or less restoring expectability as inferred from geographical position.

Hymes, apart from assumed retention rate, thus compares the yield from a smaller but pure preferred list as against that from a full but somewhat contaminated preferred list, and finds in favor of the former. I compared complete lists of Hoijer's 100-item vocabulary with incomplete lists from the same vocabulary—6, 11, and 18 items short—and found that the short samples yield short divergence times but over-high retention ratios. Our two sets of findings are by specifically different procedures, though within the same area of problem.

Of course there is no connection at all between Hymes's 78-item list and my figure of 79 instead of 100 pairs of words available for comparison for Beaver-Galice and Beaver-Kato. Hymes is comparing the "pure" part of Hoijer's list with the "contaminated" total list actually used by Hoijer; I am comparing the use of Hoijer's incomplete lists together with his complete lists. We both emerge with suggested corrections but on different scores.

Milke points out that Hoijer appears to be conservative in recognizing cognates, especially on the Pacific coast, and that as between Mattole and Kato, items 17, 20, 27, 73, 79, 85 look like cognates, and 78, 83 like possible cognates. Of course no one would assume to teach Hoijer how to deal with Athabaskan, and he may still not reconsider any of these seeming cognates to be cognate. But if they were cognate,

the retention ratio (uncorrected for shortages in the lists) would rise from 70 to 76 or 78 per cent for these two languages, and the time of divergence would fall from 858 to 660 or 597 years—in far better accord with the distance separating them, and leaving the Pacific division much less anomalous in its internal organization. It would of course not do to alter the figures for one pair of languages alone; the case shows how tender and precarious the situation really is. With only 100 or fewer terms dealt with, a handful of words differently adjudged as to cognateness can alter the true finding by centuries even in an obviously close, compact family like Athabascan, and by millennia in the loose macrophyla that we have gradually got into the habit of half-accepting but then dealing with them as if they were authenticated.

9. RECENT ETHNIC SPREADS

HOWEVER MUCH or little linguists vary in accepting or refusing the glottochronological assumption of an essentially constant rate of change of basic lexical items, they agree that close uniformity of speech throughout wide areas must be due to recency of spread. Even if all historical knowledge were blotted out, we could infer that the spread of Spanish, of Portuguese, and of English to the Western Hemisphere, for instance, must have been a matter of but a few centuries on the basis of the limited changes in speech in the Americas from that in the Iberian Peninsula and British Isles.

An instance of the combination of at least relative recency and wide spread is furnished by Polynesian. George Grace, in the *American Anthropologist* for 1955 (pp. 337-339), defines Polynesian in relation to "Eastern Malayo-Polynesian," which is, approximately, the non-Indonesian part of the Malayo-Polynesian or Austronesian stock. He finds that the two-hundred and fifty or more languages of eastern Malayo-Polynesian fall, lexically, into something like nineteen divisions or "groupings." For instance, grouping no. 1 consists of the languages of New Caledonia. Groupings 2-4 occur in the Loyalty Islands; no. 5 covers the little New Hebrides and Banks archipelagos, in geographical northward sequence plus certain extensions; 6, probably Santa Cruz; 7, the southeastern Solomons; and so on to no. 19—every one of the groupings being basically a Melanesian one. Where does Polynesian tie in?

Surprisingly enough, it stems out of a subdivision of a subgroup of Melanesian grouping no. 5. Moreover, Micronesian is probably another subgroup of no. 5. These are Grace's subgroups of no. 5:

- a. Southern New Hebrides.
- b. Middle and northwestern New Hebrides plus extensions which include Polynesian.
- c. Northeastern New Hebrides, Banks, Torres Islands.
- d. Micronesian (probably).

The subgroup *b* subdivides into part of New Hebrides, Rotuma, Fiji, and Polynesian.

In short, the grand or main line of historical linguistic relationship follows the Melanesian chain northward, northwest, and then west to New Guinea. (For the original migration or peopling, it must have been the reverse, from New Guinea to New Caledonia.) About one-fourth way up this arc, in the fifth of nearly twenty steps or stages, as the "groupings" might be construed as being, something happened if Grace's classification is correct. An emigration took place eastward to Fiji and Rotuma; from there a further movement continued into Polynesia and over the whole of it, including ultimate backwashes westward into the Polynesian-speaking outliers.

Here the inferences that can be made from Grace's classification came to an end. But we can continue with Elbert's analysis, by counts of cognates, in "Internal Relationships of Polynesian Languages and Dialects" in *Southwestern Journal of Anthropology* for 1953 (pp. 147-173). This analysis, by a variant of glottochronological figuring, takes us back down the family tree to the first internal

separation of the Polynesian dialects. Since this separation began, the Polynesians have spread over a much larger slice of the Pacific Ocean than the Melanesians ever managed to occupy, although Melanesians formed the parent trunk on which the Polynesians constitute only one overgrown twig.

It is anomalous situations of this type, though of a lesser order of magnitude, and in native North America, that I shall consider here: instances of languages widely spread with little diversity of dialect and hence indicative of a recent spread of their speakers.

ALGONKIN

Algonkin speech seems to consist of four principal branches: A, Central-Eastern Algonkin, covering nine-tenths or more of the total Algonkin area, nearly all in woodland; B, Blackfoot, at the foot of the Rocky Mountains to the north, mostly north of parallel 49°; C, Arapaho, also at the foot of the Rockies, but farther south, from Montana to Colorado; and D, Cheyenne, in the Great Plains, though east of the foot of the Rockies. There were several Blackfoot, Arapaho, and perhaps Cheyenne (Sutaio) dialects, but not very differentiated. Central-Eastern Algonkin was spoken by a long series of tribes or nations, stretching from Hudson Bay to Virginia, and from Illinois to Labrador. Some of these Algonkin populations were differentiated by full languages; but considering the vast extent of territory covered by them—more than a million square miles—it is the similarity of their intergrading speech that is more significant than its local variations.

We must conclude from the relative uniformity of speech that this central-eastern block of Algonkins had radiated out over their vast area in a comparatively short time, perhaps within one to two thousand years.

This inference bears on the "Woodland" cultures determined by archaeologists in the eastern United States and Canada and credited with a continuing antiquity of several thousand years. I do not contest the antiquity, some of which rests on carbon-14 determinations. But I would be chary of assuming that because the historic eastern Algonkins lived in woodland, their Woodland culture predecessors in the same central-eastern area three to five thousand years earlier already spoke Algonkin. We have no evidence of what they spoke, except it is likely not to have been ancestral central-eastern Algonkin in the main, else this would have diverged, by the proto-historic period, into far more differentiated languages than were encountered in the woodland area.

TETON WITHIN DAKOTA

During the historic period, the Sioux or Dakota have been the most widespread and populous group within the Siouan family. They speak essentially one language, differentiated in three larger dialects, characterized by a shift of *d* to *n* to *l* (Dakota, Nakota, Lakota) from east to west. The Teton^a or Teton were considered one of seven original Dakota peoples; but spreading southwestward across the Missouri from about 1750 on, the Teton became subdivided into seven tribes of their own, of whom the Oglala were probably the most peripheral, aggressive, and successful, and remain the most populous. Here we have a rather rapid spread corresponding to but slight subdialectic differentiation.

In a somewhat similar northwestward spread, the Assiniboine were the forefront of the *n*-dialect Dakota.

Indirect white influence, operating through more easterly tribes which obtained firearms earlier than the Dakota, initiated the westward drift, and the acquisition of horses further facilitated the permanent move into bison country. But these causes of territorial spread in no way detract from its being accompanied by a minimum of speech change in the short time that the causes were operative.

NAVAHO WITHIN APACHEAN

Another instance is furnished by the Apache, including the Navaho, who, Hoijer's recent glottochronological study, just discussed, suggests, had separated from their far northern Athabascan speech kinsmen within about 600 years and from one another within 400. Originally only one of a half-dozen closely related Apachean groups, the Navaho began to prosper modestly from contacts with their alien and more advanced neighbors in the Southwest while the Chiricahua, Mescalero, Jicarilla, and Lipan Apache were successfully terrorizing and exploiting Mexicans, Pueblos, and Americans and fighting Plains tribes like the Comanche. But the Apache wore themselves out, kept their numbers down, and ended by being confined on reservations, whereas the Navaho multiplied and overflowed other reservations and public lands. Their increase was even more marked than their territorial expansion. From constituting perhaps a sixth of the Apache nationality, they have become about six times as populous as all the other Apachean tribes together.

This extension has occurred within the past two centuries and in the half-light of frontier history. But were all historical knowledge obliterated, the spread and increase would yet be evident in the large part of New Mexico and Arizona occupied by the Navaho with but slight variation of customs and less of speech.

CHEMEHUEVI AND KAWAIIISU WITHIN SOUTHERN PAIUTE

Chemehuevi.—Similar to the recent expansion of the Navaho out of the Apache and their consolidation into a separate ethnic unit, is that of the Chemehuevi out of the Southern Paiute, who were sometimes called True Paiute to distinguish them from the Northern Paiute or Paviotso. Linguistically and in origin the Chemehuevi are the southwesternmost of a series of Southern Paiute local groups speaking very similar and intergrading dialects, stretching eastward mostly north of the Colorado River through southern Nevada, northern Arizona, and Utah, to merge—again only by a minor speech distinction—into the Ute. These Ute might fairly be described as “original Paiute” who had been exposed to Plains, Apache, Pueblo, and Spanish contacts from which the remoter Paiute had been largely exempt. Especially were the Ute exposed to influences and attacks from the Plains tribes across the Rockies after the life of these effloresced, once they got horses and guns.

At the opposite end of their range the Southern Paiute similarly acquired a name, an appearance of social identity, and some new cultural coloring by contact with the Yuman tribes, especially the northernmost of these, the Mohave, whose position protected them longest from disintegration by Spanish and American contacts. Chemehuevi is simply the Mohave name for any and all Southern Paiute

of whom the Mohave had direct knowledge; they communicated this name to other Yumans and to Spaniards and Americans for those Paiute with whom they had most frequent contacts of travel, trade, and food exchange. These Chemehuevi finally accepted the name for themselves; their congeners beyond, around Las Vegas and north and east of it, remained Payuches or Paiutes.

In 1776, Garcés found Chemehuevi well south of where Las Vegas now is, in the desert west of Mohave Valley on the Colorado River, in and around the Providence Mountains. He found them again—in fact, a few days earlier—farther south, in the desert off the river, behind the Monument or Whipple Mountains. South of these mountains, the river was then held by the Halchidhoma, an agricultural Yuman tribe generally at war with their northern and southern Yuman kinsmen and fellow farmers—the Mohave and the Yuma proper. As friends or dependent clients of the more congregated and war-proud Mohave, these Chemehuevi may have remained reluctant to occupy even the desert ranges immediately west and northwest of the riverine Halchidhoma.

But some fifty years after Garcés, Halchidhoma fortunes had declined, and they were finally expelled from the Colorado by the Mohave and Yuma in alliance. The victors claimed the whole stretch of intervening valley as their conquest and more or less occupied it, but somewhat fitfully and by families and groups rather than by any budding off of bodies of colonists ready to emigrate permanently from their ancestral homelands.

The intermittent vacuum thus left along the Colorado was the opportunity the Chemehuevi in the desert mountains to the west had been waiting for, and they slipped into the overflow lands along the river and began to plant in a series of scattered stretches, from Chemehuevi Valley downriver nearly to Blythe. They had known about farming in a pitiful sort of way where they came from around Las Vegas, in patches watered by springs. But south of the Providence Mountains the desert gets drier and drier, and they must have sat and looked hungrily eastward into the bottomlands. One account has them first venturing to the river to gather the wild mesquite which was abundant in Chemehuevi Valley near the river. But the Mohave and in the south the Yuma had more land than they could conveniently farm, and they were tolerant, and the Chemehuevi drifted in. When the American military expeditions of Whipple and Ives came up the river from Williams River and Fort Yuma to Mohave Valley in 1854 and 1858, they found the Chemehuevi established in two river stretches and eager to trade farm products. Of these two reaches, one lay between the present Blythe and Parker, the other in Chemehuevi Valley. All the Chemehuevi settlements were still on the right or west bank of the Colorado.

A few years later, the Mohave having meanwhile ventured unsuccessfully to match their strength against U. S. troops, hostilities between them and the Chemehuevi broke out, in which the latter were aided by "Paiute" kinsmen from the north. Perhaps the Mohave, now that they were no longer top dog in their little world, had begun to regret their former generosity or indifference in letting the thrifty desert rats occupy good farm land. Perhaps the Chemehuevi, now that the warlike Mohave had been publicly defeated, felt free to resent their arrogance and patronizing. At any rate, about 1865 there was a killing or two by each side, and

war was on, lasting till 1867. The Chemehuevi held their own; in mountain terrain they were definitely the more expert; losses seem to have been about equal; and though fights and killings dragged on until the reservation agent at Parker induced a peace, the Chemehuevi felt victorious. At any rate, the unratified "treaty" gave them freedom to live on the west side of the Colorado and to share in the reservation. They had by this time spread south as far as Palo Verde slough.

Their farm lands and reservation allotments held them more and more to the river valley. The back desert out of which they had come was remembered but abandoned, and the people on the Colorado River reservation who today call themselves Chemehuevi are the descendants not only of the Chemehuevi whom Garcés encountered west of the Mohave but of "Paiutes" well to the north. Charleston Peak, in Nevada northwest of Las Vegas, is still remembered as their sacred mountain, corresponding to Avikwame or Newberry Peak on the Colorado in Mohave mythology and consciousness.

Chemehuevi speech is said to differ dialectically from that of the Las Vegas Paiute. It contains a few adoptions from Mohave: names of new food plants, of several places, and such. But we have no vocabularies accurate enough to tell us whether or not there are other differences beyond some intonation habits or an occasional local choice of a variant stem.

Kawaiisu.—The history of this smallish group is the inverse of that of the Chemehuevi, in that they separated earlier but expanded less. When they reached their small promised land of upland valleys between the Sierra Nevada and the Tehachapi Mountains, they concentrated upon it, and the desert whence they had come became a residual and secondary hinterland.

As far back as 1925 I published an outline map (no. 52, p. 579, in the *Handbook of Indians of California*) of the distribution of Shoshonean, which showed seven of the eight primary divisions of that stock—all but Hopi—represented in aboriginal California. Four of the seven are confined to California. These four are Luiseño-Cahuilla, Gabrieleño, and Serrano in southern California, and Tübatulá on Kern River.

More remarkable, however, is the fact that three other divisions, mainly characteristic of wide ranges in the Intermontane plateau, also occur in California in very much smaller territories than in the plateau. Of these three divisions, one is Ute-Chemehuevi, which is represented in modern California by Kawaiisu, Chemehuevi, and Las Vegas Southern Paiute. A second, Shoshone-Comanche, is represented by the Panamint or Koso Shoshone. The third, Mono-Paviotso, is represented in California by the Mono or Monachi on the western slope of the southern Sierra Nevada, by the Owens Valley "Paiute" east of the Mono, and by a fringe of Northern Paiute in eastern California north of the Washo and south of them around Mono Lake.

Within Ute-Chemehuevi, Kawaiisu, the most southwesterly dialect, with a territory also unique in extending out of the Great Basin into the Great Central Valley of California, seems slightly but definitely set apart from all other dialects in the division. That is, Chemehuevi, Southern Paiute, and Ute together form a much larger unit whose variations seem to intergrade continuously even when they are reasonably marked, whereas Kawaiisu does not participate to the same degree in continuity with its nearer Southern Paiute neighbors.

The linguistic evidence is necessarily fine-cut and technical and has therefore been assembled for separate publication, together with some revisions of the geographical frontiers of dialects due especially to the investigations of Julian Steward.

The larger pertinence of this degree of separateness is the inference that Kawaiisu did not separate from Las Vegas Southern Paiute with any such recency as has just been sketched for the Chemehuevi. It branched off somewhat longer ago, and since then its speakers have evidently, to judge from the position of their historic territory, had more contacts with non-Ute-Chemehuevians than with their more immediate Ute-Chemehuevi kinsmen. The period of their preponderant separateness might be estimated as two or three times as long as that of the Chemehuevi from the Las Vegas Paiute, say roughly about 500 years, plus or minus.

We do not have accurate enough information to decide how far Kawaiisu speech may have been influenced away from Ute-Chemehuevi by its non-Ute-Chemehuevi contacts, or on the other hand may have retained traces of a former condition when Ute-Chemehuevi as a whole was still less differentiated from the other Plateau divisions—Shoshone and Mono-Paviotso.

NORTHERN PAIUTE AND MONO

The Mono-Paviotso or Mono-Northern Paiute division of the Plateau branch of Uto-Aztecan resembles the Ute-Chemehuevi division in its distribution in that a small minority of its territory lies in California, the majority east thereof (north-east in the present case, to be exact) in Nevada, Oregon, and Idaho. The parallel extends even further in that in both cases a fraction of the minority area lies with the Great Central Valley of California. Part of the Kawaiisu holdings when the white man came lay across the Tehachapi divide in San Joaquin Valley drainage; part of the holdings of the Mono was at the headwaters of the Kaweah, Kings, and San Joaquin rivers in the same valley. (The Shoshone-Comanche division also held territory in California, as already noted, but only to the eastern foot of the Sierra Nevada, not across it into San Joaquin drainage.)

Even the scraggy data available in 1907 showed clearly the approximate range of these two languages, or groups of dialects, Mono-Paviotso and Ute-Chemehuevi, and that they were geographically separated by dialects akin to Shoshone proper. It was also known that there were Southern Paiute and Northern Paiute, and that these were distinct in both habitat and speech. It was the desire of preventing a recon founding of this Paiute diversity that led me, in coining designations for the Plateau Shoshonean divisions, to make up the cumbersome designations Ute-Chemehuevi and Mono-Paviotso, which by-passed the ambiguous "Paiute."

But the available data were too diverse in source and too coarse in transcription for me to venture going beyond the discrimination of the three primary speeches ("genera" or "divisions") in the Plateau-Basin area. Sapir, not long after, gave us a firm touchstone for Ute-Chemehuevi in his Southern Paiute grammar and dictionary. Mono-Paviotso, however, had to wait until recently when Dr. Sydney Lamb, of the Linguistics Department of the University of California, undertook its definitive study under the Linguistic Survey of California established by

Murray Emeneau. For background, he surveyed the entire division, then focused his study on the Mono for which he has secured both grammar and lexicon. The statements that follow are based on his knowledge and are made with his consent.

Northern Paiute.—The Mono-Paviotso (or “Monoish”) division of the plateau branch of Shoshonean consists of two languages: first, Mono or Monache; and second, Paviotso, as the Shoshone and Owens Valley Paiute called the group, or Northern Paiute as the ethnographers renamed its speakers in extrication from the confused and confusing “Paiute” which American travelers and officials had bestowed on the group and language.

The boundary between the two languages, Mono and Northern Paiute, is the watershed divide between the headwaters of Owens River on the south and Mono Lake basin to the north. C. Hart Merriam had long recognized the distinction and the frontier, though apparently his statement thereof appeared in print only posthumously in his 1955 *Studies of California Indians* (see pp. 151, 166). I had never secured a Mono Basin vocabulary, in fact have not yet met any identified Mono Lake or Mono County Indians. Dr. Lamb, however, confirms Merriam, on the basis of his field studies made in the summers of 1953 and 1954.

I will return to the Mono in a moment for more close-up review; but the outstanding fact about Northern Paiute speech is the enormousness of the area throughout which it was essentially homogeneous: all northwest Nevada, a fringe of easterly California, southeastern Oregon, and some holdings in Idaho where at least the “Bannock” spoke Northern Paiute, though they seem to have been situated among Shoshone and prevalently associated with them.

The Northern Paiute speech homogeneity throughout this large area, according to Dr. Lamb as well as other observers in the field, is of the type of spreads that this essay deals with—that is, it must be relatively recent. It is true that much of the country is arid and of low productivity, so that several thousand people would need a hundred thousand square miles to subsist on. But if they overran and occupied these in the last thousand, perhaps in the past five hundred years, whom did they displace? And where did these predecessors go? Where are they now? Absorbed in part? I have no clues to offer, but those who know the historic tribes of the northerly Intermountain area and larger Columbia drainage may have suggestions.

Mono.—According to Lamb, Mono or Monache occurs in three “superdialects”: Northwestern, Northeastern, and Southern. The two first are separated by the Sierra Nevada crest; the third extends across it.

Based on sound developments, each of these superdialects contains two or three dialects, and these may again comprise subdialects. West of the Sierra, the dialects may coincide with named tribelets (cited below in parentheses), as they occur also among the Yokuts of the foothills and valley.

The dialects of the Northwestern Mono are those of the San Joaquin and of Kings River. In the former, the speech of Auberry and that of North Fork are subdialectically different; in the latter, that of Sycamore Valley and of Dunlap (the Woponoch) differ. San Joaquin was more like Northeastern across the crest.

Northeastern Mono contains three dialects. The first is on northern Owens River including Long Valley (with a subdialect around Benton); the second along and

abreast middle Owens River, at Round Valley, Bishop, and Laws; the third off the river to the east, across the White Mountains, in Deep Springs and Fish Lake valleys, bordering on the Shoshone. This last seemed to Dr. Lamb to be the most archaic Mono and to show certain special resemblances to southern Owens River.

The Southern Mono dialects are those of southern or lower Owens River drainage and of the upper Kaweah River west of the Sierra crest. There were three southern Owens subdialects: at Big Pine and Fish Springs; at Independence; and at George's Creek, Lone Pine, and Owens Lake. Most of these localities are conveniently shown on Steward's maps. Kaweah River Mono was subdialectically diverse in the stretch from Lemon Cove to Three Rivers on the main river (Patwisha, extinct), and at Eshom Valley near Badger (Waksachi).

We have in this entire Mono array an example of typical California speech fractionation; there are recognizable subdialects even along the same stream, as compared with the much larger areas of speech identity among the Northern Paiute of the Intermontane semidesert plateau. Identity of speech could apparently be maintained among only a few hundred people, and in the desert these had to space out over a much larger territory to subsist.

Inferences as to spread of the Mono.—With all their relative compactness and conformity to topography, the Mono dialects do allow certain inferences as to the history of Mono populations. Above all, the Mono in the San Joaquin Valley and those east of the Sierra cannot have been separated very many centuries, because they differ no more than superdialectally in the north and merely dialectically in the south. Although no quantitative standard attaches to the words dialect and superdialect, it might be a reasonable guess that the Kings River Mono had crossed the Sierra to settle perhaps five hundred years ago, those of the San Joaquin a little less, those on the Kaweah between two and three hundred.

We need have no hesitation in assuming for several reasons that the expansion was from east to west, and not the reverse.

First, there is more water, food, and equable weather in the San Joaquin Valley. People would not voluntarily leave it for the desert.

The seemingly most archaic Mono is that east of middle Owens River at Deep Springs and Fish Lake.

The center of gravity of the whole "Monoish" or Mono-Paviotso division obviously lay east of the Sierra. It would require a farfetched, complicated hypothesis to account, first, for its originating in the San Joaquin Valley, then throwing off "Paviotso" who all drifted to the north, and after that, the budding off of the Owens Valley region Mono who went east. Much the simplest explanation is to have the larger Mono-Paviotso group forming somewhere in the general area of the Owens Valley and Mono Lake basins, beginning to differentiate, and then spreading westward and northward.

If the Mono entered the San Joaquin Valley drainage from across the Sierra, their new occupancy must have been at the expense of previous Yokuts holdings. I will return to this point in the next section.

The close similarity of the Southern Mono spoken on the Kaweah to that on the lower Owens River indicates a correction of the map of native California as of 1770. The drainage of the upper Kern River and the South Fork of Kern River

has customarily been assigned to the Tübatulabál whose settlements centered around the confluence of the two streams. I probably am responsible for first making the assignment, on the general ground of California ethnic boundaries tending to follow watersheds; there could be no year-round occupancy of the upper canyon of either Kern. How much the Tübatulabál and the Mono hunted or gathered seasonally in this mountain region, we do not know; the Owens Mono would have crossed it to reach the Kaweah and then traversed it to maintain connections with their kinsmen, for it seems likely from the closeness of their speech that they did maintain them. More likely than not, accordingly, the "tribal" map should be changed to give the upper Kern drainage to the Southern Mono instead of to the Tübatulabál.

Nomenclature.—Some remarks are also necessary as to the names Mono and Paiute in use for ethnic groups.

Merriam in his posthumous 1955 *Studies* called the Mono Lake Indians "Mono Paiute" and says that that is what they call themselves (p. 72)—no doubt in speaking English and meaning that they are "Paiute" living in Mono County. On page 158, in listing "Northern Piute" tribes, he gives Tu-ne-ga-bah as a "name sometimes applied to themselves by the Mono L. Piute." Merriam consistently calls the Northern Paiute or Paviotso (including the Mono Lake Indians) "Piute." All the groups that I labeled "Mono" in the *Handbook* and that Lamb recognizes as Mono, Merriam consistently calls Monache. The term Mono he considers unfortunate and confusing and recommends that it be dropped (p. 166). In principle, I concur completely with Merriam as to Mono (other than as a place name), though it may be too late to rid ourselves of the term.

There are two completely unambiguous terms available to denote the two ethnolinguistic groups we are dealing with: Monache (or more precisely Monachi) for the southern division, Paviotso for the northern. True, they are foreign terms—Monachi a Yokuts designation, Paviotso a Shoshone one—but foreigners' names have the frequent advantage of being both sufficiently inclusive and sufficiently specific, where native names tend to be either dialectic variants of the noun for "people" or to denote the inhabitants of particular local settlements. However, Paiute (with or without the qualification of Northern) and Mono have become so widely established in popular, official, and scholarly usage, that an attempt to replace them now might only increase confusion.

The real trouble is that there are two similar words which have been confounded in Monache-Mono: one Indian, the other probably Spanish. All the Yokuts called their west-side-of-Sierra Uto-Aztecan neighbors Monay, Monoy, or Monachi (-chi being a suffix for groups or tribelets, like Yaudanchi, Wimilchi, Tachi, and so on); with them they included also their Owens Valley cousins. The same stem recurs in Southern Sierra Miwok designation Monah and Central Sierra Miwok Monahk or Monok for the Mono Lake Paiute (Merriam, p. 158); and Powers (p. 320) says that the Nisinan Maidu called the Paiute Moan'-au-zi, which I transliterated as Monozi in the *Handbook* (p. 582). The term was accordingly used by three California stocks for their Shoshonean neighbors to the east, though the Yokuts applied it to peoples who by Lamb's classification spoke Mono—but the Miwok and Maidu to Northern Paiutes. I recorded a Yokuts folk etymology connecting the name with the word for "flies"; but its real meaning and actual derivation are unknown.

There is also a Spanish word *mono*, which as a noun means "monkey," and by extension "puppet," "image" (such as were used in the Mourning Commemoration), and as an adjective, "neat," "pretty." I believe that by the time Americans took over California, the Spanish and the native word had begun to be confounded. Certainly *monay/monoy* is close enough to *mono*. The Spanish form, with occasional variant *Mona*, was the first to appear in print in English, according to Merriam's elaborate compilation (pp. 167-173), being used in 1851 by Savage for the Mono Lake Indians, by Beale and von Schmidt and Henley in 1856 and 1857 for the Owens Valley people, and by McKee-Barbour-Wozencraft and by Johnston in 1851 for the west slope Shoshoneans. At any rate these earliest forms all might be from the Spanish, directly or indirectly.

The indubitably Indian term *Monachi*, with its Yokuts suffix, appears in print in English ten years later in 1861, when Hutchings applied it to the west slope Shoshoneans, though misspelled *Ho-na-che* (a typographical error that died hard, according to Merriam: Lester, 1873; Bunnell, 1880; Galen Clark, 1904). W. H. Knight spelled it *Monatchee* in 1864; Powers, *Manachees*, in 1877, also for the west slope Shoshoneans. It got more frequent use for the Owens Valley Indians: Knight, 1863, *Monatchee* ("in Tulare Valley"); Daley (1865), 1867, *Monache*; MacIntosh and Miller, 1870, *Monache*; Whiting, 1872, *Monacha*; Belknap, 1876, *Monache*; the Commissioner of Indian Affairs, 1876 and following, *Monache*. However, *Mono* and *Mona* continued to be used also, for both east and west side Shoshonean.

This was the situation that confronted me in 1917 when I completed the main text and maps of the *Handbook*. I realized from my own vocabularies that all the Shoshoneans east of the Yokuts, whether on the west or east slope of the Sierra, spoke variants of one language. I might have named them Western and Eastern *Monache*, and in retrospect it is clear that that would have been the more fortunate choice. I chose Western and Eastern *Mono* because in Mono Lake and Mono County permanent geographic terms had been established, and because I happened to be without any data on the dialect of Mono Lake itself and assumed from the name that it would classify with the *Mono-Monache* speech of Owens Valley, rather than with Northern Paiute; however, Merriam and Lamb, who were on the spot, found it did correspond with Northern Paiute.

Well, so we got Western and Eastern *Mono* on the maps and classifications, when along comes Julian Steward in 1933, with an *Ethnography of the Owens Valley Paiute* in volume 33 of this series. He insisted that neither the Indians nor the whites of Owens Valley knew or would acknowledge the term *Mono*, that the only name in local use was *Paiute*, and that the only way to identify them was as Owens Valley *Paiute*. I was editor but let author's right prevail, with the result (1) that Gayton has since published largely on the "Western *Mono*," (2) that there no longer are any "Eastern *Mono*," (3) that Steward recognizes the truncated Western *Mono* (1938, map 1—but not in the index), and (4) that he has got the name *Paiute* authoritatively affixed to the Owens Valley people who speak not *Paiute* but *Mono-Monache*, according to the one professional linguist who has examined the language. It would be interesting to come back in fifty years and see how this little scholarly tangle of names has been resolved.

Alleged cross-ties of Mono.—Merriam, who spelled foreign languages by Webster's dictionary and therefore was not a linguist, says (1955, p. 169) that Monache seems to have somewhat closer relationship with Panamint Shoshone than with Northern Paiute, that it shares some words with Chemehuevi, and that its "ancestry . . . dates back to a period antecedent to the complete differentiation of the surrounding tribes"—that is, it forms a connecting link among the three Plateau divisions of Uto-Aztecan.

I have no material exact enough to test this interpretation. Steward gives only one Mono-Monache vocabulary, from Georges Creek in Owens Valley, and only two of Northern Paiute, both from a distance—Mill City in northern Nevada and Bannock in Idaho. The Georges Creek list does show a few resemblances which are closer to the nearer Shoshone dialects (Little Lake, Panamint, Lida): mountain, bear, good. But on the whole, it agrees with Northern Paiute far more often, though it clearly is well differentiated from it: a distinct language, in short, though within the same division or grouping. Merriam has probably noted special cases of similarity, quite possibly including some instances of borrowing by Mono-Monache from Shoshone. There is nothing to indicate that the accepted classification of the place of Mono with Northern Paiute needs revision. That is where we shall have to leave the point until Lamb or some other Uto-Aztecanist can settle the matter with material of his own collecting.

YOKUTS

We have seen that the Monache occupancy of the middle-altitude western slope of the Sierra Nevada probably is rather recent—a matter of some centuries. In the foothills below them, and across the entire floor of the upper San Joaquin Valley, there were only Yokuts at the beginning of historic record. It thus seems almost certain that it was Yokuts whom the Monache dispossessed when they settled west of the crest. Did they crowd former mountain Yokuts tribelets down into foothill habitat, former foothill groups into the valley itself, and perhaps start or accelerate valley tribelets into a push northwestward down the valley? It seems that all these things happened in some degree.

My Yokuts grammar of 1907 has been superseded by that of Stanley Newman of 1944, which is better founded in recognition of sounds and carried further in exact presentation of morphological pattern. However, working later, Newman was able to secure information on only six dialects, whereas I got vocabularies from twenty-one, collected with more standardized lexical coverage. Consequently I was in position to make a classification of Yokuts generally, for more than half of its historic area. From the northern third or so of Yokuts habitat, which here lay wholly in the open valley (the Miwok holding the foothills and higher reaches of the Sierra behind them), the entire population had been drained off to the Franciscan missions during their later period of activity. Parts of some of these northernmost Yokuts tribelets returned after secularization of the missions in 1834, but in remnants, often commingled, and not always to their precise former habitats. After 1906, when I made a survey of the Yokuts dialects and territory, I was able to secure only one Yokuts vocabulary from this northern third of the valley floor—from the Chulamni (Alphonse Pinart's Cholvone), attributed to

the latitude and vicinity of Stockton. This proved to be definitely close to the dialects of the central third of the valley floor, especially so to Chauchila of the Chowchilla River, the northernmost of twenty-one dialects from which vocabularies were secured in 1906. With these twenty-one, about twenty more former named and neighboring dialects could be grouped or associated by means of statements made by members of surviving tribelets as to degree of similarity.

There exist various miscellaneous data on the most northerly and now extinct Yokuts dialects, word lists and such, recorded at times between 1800 and 1900, which show perceptible deviations from Chulamni-Chauchila. But these data differ in provenience, in range, in orthography, in accuracy, and therefore in reliable comparability; most of them cannot be associated with a specific locality, or if so, we do not know whether it was their locus already in pre-Mission or only in post-Mission times. As against this tangled skein of data, the clean-cut Chulamni-Chauchila resemblance enables us to affirm that though there may also have been certain more deviant dialects of Yokuts spoken in the northern third of the valley floor, in the main the Yokuts of this area is construable as an extension of the Yokuts of the central third.

The classification of the Yokuts dialects is gone into in detail in my monograph *The Yokuts Language* (1907, pp. 308-346). There are a family tree and diagrams on pages 314-315, percentaged figures on 326, and a table of outstanding phonetic and structural differences on 346; so the findings will only be summarized here. I recognized six groups of Yokuts dialects: Buena Vista Lake, Poso Creek, Tule-Kaweah, Kings River, Southern Valley, Central Valley (this was called simply "Northern" in 1907). Except perhaps the two last, these six groups intergrade quite continuously in the sequence mentioned, which is in the main a geographical order. Thus, the percentages of cognate stems shared by Central Valley with the rest are: with Southern Valley 69, Kings 59, Tule-Kaweah 28, Poso 34, Buena Vista 23. The other way around, from the point of view of Buena Vista and basing on a larger selection of lexicon, the cognate percentages run: Poso 29, Tule-Kaweah 30, Kings 21, Southern Valley 15, Central Valley 10.

I further classified these six Yokuts dialect groups into two "divisions" (p. 310), named Foothill and Valley, according to their prevalent habitat. Buena Vista, Poso, Tule-Kaweah, and Kings constituted the Foothill linguistic division, Southern and Central Valley the Valley division. Twenty-two tribes were listed in the first, sixteen in the second, north to the Chowchilla River; there may have been ten or a dozen more Valley tribes north to the Chulamni at Stockton, making around fifty tribelet dialects all told. But the Foothill dialects occupied much less area. Our located vocabularies of Foothill type cover barely a third of the Yokuts territory north to the Chowchilla River; perhaps less than a quarter of the supposed total area held north to Stockton. Evidently the hills separating the rushing rivers and their tributaries higher up were able to sustain more population per square mile than the open plains separating the more sluggish courses of the same streams in the level valley.

Of course a primary *linguistic* division set up on the basis of hill and plains habitat contains an irrelevant criterion which may be justified as pragmatically, convenient, but it might expectably not conform with entire strictness to the topo-

graphical nomenclature. This is the actual case. The three tribelets of the Buena Vista Lake dialect group lived in the southernmost head of the flat San Joaquin Valley. They did own some back hill land, but it was the inner face of the Coast Ranges, not part of the Sierra Nevada, as elsewhere.

Conversely, dialects of Central Valley type extended along the San Joaquin River, where this still flows *southwestward* through the valley, up into the foothills, almost to the North Fork where the Monache began. In this region, plain and hills merge into each other quite gradually as in the Miwok country to the north, whereas to the south, among the other Yokuts, the passage from valley to foothills is usually abrupt. In the *Handbook* in 1925, where topography had to be emphasized but finer linguistic evidence or discussion was inadmissible, I therefore added a seventh dialect group by separating off five San Joaquin River hill tribes from the San Joaquin River plains tribes (p. 481, maps, pls. 1, 47), naming them the Northern Foothill group. This new group consisted of the Toltichi, Kechayi, Dumna, Dalinchi, and Chukchansi (the last actually on a near-by affluent of Fresno River). It would have been more specifically descriptive to call them the San Joaquin River group of Foothill Yokuts. No material violence was done to the older classification, because the new group represented only a further subdivision and because Yokuts dialect differentiation mainly varies along a continuous gradient.

So far the necessary linguistic facts in their relevant setting. Now for their bearing on movements of speakers!

The tabulation in *The Yokuts Language* (p. 346), shows an array of both phonemes and grammatical morphemes which run with consistency through all Valley division dialects, whereas the Foothill division dialects are much more variable. Sometimes all the Foothill groups have the same form as the Valley. Sometimes all Foothill groups have or lack a sound or form which Valley lacks or has. Sometimes the four Foothill groups differ among themselves, some (usually Kings and Kaweah) agreeing with Valley. There can be no reasonable doubt that there exists a primary dichotomy of Yokuts and that one moiety, the Valley division, is conspicuously the more homogeneous structurally and in the use of cognate stems, and the other, or Foothill, is more diversified internally.

Now the Foothill division (1) occupies, as said, much the smaller area; (2) this area is restricted to the southern half of total Yokuts historic extent; (3) the Foothill Yokuts group diverges progressively more from Valley the farther south its tribelets were situated—where the division ends on the San Joaquin River, hill and plains dialects are so much alike that they can be classified about equally well into one or two dialect groups (as in fact I did in 1907 and 1925); but (4) the most southerly and most divergent Foothill group, Buena Vista, was not situated *to the east* of the plains as the rest were but lived *in* the plains at their *southwestern* edge.

From these facts several inferences derive.

1. The large Valley type spread is the more recent; the smaller Foothill distribution is older.

2. With the distribution of the Foothill dialect older and southerly, the spread of the Valley dialects is almost certain to have been, in the main, northward. This

would not prevent southward shifts of occasional Valley tribelets having occurred also. Such were the Yawelmani, who were almost surrounded by two Foothill groups, three different kinds of Shoshoneans, and Chumash. But all in all the most recently occupied Yokuts territory is likely to have been the most northerly.

3. The populations that expanding Yokuts would have displaced in the north are, to judge by the historic-period map, Costano or Miwok or both. These two stocks are recognized as having formed a closer unit within California Penutian, but in 1770, according to our maps, they were not in immediate contact with each other except across San Francisco Bay and perhaps in the delta region of the Sacramento. Yokuts pressuring northward down the San Joaquin Valley could have pried adjoining Costano and Miwok apart. The accepted Yokuts-Miwok boundary along the valley-foothills boundary from Fresno River north is somewhat anomalous. The more southern Yokuts, the southern and the northern Maidu, all occupy both valley and hills to the east; the Sierra Miwok, only the hills. Finally, from the Cosumnes to the Mokelumne and perhaps Calaveras, there were true Plains Miwok—in the delta, in the level valley, and in the lowest hills; but these Plains Miwok begin where the Yokuts end in the valley.

It seems as if there might formerly have been Valley Miwok and Foothill Miwok, as there were Valley and Foothill Maidu and Yokuts, but that the coming of the Valley Yokuts pushed the Valley Miwok northward and compressed them into the Plains Miwok of the Cosumnes and Mokelumne, whereas the former Foothill Miwok became what we are accustomed to calling the Sierra Miwok.

The Saclan language of Arroyo de la Cuesta which Madison Beeler recently identified as being not Costano as I had supposed, but a hitherto unknown Miwok language most similar to Northern Sierra and next most to Plains, would fit in as that of another displaced or affected group. The Saclan historic habitat has not yet been determined with certainty. Perhaps before the Yokuts expansion the Saclan were "geographical Plains Miwok" living where our historic Plains Miwok lived in historic times along the lower Cosumnes and Mokelumne, and the Plains Miwok dialect as we know it was then spoken on the valley floor along the Calaveras, Stanislaus, and Tuolumne. Or if the name Saclan proves to be the origin of the Spanish "Acalanes" grant on Walnut Creek, this tribelet would then have been displaced westward to become an enclave among the Costano, as other Miwok units were displaced eastward and northward.

Even the Lake and Coast Miwok, on Clear Lake and the Marin coast and bay, may have gained their separated historic territories as part of the displacement of San Joaquin Valley Miwok by encroaching Yokuts. Lake and Coast Miwok have been classified as definitely more similar to modern Plains than to modern Sierra Miwok.

Accordingly much in the native speech maps for 1770 fits, or can be reconciled with, a relatively recent spread of the Yokuts northward along the floor of the San Joaquin. Indeed, a few features become more intelligible through the assumption.

I am aware that a famous and important paper by C. Hart Merriam on the "Mewan" stock, in the *American Anthropologist* for 1907, allows to ten tribes of the Plains Miwok considerable territory around the delta and lower course of the San Joaquin which I have blanketed as Yokuts in the *Handbook* and other

maps. If I seem here still to ignore Merriam's attributions, it is not that I disbelieve his conclusions as against my sparser findings in the field, but that I am now convinced that no one making inquiries between 1895 and 1915 could have ascertained from the surviving northern San Joaquin Valley Indians their pre-Mission tribal habitats with any reliability. The locations they gave Merriam and me were surely post-Secularization. Even these habitats antedated our inquiries by 60 or 80 years, and they had been again mostly swept away by the tide of American occupation 15 to 25 years after 1834. No doubt the missionized survivors of some tribelets and rancherias tried to return to their former homes; many could not or would not, and we do not know which; nor, for that matter, did it occur to us around the century's turn to inquire into the point. I believe that the original location of their settlements, and frequently the determination of their speech, can be had for the lower San Joaquin Valley region as for all the Costano only from analysis of preserved mission period documents. Of Merriam's ten "tribes," eight may well have been Plains Miwok as he affirms. But for two I have vocabularies which are indubitable Valley Yokuts: the Tulamni (Chulamne or Cholovon) and the Yachikamni or Yatchatchumne.

4. The Monache pressure on the Yokuts formerly in the Sierra Nevada, which had its presumable repercussions in the just discussed population shifts in the lower San Joaquin Valley, evidently began in the north where Northwestern and Northeastern Monache differ more, as "superdialects," than the Kaweah and Lower Owens River dialects of Southern Mono differ from each other. Still farther south the Tule River and Poso Creek Yokuts were altogether protected from Monache pressure by the intervening Tübatulabál of Kern River.

As between the upper San Joaquin and upper Kings, Dr. Lamb mentions special similarities between the San Joaquin dialect and that of the uppermost Owens River of Long Valley. He does not mention special resemblances between Kings River Monache and the Northeastern Owens people around Bishop. As a matter of fact, the Owens Valley Monache who are most directly opposite Kings River are not Northeastern Monache at all, but speak the South Owens dialect of Southern Mono-Monache. The Kings River Monache evidently came across the Sierra as part of the same movement that brought their brethren on the San Joaquin, and therefore are not derived from the people living immediately opposite them but also sprang from the North Owens River people. It is quite possible that the special upper San Joaquin-uppermost Owens similarities are due to continued and recent contacts via the pass over the crest from Mammoth Lakes to south of the Ritter subrange.

On the Yokuts side of the line, it is significant that of all the Yokuts still in the foothills in the historic period, those on the San Joaquin, such as the Dumna and Chukchansi, spoke most like the mass of Valley dialects, so much so that in 1907 I still reckoned them as linguistically in the Valley moiety. Next similar to Valley Yokuts were the Kings River tribelets, such as the Choinimni, whom I classed as clearly part of the Foothill moiety. With the Kaweah foothill drainage, we are definitely in the heart of the Foothill linguistic moiety, with its characteristic sounds, d, n, i, e (ü, ö) and typical suffixes like preterite -ji and aš, continuative -ad, and so on.

The Yokuts details of classification thus point to the first impact of Monache immigration being down the San Joaquin; the Monache situation seemingly points the same way. This inference in turn brings up the question whether the whole Valley moiety of Yokuts may not have originated from tribelets originally living as far up the San Joaquin as it was occupied, and down it for a certain distance, though not very far down after it turns northward, say to the Fresno, Chowchilla, or perhaps Merced River. As these tribes, of what may then have constituted the northern frontier of Yokuts speech, were pushed downstream, they initiated the northward movement down the broad valley. Quite likely the expanded territory stimulated an increasing population, and this in turn fed back into more expansion, somewhat as with the Navaho. Such a movement, though mainly north-northwestward, might also have flowed partly in the opposite direction, though there it would have encountered speech-related and habitually friendly tribelets.

It is also not necessary to limit the start of this spread wholly to the upper San Joaquin; the Kings River tribelets may have become involved almost at once. At any rate, it can be said that there is reason for inferring that the later Valley division or moiety of Yokuts is mainly an expansion out of dialects formerly situated on the upper San Joaquin River with possible participation of those on the upper Kings. Before this expansion terminated it had covered an area three or more times greater than before, mainly northward at the expense of aliens, but perhaps also southward to the shallow and unstable Tulare Lake. What began as a loss of territory to aliens; eventuated in a larger gain in another direction.

Meanwhile linguistic distributions in the southern half or two-thirds of old Yokuts territory (say around the southern third of the historic Yokuts area of our maps), where there was no pressure from the Monache or other Shoshoneans, remained relatively stable, not exceeding local shifts of tribelets. This is shown by the fact that it is at the southern end that the most aberrant forms of Yokuts speech were encountered after 1770, Tule-Kaweah, Poso, and Buena Vista, but each restricted to a limited territory.

In my 1907 dialect classification I delimited "Southern" and "Northern" (now "Central") Valley dialects, perhaps somewhat arbitrarily, by a line drawn across the plain at about Fresno city, between the Kings and the San Joaquin rivers. The Southern Valley group included four tribelets on Kings River, four on the Kaweah, most of both groups in the deltas of their streams; three on Tulare Lake, which receives the normal drainage of the Kings and Kaweah as well as the Kern; but only two, the Koyeti and Yawelmani, in the long stretch of 85 miles from north of Porterville to Bakersfield and Tejon Rancho. The streams that enter the valley here are Tule River, Deer Creek, White River, Poso Creek, Kern River, and Walker's Basin, Caliente, Sycamore, Comanche, Tejon, and Paso creeks. The central habitat of the Koyeti may be described as the delta of Tule River, that of the Yawelmani as the upper delta of the Kern; the lower delta of the Kern, including several lakes and long tulares, was the haunt of the Buena Vista tribelets.

How far all these thirteen Southern Valley tribelets may have spread over this upper end of the San Joaquin Valley as part of the movement which carried the central and northern Yokuts tribelets down the San Joaquin Valley, is difficult to say. Certainly there were some Yokuts tribelets dispossessed from the

upper Kings and Kaweah; and these, or original foothill tribelets dislodged by transmitted pressure, may well have been the ancestors of the historic tribelets of Valley speech on the lower Kings and Kaweah, perhaps even on Tulare Lake. It is easily conceivable that at a former time the Buena Vista tribelets of historic Foothill speech (read: original southern Yokuts speech) held the waters and sloughs and reed marshes of Tulare as well as of the more southerly lakes.

If this was the case, the thin ribbon of Koyeti-Yawelmani occupancy along the east edge of the southern valley plain—directly alongside (and in part between) the seven Tule, Poso, and Buena Vista tribelets of most pronounced Foothill quality of speech—this nonexpectable distribution in historic times would find its simplest explanation in a relatively recent southward thrust of the Koyeti and Yawelmani between the Tule and Poso and the Buena Vista groups. This assumption is of course not provable at present, but it does seem most in accord with the total context of known Yokuts dialects and topography.

At any rate, it fits in with the more general inference which seems to me established with some probability, namely, that the dichotomy of Yokuts speech was in the past not so much into Foothill and Valley divisions as into a Southerly and a Northerly division. The Northerly division at first did not extend north much beyond the main SW-to-NW bend of the San Joaquin. But with Monache dispossessing the uppermost tribelets on the San Joaquin (and perhaps Kings), impacts of unsettlement were transmitted downriver, which were before long transformed into aggressive expansion by the dispossessed over the valley plain, mainly northward and of farthest reach there, but probably also southward. The northward thrusts gave the Yokuts a large expanse of new territory; the southward pressures resulted rather in internal Yokuts redistributions, such as the separation of the Foothill Buena Vista from the Foothill Poso and Tule tribelets by intrusive tribelets of Valley type of speech.

The era of these events I would estimate at somewhere around five hundred years ago, perhaps beginning then and continuing for a couple of centuries or so before a period of relative restabilization set in. It seems that the period could not well be much lengthened, say to a thousand years, else the Valley dialects would have lost more of their grammatical homogeneity, which, in spite of a fair lexical differentiation, remains striking.

Although this has been a historical reconstruction, which of course can never be given 100 per cent proof, it seems to deserve some probability credence—until a better one is devised—because it rests on fairly detailed knowledge of a considerable number of dialects of one language, with allowance for their taxonomic classification. The inferences derivable from Dr. Lamb's corresponding survey and classification of the Shoshonean Monache dialects mesh rather nicely and help explain certain unexplained language-topography distributions that had long been noted. I emphasize the two bodies of detailed dialect information, each obtained with uniformity and therefore internally analyzable and comparable. The reconstruction here outlined has not been made by pushing around highly varied blocks of linguistic data whose diversity is masked under uniformizing labels like Uto-Aztecan and Penutian, or even Mono and Yokuts. If I have a measure of faith in this reconstruction, it is because it rests on a fairly detailed, precise, and verifiable linguistic taxonomy.

Since the evidence on which this putative history of events rests is linguistic, two further linguistic observations will be germane.

In 1907, in pointing out certain phonetic and structural resemblances of the more southerly Foothills Yokuts dialects and their occurrence also in Shoshonean or Chumash, I suggested that these features had been taken over by the southern Foothill dialects from these foreign languages. These features were the mixed vowels \ddot{e} , \ddot{i} ("ö, ü"), possibly $d < l, \eta$, and an element k in the first person possessive (1907, pp. 328–337). I still think that some or most of these traits are likely to be cases of "borrowing" by Yokuts, but they are evidently phenomena of limited scope, superadded but not symptomatic, and I no longer suspect that similar alien influence may have had a dominant or serious hand in the modification of southerly Foothill away from the remainder of Yokuts. Rather would I see the three most southerly Foothill dialects, apart from some scattering modifications, as being relatively archaic dialects in the sense that they probably once represented a considerably larger proportion of Yokuts speech, which, however, sank into a smallish minority through the efflorescence of northern dialects which expanded and subdivided into the "Valley" moiety or majority.

The second matter concerns a point which I first developed, or misdeveloped, in 1925 in the short chapter on the Penutian Family in the *Handbook* (pp. 347–350). I quote a paragraph which accompanies the map of figure 33 on the organization of the Penutian superstock.

The hearth of the "Pen" [Wintun, Maidu, Yokuts] as well as the "Uti" [Costano, Miwok] group, and consequently of the entire [California] Penutian family, is therefore the spot at which all five of the principal languages abut, namely, the region where the conjoined Sacramento and San Joaquin debouch into the head of San Francisco Bay. Here or near this point is the philologist's center of gravity, and in this vicinity, too, the ethnologist must look for the greatest interchange of customs. The historian, however, need be on his guard against assuming this overflowed region of sloughs and tule swamps as the original home of the Penutian family. Natural condition would render such a conjecture extremely unlikely to be true. This center point is one where rising differentiations were most efficaciously prevented by international contact or covered over by new assimilations. The speech and perhaps the customs of this half-drowned region, where the two great rivers of the State meet tidewater, are likely to be more similar to Penutian speech and customs of a thousand years ago than the tongues and habits of any other [now] Penutian area, and that is all. The first seat of the family while it was yet undivided is entirely unknown.

In spite of the negative caution at the end, the paragraph is ambiguous. I argued that we must not believe that Penutian originated in the delta, but yet that the languages and cultures found at the delta best reflect generic Penutian language and culture through assimilation. Even the metaphors point two ways. A hearth emits radiations, a focus gathers them, yet "hearth" is the term used. Philological "center of gravity" strictly means a point determined from its established peripheries, but somehow suggests the point of origin also, especially after "hearth." As a matter of fact, too, although the delta does contain much "overflowed" and "half-drowned" slough and swamp, it also contains fertile dry land, and the latter prevails in its environs.

In the light of present knowledge I would reëvaluate the *linguistic* facts bearing on Penutian as indicative of the following conclusions.

1. We do not know where the California Penutian languages originated. They

seem more similar to one another than to any extra-California languages recognized as Penutian, but it may be that personally I see more similarities because I have known them longer and better. Even if this taxonomic point is sustained by additional knowledge, it remains possible that California Penutian constitutes a unit and that it split off from other, non-California Penutian elsewhere than in California. There would be some presumption, however, that the further internal differentiation of the California block took place mainly, perhaps wholly, within California. But determination of the probable point of origin of the California unit cannot be even attempted except within the full context of the entire Penutian group. If Macro-Penutian is valid, the context becomes much enlarged and complicated, and an answer is not likely to be forthcoming soon.

2. The five California Penutian languages present a good array of striking similarities, but also a disconcerting array of as yet wholly inexplicable dissimilarities. Their development has been much more complex than the steady diversification of a trunk into limbs and of limbs into branches. Considerable time must have elapsed to reach the recent condition of the languages: I would estimate three thousand years as a minimum since first separation, and probably much more.

3. The guide lines on *Handbook* map 33 probably show with a certain correctness lines of connectedness of sublanguages and dialects within the five stocks, each viewed separately. They do not properly show lines of diversification from a supposed center of origin at the delta, as the arrow points at their ends would suggest, and as I apparently half-guessed, or wanted to believe, in 1925. The analysis of Yokuts dialects in the present study shows that Buena Vista and Yawelmani are not end products of either diversification or migration from north to south within Yokuts. Buena Vista is more likely a remnant of something old in its area; Yawelmani reached its habitat probably by a movement that was minor, local, and late; and the major late expansion of Yokuts has almost certainly been toward the delta, not from it.

4. This northwestward spread of Yokuts undoubtedly can be connected, with reasonable assurance, with anomalies of distribution of Miwok and probably of Costano, once the internal taxonomy of these languages has been worked out with a degree of reliability.

5. To a very considerable degree, basic or original Yokuts, Miwok-Costano, Wintu, and Maidu look as if their structural and lexical divergence from one another had occurred during a period when they were not in close contact or interinfluence. Such a period may have been followed by one of contact and increased assimilation. Or there may have been several such periods. Or different ones of the four stocks may have been variably in different contacts with one another—let alone foreign contacts. In any event, as we proceed in the analysis of the languages, it becomes increasingly evident that their history has been much more complex than we tend to infer from the pleasingly simple, radiating picture which they present on the map, especially when printed in four blocks each of uniform color.

WINTU

The Wintu and the Northern Paiute were the California peoples who first occurred to me as illustrating the principle of recency of spread as inferable from degree

of uniformity of speech. However, I must treat the Wintu more briefly than the Monache and Yokuts because the dialect data for them are scant, uneven, and inexact.

The name Wintun was standardized by Powers and especially by Powell as editor of Powers when the latter's *Tribes of California* was reprinted in Contributions to North American Ethnology with supplementary vocabularies. This was before Powell had been bitten by the bug of priority borrowed from biological nomenclature, after which he rechristened the Wintun the "Copehan" stock. The large colored map in Powers still called the stock Wintun, as most of us have done since.

In the rough, the territory of the Wintun as a stock was the west side of the Sacramento Valley. It was therefore a relatively long and narrow territory. It comprised two languages, which were well differentiated from each other, but probably only dialectically divergent internally. These two are most conveniently distinguished by their words for person, which are *patwin* in the southern tongue and *wintun* or *wintu* in the northern. Though Patwin and Wintun-Wintu are obviously related, they are more than "mutually unintelligible"; they are quite markedly different, so that the time of their divergence must be set pretty far back, perhaps around two thousand years ago. Why Patwin and Wintun-Wintu have not each diverged more internally since then, and why there are no transition languages or dialects between them, is a real problem. They may have been separated for a long period so that the Patwin associated only with other Patwin (besides aliens) and thus kept their dialects reinfluencing one another, and likewise for the Wintun-Wintu. This steadying influence would have been stronger if each population was then small and its territory small.

The relation of Patwin and Wintun is somewhat like that of Achomawi and Atsugewi in the Pit River drainage, or Quechua and Aymara in South America. The two languages show wholly different stems for surprisingly many meanings; but when stems are cognate they are for the most part rather closely similar. The situation would be much more intelligible if we had reason to infer that there had been large-scale borrowing between them, or perhaps by one of them from a third language.

The Patwin in the south held the smaller territory, about 38 per cent of the total if our maps are correct. Their northern frontier was about at the line between Colusa and Glenn counties.

The northern division of the stock is subdivided at one of the branches of Cottonwood Creek, quite likely the same branch that now serves to separate Tehama from Shasta County. Ethnologists have got in the habit of calling the two subdivisions respectively Wintun and Wintu, from their terms for man or person. The more southerly of the subdivisions is also known as Nomlaki, the northerly as Shasta [County] Indians. Though no really reliably exact vocabularies are in print, Wintun and Wintu seem to be not very different, and apparently they intergrade, which is perhaps why the Indians vary or are uncertain as to which branch of the Cottonwood separated them. Of the total of 62 per cent of stock area held, about 29 was Wintun and 33 Wintu.

However, only a minority (14 per cent of the total) of the Wintu territory lay

in Sacramento drainage, more (19 per cent) in Trinity drainage, on the upper main Trinity and South and Hay forks. These Trinity Wintu are very sketchily known; no ethnologist has ever sat down with any of them even for a few days, or if he has, nothing has eventuated in print. But what we do have in the way of vocabularies, place names, and the like seems to be very similar to the Wintu and even Wintun in Sacramento drainage. We can therefore conclude that all the territory of the Wintu on the Trinity has been occupied by them recently, within a matter of some centuries.

In fact there is no known reason to assume that the spread was from Wintun to Sacramento Wintu to Trinity Wintu in successive neat steps. The whole Wintu subdivision may have been merely the northern frontier dialects or border marches of the Wintun, or near or even south of Cottonwood Creek, some four or six or such number of centuries ago. When their northward expansion began, they may have moved across the inner Coast Range into Trinity drainage simultaneously with pushing up the Sacramento and Pit. This cannot be affirmed, but must be considered a possibility.

Whom did they encounter and displace or push against? To the northwest, Athabascans; to the north, Hokans.

The remote and ethnographically unknown Wintu of the South and Hay forks of the Trinity extended downstream until they met such of the Hupa as lived above their main valley at Willow Creek and at South Fork. We do not really know how much Northwestern culture these Wintu took over from the Hupa and how much they retained of the Sacramento drainage culture they had started with. The Yurok on the Klamath, beyond the Hupa, knew of their existence but were uninterested; at any rate, nothing specific has ever been elicited from them about these Wintu.

The western boundary of these people presumably was the long ridge of the South Fork Mountains in the Coast Ranges, but there are mentions of some of them having crossed over into headwaters of the Mad and Eel rivers, permanently or transiently, into Whilkut, Nongatl, or Lassik territory.

The Wintu northward impingement on the Hokans affected several tribes and languages.

Of the eleven languages in California originally reckoned as independent but later as being Hokan, four—Esselen, Salinan, Chumash, and Yuman—were situated south of the Patwin-Wintun and need not be considered here.

Two others, Pomo and Washo, were abreast of Patwin-Wintun: Pomo adjacent on the west, and Washo to the east beyond the Maidu and across the Sierra Nevada. The contact with the Pomo is likely to have been prolonged and to have left territorial and perhaps some linguistic effects, but there is nothing superficially anomalous about either Pomo or Patwin distribution in 1770. Pomo population was rather dense for the area it occupied, and seven dialects are recognized, which probably group into three or four languages. This Pomo cluster is the only Hokan one of its kind in northern California, in fact in all California except for Chumash and Yuman in the south. These several Pomo languages, rather crowded together, suggest possible former Patwin pressure, or pressure from Miwok thrust northward by Yokuts expansion, or both; various other movements or impingements might

be conjectured. The Salt Pomo tribelet around Stony Ford is isolated in direct Sacramento drainage and may represent a separate eastward thrust by it, or it may be the remnant of a former greater Pomo extension across the inner Coast Range. It is however a local phenomenon and presumably a fairly late one.

North and northeast of the Wintu we encounter a close-knit semicircle of five Hokan languages in contact with each other: Chimariko, Karok, Shasta, Achomawi, and Yana. These were formerly reckoned as independent languages. Karok was not in immediate contact with Wintu since Chimariko and Shasta intervened, but it is obviously a part of this North Hokan complex. It is probably significant that no languages farther north have ever been classified as Hokan. This is the Hokan farthest north, and here a half-dozen of them were huddled together as if in a last stand. Atsugewi was classified under Achomawi perhaps only because of the meagerness of early data on both. It seems to be actually a sixth separate Hokan language in the region, though perhaps considerably influenced by Achomawi. The general picture on the map is that of a huge Wintu fist thrust upward into a diversified and yielding Hokan mass.

Cardinal in this situation are two facts: the Wintu expansion must have been recent, but the North Hokan diversification into what were long accepted as independent languages must be ancient. Swadesh, in a 1954 *Anthropologist* article on "Time Depths," gives some scattering Hokan glottochronological computations: in North Hokan, Chimariko and Yana separation 38 centuries ago; North and South California Hokan, Yana and Yuman 37 centuries; Central and South California Hokan, Yana and Washo 50 centuries. However provisional these computations may be—and some of the time depths are out of step with the map distances—they are all of an order of around 4,000 years, and consequently on a wholly different time level from the recent Wintu expansion for which I would estimate well under 1,000 years.

That means that the North Hokan diversification was a long accomplished event before Wintu expansion began, and that the main effect of this expansion must have been a further crowding together of well-differentiated Hokan languages which had managed, in spite of their long diversification, to remain geographically rather close together (or to reassemble) in northern California. The most remarkable thing about these Hokan stocks is that though related they show no intergradations; the next most remarkable is that, with possibly one exception, they each consist of one language only, with no variations beyond mutually intelligible dialects. Karok and Chimariko do not even have dialects. Yana has four, but all intelligible; Achomawi and again Atsugewi, one for each of their tribelets, which means they can all communicate.

Only Shasta has left evidence of several divergent forms along the southern border of the main Shasta, each spoken in a limited territory by a small number of people who were killed or died before an adequate record was made. Our vocabularies of these "Shastoid" forms of speech are quite brief, varying in content, and not too exact. Dixon recognizes the Okwanuchu on the upper McCloud, the Konomihu around Forks of Salmon, and the New River Shasta. Merriam is dubious about the Okwanuchu; he calls the New River people Tlahomtaho and cites a brief vocabulary for them which is at best dubious Shastan; and he puts another tribe,

the Hatokehewuk, on both upper branches of the Salmon. Sufficient analysis and comparison probably will some time decipher this fragmentary and conflicting little body of material and determine its Shastan, other Hokan, and non-Hokan ingredients. It may well turn out to contain unique and important data. But that will be only after all the surrounding sure languages are adequately known and we are in a position to distinguish what is reliably new from what is old, or old in an imperfect dress, or erroneous. One or two of these Shastoid languages may well turn out to be nothing but a pigeon Shasta talked by non-Shasta neighbors, or vice versa. At any rate, it is only along the borders of Shasta speech—which include also a Karok-Shasta border jargon—that we get any of these subsize, aberrant forms of speech that we cannot securely classify.

A hypothetical comparison may make the uniqueness of the North Hokan cluster of languages more vivid. Imagine an irregularly semicircular area about 175 miles across from east to west and again from northwest to southeast; within this, five or more properly six languages spoken, each in a unit territory, and all about as different from one another as modern Hindi, Armenian, Lithuanian, Gaelic, Portuguese, and Icelandic would seem if no records of ancestral forms of these or other forms of Indo-European had been preserved. We might add that the inner diameter of the semicircle—as if, say from Hindi to Gaelic—corresponding to the line from the nearest point of Yana to Chimariko, would be only 40 miles long. Further, that less than 100 miles would bring one from Hindi to territorially discontinuous modern Greek (corresponding to: from Chimariko to Pomo), and still less than that distance from Gaelic to Albanian (corresponding to: from Yana to Washo). And all this incredible snarl would be three-quarters enclosing a single non-Indo-European language corresponding to Wintu, whose area would be no more than 75 miles across from north to south and from east to west.

It will be apparent that though the recent spread of Wintu is an answer to one limited problem, the impingement of that spread on the North Hokan situation and the probable intensification of that situation touch on a whole set of other and most widely ramified problems in ethnolinguistic history. And the most needed data for attacking that and similar sets of problems is more vocabularies recorded with accurate phonetic apperception, phonemic determination, and semantic translation—in other words, by trained modern linguists.

10. PROBLEMS ON BOSCANA .

THE FOLLOWING ARE interpretations of certain segments of southern California Juaneño culture as the Franciscan missionary Jerónimo Boscana encountered and recorded it in the second and third decades of the nineteenth century. I view Boscana's report particularly through the lens of a modern grammar which is in press and an assembled manuscript dictionary of the adjacent and closely similar Luiseño language begun by Sparkman and finally readied for printing by George Grace and myself. Further, Boscana's statements are examined against general California Indian culture as a background. And finally, there are some points at issue due to the existence of two versions by Boscana.

Let us begin with the last consideration.

THE TWO VERSIONS

Jerónimo Boscana was born on Mallorca in 1776 ; he was stationed as a Franciscan missionary at San Juan Capistrano from 1812 to 1826 and died in 1831 at San Gabriel. The Indians he described were known to the Spaniards and Mexicans and thence to Americans as Juaneños. Their speech was a northern dialect, probably wholly intelligible, of the Luiseño language named after Mission San Luis Rey. The customs of the two groups, Juaneño and Luiseño, were also similar. Since the Juaneño on their north adjoined the Gabrielino (named after Mission San Gabriel), a culturally active people, they served as purveyors of ideas and ritual from them to the Luiseño on the south.

Boscana's account is by common consent the outstanding ethnographic description left to the world by the Franciscans of any of the several nationalities of California Indians in their charge whose living culture they destroyed. His picture is much the fullest, is spiced with concrete detail, but also is definite in its broad contours, and, for his time and profession, is liberal and enlightened.

First knowledge of Boscana's account by the world of scholarship is due to Alfred Robinson, who in 1846 published in New York a work called *Life in California*, in which he included an English translation of Boscana's manuscript. The original Spanish manuscript has never been found. Robinson says merely, on page 234: "After his [Father Boscana's] death, in 1831, it was found among his effects, with other writings, which came into the possession of the Syndic of the Missions, who kindly presented it to me." Three years after Boscana's death, namely in 1834, and twelve years before the publication of Robinson's book, the upper California missions were secularized.

It is not known what title Boscana gave his work, or if he gave it any. The title Chinigchinich was probably bestowed by Robinson, who took it from chapters ii and iii of the manuscript where it appears as the name of a deity. It seems unlikely that a Franciscan missionary would have named his booklet after a heathen god. Robinson's translation is somewhat flowery, and we do not know what liberties he took with the vanished original. He almost certainly meant his translation to be faithful in essentials, for he was evidently interested and impressed by the original; his version is coherent and consistent. The Chinigchinich appendix fills pages 229-341 of *Life in California*.

After upper California became American, Robinson's book was reprinted, but without the Boscana appendix; there was more expectable interest in the immediately pre-American Mexican California than in the beliefs and practices of another tribe of Indians. On the other hand the enthusiastic Alexander Taylor reprinted Robinson's Chinigchinich alone in his prolonged series "Indianology of California" in the *California Farmer*, San Francisco, Vol. XIII, 1860. Files of this weekly journal have become perhaps more scarce than the original Robinson volume, but the typographical correctness is that of a frontier newspaper. In 1933 Phil Townsend Hanna issued a de luxe reprint, at Santa Ana, of the Chinigchinich alone, with voluminous scholarly annotations by John P. Harrington. This was an expensive edition, and has become rather scarce.

Within a year the translation of a second version of Boscana's account was issued by the Smithsonian Institution (1934). This translation was by John P. Harrington, and is entitled: *A New Original Version of Boscana's Historical Account of the San Juan Capistrano Indians of Southern California*. Robinson's "Chinigchinich" has disappeared from the title. The text is a variant version of Robinson's; the precise relation of the two will be discussed in a moment.

Harrington says: "It was . . . a gala day in my life, unparalleled by any other, when I recently discovered the long lost Boscana original." Discovered; but he does not say where. It is perhaps one of the innocent satisfactions of a scholar to prolong the gala day into a decade or a lifetime, with himself alone knowing where his discovered treasure remains lying, lost to the world. There are tales of famous palaeontologists who, having filled their museums with fossils, used a final blast to wreck their most prolific quarry.

However, my colleague Robert Heizer (1951, p. 4, n. 4, and p. 11) has solved, from the printed literature, the secret of the history and whereabouts of this second Boscana manuscript. It was secured at Santa Barbara Mission in the early 1850's by L. de Cessac, brought to Paris, and acquired in 1884 from de Cessac's associate, Alphonse Pinart, by the Bibliothèque Nationale, where it became No. 677 of the Spanish and Portuguese manuscripts. It has been seen there recently by our colleague John Rowe and by Edward H. Carpenter, Jr., of the Huntington Library; the latter, in fact, secured a microfilm copy of it.

The relation of the two versions remains uncertain and may remain undetermined until close textual criticism will perhaps indicate one as probably earlier. Harrington speaks of his find as "the long lost Boscana original." Perhaps he means merely the original Spanish of *one* of the two drafts written. In the Introduction to the Harrington version, where Boscana speaks of ten years' labors, there is a marginal annotation, "from 1812 to 1822," 1822 being thus its apparent date of composition. The Robinson version mentions the effect on the Indians of the news of the Emperor Yturbe succeeding the King of Spain in Mexico, and of the comets of 1823 and 1825; but both passages occur in an addendum, "Characteristic Anecdotes," outside the numbered chapters, and may have been added subsequently.

On the following page I have tabulated chapter numbers, titles (somewhat abbreviated), and lengths in number of pages of the two versions. It will be obvious that each version contains or lacks two or three chapters, and that those com-

mon to both often come in different order. Thus the Vanquech Temple is fourth in Robinson, eighth in Harrington. The number of pages comes out nearly double, 105 for Robinson, 56 for Harrington; but a sample count gives 280 words a full page for the first and 540 for the second, so that each account totals close to 30,000 words.

ORDER AND ABBREVIATED TITLES OF CHAPTERS IN THE TWO BOSCANNA VERSIONS

ROBINSON'S BOSCANNA	HARRINGTON'S BOSCANNA
I. Of What Race? (5 pp.)	1. From What Race? (3 pp.)
II. Creation of the World (7)	2. Creation of the World (2)
III. Creation of the World according to Playanos (9)	3. Life of Quiot and Origin of Indians (4)
IV. Vanquech Temple (6)	8. Vanquech Temple (4)
V. Obedience and Subjection (6)	7. Obedience and Subjection (4)
VI. Instruction of Children (8)	4. Instruction of Children (7)
VII. Matrimony (7)	5. Marriages (6)
VIII. Mode of Life and Occupation (4)	6. Manner of Life (2)
IX. Feasts and Dances (7)	9. Feasts and Dances (5)
X. Extravagances (6)	11. Extravagances (3)
XI. Calendar (4)	10. Calendar (3)
XII. Wars (4)	
XIII. Funeral Ceremonies (6)	12. Burials and Funerals (2)
XIV. Immortality of the Soul (13)	13. Immortality of the Soul (6)
XV. Origin of the Population of San Juan (5)	14. Origin of the Inhabitants of the Mission (3)
	15. The Rancherias Inhabited (3)
XVI. Character of the Indians (6)	

By way of example, Robinson's chapter vii is named "On Matrimony," and Harrington's chapter 5, "About their Marriages," but they correspond in the main. Harrington's version begins with a paragraph about marriage in general which Robinson perhaps omitted as nonspecific; or Boscana may have written it in one of his versions only. There are three paragraphs on proposal and betrothal, about 500 words long in H, 450 in R, which agree pretty well; the one H item missing in R is that the bridegroom must observe continence while working for his bride's household. R also has run into the text a note of explanation that H gives in full. R follows with a long paragraph on the nuptials, with speeches in direct quotation, which matter in H forms paragraphs 5 and 6, the speeches being in indirect discourse and part of paragraph 8. Paragraph 7 of H is 2 in R. The long paragraph 4 in R is followed by a brief one, 5, about Boscana himself in 1821 having married a boy 2 years old to his contractual bride of 8 or 9 months; the H version includes the incident in paragraph 8, but has the girl about 6 months old. On the semi-couvade, R has only the main fact of diet observance in half a dozen lines, even omitting a reference to the Ancients' observing the same odd custom. The corresponding paragraph in H is more than twice as long, mainly devoted to telling how the child will die if the parents break the taboo, and how even the converts still observe it; and there follows a second paragraph of equal length narrating a case history. Probably it was Robinson who made this heavy reduction, but in the absence of a Spanish original, it is possible that Boscana himself wrote the

brief version. Sometimes, however, the ethnographic detail occurs only in Robinson, as, for example, the "Verses Sung to the Betrothed," which are discussed below. These come between paragraph 7, on whether the native marriages were true ones, and 8, on first pregnancy, which in H follow immediately after each other as 11 and 12. The final paragraphs, 10 in R, 15 and 16 in H, deal with recognized homosexuality. These run substantially parallel until mention of the native terms for berdaches, which Robinson fails to give. He may have cut them out here, although usually he seems to have preserved the Indian words cited by Boscana. After that, R concludes with a statement by a Lower California missionary about homosexuality among the Colorado River Indians, but H goes on to take issue with the famous Torquemada as to the inverts being impotent and to insist that they also had intercourse with women.

This sample analysis will give an idea of the degree to which the two versions differ. It will also serve to negate any suspicion that Robinson examined the H version and "translated" it with serious reduction. There is enough material occurring only in his translation to insure that Boscana himself wrote two versions.

What would be theoretically desirable would be the publication of the Harrington version in the original Spanish. This should not prove too costly, but if it does, the two English versions should be reissued on facing pages so that parallel passages could be properly compared; every passage should of course indicate its chapter and paragraph number, however transposed. Or it might be feasible to couple a reprint of the Spanish of H with a consistent facing by the corresponding English of R, again with indication of chapter and paragraph, and with the unique passages of R assembled together at the end. This is after all much the most important ethnographic document on the California Indians left by the Franciscans who converted them, and a collation of its two versions is a scholarly need.

THE BRIDAL SONG

Turning now to specific Boscana materials, I cite first some "Verses Sung to the Betrothed" as they are given on page 282 of the Robinson version (they do not appear in the Harrington one) and then follow them with my reconstruction into Luiseño, with literal translation. Words or parts of words which I cannot identify have empty square brackets corresponding to them in the translation. The stress accent is written when it falls on the second syllable; otherwise it is on the first.

Quic noit noivam
 Quic secat peleblich
 Ybicnun majaar vesagnee
 Ibi panal, ibi urusar
 Ibi eebal, ibi sefa, ibi calcel

I go to my home
 That is shaded with willow
 These five they have placed
 This agave, this stone pot,
 This sand, this honey, etc., etc.

ki·k no-[it] [noyvam]
 ki·k šaxát pelé-vli-š
 ivi-k-non maxá·r pesaŋa-q
 ivi pa·nál, ivi aró·sat
 ivi exval, ivi sa'is, ivi [qalsel]

house-to I-[] []
 house-to black-willow trodden
 this-to-I five go-outdoors
 this yucca-butt, this basket-rim-mortar
 this sand, this "honey," this dwell-[]

I would construe the song as putting words into the bride's mouth, rather than as sung to her. The five foods, materials, and implements mentioned have to do with woman's work and housekeeping.

Father Boscana's apperception or at least writing of Juaneño was not too fine, considering his ten or twelve years' hearing it. Some differences from Luiseño are dialectic: as the first vowel of *secat*, "willow," as against Luiseño *šaxát*; e ~ a, like e ~ ay, is a frequent correspondence. Boscana's writing s for š (sh) is owing to the lack of sh in standard Spanish of his day, although a few centuries earlier š had been represented by x, as it still could be in his native Catalan; so he wobbled between s, x, and ch. But the middle consonant of *secat* was familiar to him as the Spanish *jota*; that he did not write *sejat* is just slovenliness or indifference; see ecbal for ejbal, exval below.

On the other hand he does write j in *majaar*, "five," besides correctly designating the second vowel long by doubling it. The sound r occurs in Luiseño, but not in final position except in two loan words from Gabrielino, one of which is *maxár*, five. Juaneño territory lay between Gabrielino and Luiseño, and Gabrielino influence or borrowing would expectably be stronger in Juaneño. Indeed, we have *urusar* for *aró-sat*, a type of mortar, the -t being one of the regular absolutive noun suffixes, to which Gabrielino -r presumably corresponded.

One Luiseño and Juaneño term for honey is "[bumble] bees their water," an alternative being "bees their excrement." This appears in the song as non-pronominal "excrement" simply; but in general Boscana's rendering is far from close. For one thing, he has omitted the absolutive suffix -š, which is mandatory except when there is a possessive pronominal prefix.

Harrington, in his notes on the 1933 reëdition of Robinson's Chinigchinich, also has difficulties with the bridal song, and for the same reason: the text is corrupt. For the last word, he simply deletes the internal "l" and reads *qašil*, white sage, which I am disposed to accept. My try falls back on the verb stem *qal*, "to be somewhere," "to live," but I would not know of what Boscana's syllable -š*el* is a corruption. *Urusar* Harrington translates as being a spherical pot of steatite, whereas my "stone mortar with a rim or hopper of basketry" derives from Sparkman, whose knowledge of actual ancient objects was far surpassed by Harrington's.

VILLAGE NAMES

Chapter 15 of Harrington's Boscana is entitled "About the Rancherias Inhabited by these Indians." It lists the fifteen principal villages of the Juaneño, analyzes their names or tells incidents accounting for them, and adds the name of the chief of each. Both sets of names are analyzed here.

1. Putuidem, "navel sticking out." Sparkman gives *po-tidi* for "his, her navel." The -m might be plural, though that is mostly not expressed with body-part terms. The sticking out is implicit: it is mentioned in the story of the incident, but it is not expressed in the word itself.

2. Atoum-pumcaxque (or -pumiaxque, says Harrington), small insects like yellowjackets that live in the ground. We have several Luiseño words for wasps and bumblebees, but none like *ato*; -um would be the animate plural. The Luiseño word nearest to *atoum* in form is *a-toula*, its (indefinite) trunk, butt.

The second part of the phrase analyzes into *pum-*, their: -i- (more likely than -c-), unexplained; -axque might stand for *aške* or *axke*—probably the latter, which corresponds to Luiseño *akí*, nest, hole. The x may be there to indicate a momen-

tarily fricative approach to k or q frequently heard in Luiseño. The whole phrase then would mean: "wasps their holes."

3. Ulbe, "California Sagebrush," (*Artemisia californica*), I would reconstruct as ulvi-š or olvi-š, which however does not occur in Sparkman and not in the Kroeber-Grace notes.

4. Tébone, an "edible herb" growing "in the lagoon" or "at the port" of the mouth of the stream flowing by San Juan Capistrano mission. This is also wholly unidentified.

5. Eñe, a shore plant with edible salt on its leaves. This might be *Distichlis spicata*. Boscana's ñ here is not for n̄ as in Spanish "n tilde," but for η = ng; for eñla is salt in Luiseño, objective eñil; cf. eñ-samut, alkali according to Sparkman, but šamut is the generic Luiseño word for grass. Perhaps eñ-samut is primarily the salt or alkali-yielding grass Boscana refers to, and only secondarily its alkali.

6. Panga, "cañada." This is simply pa-ŋa, at the water, which is what a cañada would contain.

7. Souche, "a little cañada or gulch." Sparkman gives wolakat for gulch or canyon; we have no record of šowtša (which would be the presumptive Luiseño equivalent of souche), occurring with the sense of valley or gully. The nearest in form to souche that I can find is šowut, "black" rattlesnake, diminutive sow-mal.

8. Tobe, "white clay for painting." Luiseño, tovi-š, white clay. Again Boscana omits the absolute suffix -š, without which in Luiseño the word would be in construct or compounding form.

9. Túmume, a "flat or bench on a hillside." The nearest Luiseño form is tu·ma-l, opuntia cactus species, diminutive tu·ma-mal, to·ma-mal. The Luiseño suffix -mai, place of, would be -me in Juaneño (cf. 15, uxme). The literal meaning thus seems "opuntia place"; the particular spot may have been a flat or bench.

10. Tepipeche or Tapipeche, a kind of "bush or chamizo" (brush). No Luiseño equivalent is known.

11. Ecjelme, "wild amaranth seed," an important food. Sparkman does not mention anything comparable in the botanical appendix of his "Culture of the Luiseño Indians" (in AAE 8:187-234, 1908), but his dictionary gives eké·la as a red-flowered plant with black seeds. Here we have -cj-, that is, -kx-, for Luiseño -k- (or -q-)—presumably the same combination of sounds as in no. 2, though in reverse order. The -me might be Luiseño -mi, -me, objective plural; but it might equate with the diminutive -mal, construct form -may; or most likely it is -may, place of, as in nos. 9 and 15. The one hitch with this identification is that in Luiseño other suffixes ordinarily replace the absolute suffixes: the forms would be eké·la, eké·mal, eké·may, but not eke·l-may corresponding to Boscana's ecjelme.

12. Taje, "flint arrowhead." This is Luiseño tekét, -také in the plural construct. Again Boscana has omitted the absolute noun-suffix.

13. Uút, "arrow foreshaft," of "a special kind of bush." The Luiseño is u'út, and denotes the chamisal bush *Adenostoma fasciculatum*, which is standard for arrow foreshafts, the main shafts being of cane, *Elymus condensatus*. However, Sparkman and we both recorded ukiš (u·ki-š) for the foreshaft itself.

14. Alume, "raising the head in looking upward"—the site being at the foot of the steep mountain called El Trabuco. There are several Luiseño parallels, none

quite exact. There is a verb *lomi*, *lomax*, to blow over, overturn, upset. A verbal noun formed from the medio-intransitive form of this stem, with the indefinite possessive prefix *a-*, is *alómax*, expectably: "their being upset," actually denoting "waves." A similar and possibly cognate form *alómax* or *alúmax* means "live coals, embers." The Juaneño place name undoubtedly corresponds to one of these two forms, more likely the first, but Boscana was translating an implication, or the Juaneño word had crystallized around a different metaphor from Luiseño—its "overturning" or "upsetting" being of the upward-gazing head.

15. *Uxme*, "rose." Here the *x* has the old Spanish value of *š*, not of *jota*. The Luiseño for rose is *uš-la*, the diminutive *uš-mal*—or *uš-may* in both the objective and the construct form—or most likely with *-may*, place of. The site is called *ušmay* by the Luiseño and gave rise to Spanish *Las Flores*, a creek and valley at the Juaneño-Luiseño boundary.

These village names seem wholly pre-Spanish. Seven of them refer to a plant (nos. 3, 4, 5, 10, 11, 13, 15) growing at the site, two to a product (nos. 8, 9). Most of the others refer to landscape or landmarks (nos. 2, 6, 7, 9, perhaps also 14). Even the "her navels" of no. 1, whose reference is mythological, presumably has its legendary incident connected with a natural rock or soil formation at the site.

CHIEFS' NAMES

With each of the fifteen villages Boscana gives also the name of its chief. It is unclear whether these are legendary founders or the actual individuals encountered as town heads when the Spaniards came. The chapter on villages and chiefs follows the one on the inhabitants of the mission which recounts a sort of migration legend of how the Juaneño originated among the Gabrielino Puvuiem or Puvu people at Sejat ("Bumblebee") at Los Nietos across the lower Santa Ana River. This story may reflect a historical element, namely, the relatively recent bringing of the *Changíchnich* cult from the Gabrielino to the Juaneño, who then passed it on to the Luiseño. But the migration legend seems also to tie on to the creation story. It may further be that the Juaneño had developed a particular set of pseudohistorical legends about the original chiefs of all their principal towns, in which the chiefs' names figured as a specific element of the tale; however, I know of no close counterpart to such a type of story in California. However, Boscana is not unqualifiedly specific whether this series of chiefs were in Juaneño opinion primeval or in the recent past—possibly those in office when the mission was founded.

1, 2. *Choqual*, "lift it up." This would be *čokwal*, evidently cognate with Luiseño *čakwi*, to seize or filter, and even more closely with *čakwlax*, to wrestle. Probably the combination *-kwla* was too much for Boscana—or perhaps for the Juaneño—and it was metathesized to *-kwal*. Wrestlers do lift to throw one another, but Boscana's translation is again connotive: "lifting" is *heti* or *kwati* in Luiseño.

3. *Temiachocot*, "where much willow grows." This would be a strange name for a person. I can venture two partial analyses in terms of Luiseño. One renounces translation of *temia-* but construes *-chocot* as a poor rendering of *šaxát*, black willow. This is possible, since Boscana was troubled by *š* and variously wrote it *s*, *x*, or *ch*; and he also tended to confound the fricative and the velar stops *x* and *k*. At least the willow part of his translation is preserved by this reconstruction.

An alternative is derivation from *teméčax*, to sun oneself, bask, from *temét*, sun; with the habitual agentive *-kat*, it comes out *teméčakat*, basker, sun-loafer, in a form that corresponds very closely to Boscana's original, although his translation must then be wholly jettisoned.

4. *Tobalaúc*, "wrinkled old man." This is again a translation by implication, referring neither to being wrinkled (*kwaši'i*) nor to old man (*naxánmal*). *A-towla* means "bottom," "butt," "trunk," and *po-towla* means "his great-great-great-grandfather" or "his ancestor." *Tobala-* represents this stem *towla*; the *-uc* (*uk*) I cannot explain except that *-q* is a suffix of indefinite present tense. The word might thus mean "is an ancestor" and therefore: "he is aged and wrinkled."

5. *Sidoc*, "jet or gush of water." There was such in the creek near the village where *Sidoc* was chief, but it is obscure why, and unlikely that, he should be named for the phenomenon. I am unable to suggest any confident analysis, but there are several Luiseño verb stems in *še-* or *ši-* that might be involved. Thus, *še'i*, prick, shoot; *šéhápax*, burst open; *šexax*, hiss, boil; *ši'ax*, urinate; *šili*, spill; *ši'li*, pour. The last pair seems the closest in form or meaning, especially as *r* and *d* sometimes replace each other in Luiseño, and Boscana's *-c* (*-k*, *-q*) might again be the tense ending.

6. *Seqüilqüix*, "plant which dries up." I assume that *qüi* stands for *kwi*, whereas Spanish *qui* would be read *ki*. If so, the form would be *šekwílkwíš*, corresponding to syncopated stem duplication with shift of accent of *šékwi* > *šekwí-škwí-* > *šekwí-lkwí* (initial consonant > 1), which, with the nominal absolutive suffix *-š*, represents a standard pattern of adjectival or past participial formation. All that is lacking is a recorded Luiseño base *šekwi* meaning "dry." The assumption of such a base is, however, made probable by a long series of actual Luiseño adjectives of this form for which no corresponding free verb base is reported; like *taká-tka-š*, one-eyed; *tšewó-šwo-š*, tousled; *tuyú-tyu-š*, lukewarm; *matá-mta-š*, square—and in fact *čañí-čñi-š*, Changíchnish, "Chinigchinich" itself. The change of initial consonant to *l-* also has precedent, as in *tovó-lvo-š*, many colored; *hivé-lvi-š*, wide; *kašá-lša-š*, noisy.

7. *Toroc*, "to limp, sprain foot." Luiseño, *no-e' torí'kat*, ankle, *no-e'* being my foot, **torí'* an unrecorded verb stem, *-kat* the agent. This seems a more defensible etymology than to go by direct meaning and to base on *čokaxot*, lame, related to *čuka'ax*, to kneel. Again the Boscana translation proves to be connotive and inexact.

8. *Quapchocops*, "caretaker, watchful." This is Luiseño *kwavčokat*, caretaker, maintainer, judge, umpire, from *kwavčo*, to take care, watch, observe. In the final *-ps*, the *p* was probably misheard for *t* by Boscana; the *s* is an error.

9. *Temex*, "stumbler." Luiseño *tedémax*, stumble, trip. The Luiseño intervocalic *d* alternates with spirant *ð*, which a Spanish ear is likely to suppress, as in vulgar *hablao* for *hablado*: *tedéma-* > *teðéma-* > *teéma-* > *tema-*. The *-x*, if meant for *jota*, would stand for the tense suffix *-q*; if for *-š*, it would be the absolutive nominalizing suffix.

10. *Páat*, "mountain sheep." Luiseño *pa'at*.

11. *Talet*, "hump-backed, crook-backed." Translating the given meaning into Luiseño, we find *ya'áš po-pi'di*, man his-broken. However, there is a verb *taki*, to

straighten, which has reference at least contrary to that of a broken or hunched back. Also, takalaš, which looks as if it might mean "place or means of straightening," is the word for "kidney," which lies under the small of the back. There is evidently some idiom involved, of which Boscana renders the purport but muffs the etymology.

12. Gualua, "drag it." This would equate to kwalwa, hwalwa, or walwa; or hwalu'a. "To drag" is xa-wi in Luiseño, and xawwi is to catch rabbits in a seine net; the intransitives or passives would be xa-wax or xaw-wax. This is evidently our gualua, but requires emending its first syllable to read gau- in Spanish orthography (= gau-) and the second as -hua (= -wa) instead of -lua.

13. Uchat, "all unanimous." There is nothing known in Luiseño to correspond to this. U-tšanat, nearest in form, means "elk," an animal known by report by the Luiseño though not found in their territory.

14. Cusuol, "severed, cut." The Luiseño cognate is košó'lax, to be sour (we too speak of acid "cutting"), which is connected with košax, be sweet. The verbs that literally mean to cut off, across, or into are all wholly different: woki, čeni, čevi, čori, čorí'i.

15. Chululeck, "hair tied on top, chief's insignium." I can do nothing with this, except to point to a derivative from čori, cut, namely čoró'i, to level, even, measure, complete, from which there are denominatives like čoró'ikat, surveyor, and čoró'ilaš, thing to measure with. The Luiseño r-l substitution comes more natural to a Spanish than to an English ear; and the "tied on top" might be loose for "hair (or feather ornament) leveled on top, standing up even." This is conjecture only, but the best I can venture, and I hope not too farfetched.

As meanings pile up for these names, more and more suggest a nickname type. About half-a-dozen are downright disrespectful, and some of the others may be.

1, 2. Choqual	Wrestler
3. Temiachocot	Sun bather
4. Tobalauc	Great grandpappy
5. Sidoc	Squirter (?)
6. Seqüilqüix	All dried up
7. Toroc	Knuckle, Ankle (?)
8. Quapchocops	Caretaker
9. Temex	Stumble
10. Páat	Mountain sheep
11. Taclet	Straight-back (ironic?)
12. Gualua	Net dragger (?)
13. Uchat	?
14. Cusuol	Sour-puss
15. Chululeck	Level-top, Bristle-top

The question arises: How is it that the Juaneño referred so casually, humorously, and derisively to the reputed ancestral founders of their lineages or towns? That it is these founders who are referred to is what Boscana seems to mean, for in his preceding chapter he tells how Chief Oyaison of Puvu, with his daughter Coronne

or Putuidem, led a migration out of the land of Sexat to Acagehemem, with change of their language to Juaneño. After Coronne-Putuidem turned to earth, Choqual, a near relative, became chief, as also of village number 2 in the list. This would seem to indicate that Choqual and the thirteen others were considered venerable founders.

Even the names of the daughter whom Oyaison left in charge when he returned to Sexat, Coronne or "lady bug" and Putuidem or "her navel [projects]," are scarcely respectful. It is a strange pattern for a legendary national history, and one to which I know no exact parallel. All we can do at present is tentatively to accept it and to hope that the future will bring further knowledge which will make the pattern and its motivation more intelligible.

If we could infer that the names were those of historic chiefs encountered by the Spaniards, we might conclude that they were nicknames used behind their backs by their following, or possibly in interclan reviling contests (nyachish, "enemy singings"). Also, the Spanish tendency not to bother to learn to pronounce the real appellations but to call prominent natives Gordo, Cojo, Ciego, Sordo (Fatty, Lame, Blind, Deaf) might be invoked; and these nicknames by the Mexican settlers and soldiers might have been retranslated into Juaneño. But these suppositions must be held in reserve as long as the chiefs' names are likely to have been traditionally ancient.

CHINGICHNISH, GOD OR MAN

The Luiseño know the name Chinigchinich or Chinigchinix in the form Changichnish (čanjčičniš), which is a participial or adjectival derivative, by duplication, syncope of vowel, shift of accent, and addition of the noun suffix -š, from an otherwise unknown verb stem changi (*čañi). When the Luiseño venture to translate the name, they do not give an etymology but refer to one or another of the deity's attributes or functions. He is a high god, punisher of ritual infractions; he is abstractly conceived, and outside mythology. For this reason I suggested as long ago as the *Handbook*—published in 1925 but its writing finished in 1917—that Changichnish might be a reaction formation, an invention due to imported stimulus, made by natives desirous of preserving their old religion; an imitation of the Christian God of the missionaries, whom they took over and furnished with a native name and added to their own beliefs. The Luiseño certainly attribute the Changichnish part of their religion to a Gabrielino origin, specifically associated with Catalina Island (*Handbook*, pp. 621–622). They still sing their boys' initiation songs with Gabrielino words. However, the name Changichnish, or Chingichnish as the Juaneño may have spoken it, has not been reported from the Gabrielino, whose language may not have contained the root *čañi nor the precise derivational process of Luiseño. They are said (*Handbook*, p. 622) to have called the deity Kwawar (Boscana, Quoar), "initiate," or Pura, which latter sounds like a Gabrielino counterpart of Luiseño pul, pula, meaning "shaman," "religiously wise."

I still consider that Christianizing influences have entered into the formation of the Changichnish belief. The cult remained confined to the tribes reached by the Franciscans. On the other hand, a great deal of the older mythology of the Luiseño and Juaneño, in fact everything concerned with the god Wiyot (Ouiot)

and his death, has many close parallels as far away as the Yuman tribes of the Colorado.

Now, however, comes Harrington, in the introduction to his version of Boscana, with a flat and specific statement that goes far beyond my conjecture, but yet is not supported by a word as to the source of his information.

He says (p. 2) that the religion

...described by Boscana centers about the revelations of a prophet named Chinigchinix... [who] was known by three sacred names: Saor, meaning common person, noninitiate; Tobet, medicine man, initiate; and Quoar, a name too sacred to pronounce aloud. These three names apply to three successive periods in the prophet's revelatory life. The prophet was born at the rancheria of Pubu in Los Angeles County, California, only a couple of miles inland from Alamitos Bay, there accomplished his principal teaching, and when he died, was from there merely translated to the heaven of the stars, leaving no earthly bodily remains. From above and everywhere he watches our deeds and thoughts...."

Some of these statements are out of Boscana: the three names (p. 14 in H, p. 255 in R); and that he "went away dancing to heaven." The R version adds (p. 254):

... there appeared in the same town of Pubuna [where Quiot had been], one called *Ouiamot*, son of *Tacu* and *Auzar*.... And this was the God *Chinigchinich*,... who taught first in the town of Pubuna, and afterwards in all the neighboring parts...[p. 255]. The name of *Quaguar* was given to him when he had died and ascended above among the stars."

It is clear, I think, that in this passage and its context Boscana believed that he was telling of a false heathen god of whom the foolish Indians believed this and that folly.

Now Harrington goes further and says that Chinigchinix-Quoar was a prophet, in other words a man, a human being. He says, moreover, that he was born at Pubu—not that the Indian legend had him born there. Is this an inference drawn by Harrington from Boscana's statements? Or has he possibly collected from the surviving Indians or from some other documentary source hitherto unrevealed information about a particular Gabrielino, perhaps *Ouiamot* son of *Tacu*, who turned prophet of a Messianic cult during mission days, the cult surviving him as a retronative one and spreading to other tribes?

This sort of thing might well have happened; if so, we would all like to know the specific account that says so, and from whom it was obtained, so we could judge for ourselves how much of it to accept, and why.

If on the other hand there is no new documentation, and it is simply Harrington's conjecture that *Ouiamot* was merely one of several hundred miserable Gabrielinos at Pubu who proclaimed himself and was accepted, not only as a prophet and Messiah but as God himself under a name like *Changichnich*—well, an avowal that the statement was conjectural would at least be informative as to the basic situation, and reasons for belief might be convincing.

Without some further illumination, Harrington's view, though interesting and possible, is wholly unsupported by either evidence or argument, and can be viewed only with reserve.

In both cases, this one and the matter of chiefs' names, the same factor enters: whether the situation reported was recent, virtually contemporary, and therefore actual and more or less historical, or whether it was part of the established native

belief and value system and therefore primarily legendary. Indians have difficulty sensing time lapse. Boscana is not always too clear whether or not he thinks his Indians attributed an event or situation to their beginning of the world or to the generation immediately preceding their own. Harrington with his "prophet" implies the latter for the Changichnich cult. But he also implies that after his death the prophet was exalted into a supreme deity. Could it be that the prophet or his followers were born so late as to live in mission times, and therefore to have heard of a far away Messiah who was the Son of God, and that they were emulating Him?

11. DESERT MOHAVE: FACT OR FANCY

MR. ROYAL D. MARKS OF PHOENIX, attorney for the Chemehuevi Indians who now live mostly on the Colorado River Reservation and whose agency is at Parker, Arizona, has put into my hands some twenty-five typewritten pages of ethnographic and geographic notes. They were collected in 1934 from several Chemehuevi informants by the late Richard F. Van Valkenburgh and Malcolm Farmer in connection with an archaeological excavation made in the region of the Providence Mountains of southern California under the auspices of the Los Angeles County Museum.

These notes contain a story of a group of Mohave living in the desert until their extermination by the ancestral Chemehuevi. This tale is of human interest besides raising some ethnologic problems, and since it has no direct bearing on the land claims in which Mr. Marks represents the Chemehuevi, I have asked Van Valkenburgh and Farmer to allow me to publish and discuss it.

CHEMEHUEVI AND MOHAVE

The principal Chemehuevi informants used were Mukewiune, a woman "well over 90" in 1934, who was not sure but who believed that she was born some miles west of Eldorado Canyon; Tasamavant, a woman of 80, born in the Paiute Springs region; Satinia Lopez, near 40, born at the old Dominguez ranch on the eastern slope of the Providence Mountains; and two half-breeds.

It should be noted that the three full-blood women among these informants were all born north of the line which Isabel Kelly, in the map in her 1934 paper, "Southern Paiute Bands," draws as the northern limit of the Chemehuevi against their close kinsmen who constitute the "Las Vegas band" of Southern Paiute. A sharp distinction between the Las Vegas and Chemehuevi bands is difficult to find or maintain, as Kelly herself says. I am rather inclined to define as Chemehuevi all those Southern Paiute who gave up small-patch farming at springs and, when they could, adopted Colorado River natural-flood farming and therewith came into contact with and some cultural influencing by the Mohave. This push to reach the river was a nineteenth-century phenomenon, as has been set forth above in "Recent Ethnic Spreads" (No. 9, pp. 261-263); it could develop on a larger scale only after the expulsion of the Halchidhoma about 1828, mainly by the Mohave. Neither eyewitness Garcés in 1776 nor Pattie in 1826 reported any Chemehuevi or Paiute actually on the river.

However, this lateness of bottomland occupation by the Chemehuevi must not be construed as meaning that until 1828 the ancestors of the present-day Chemehuevi were all still living in what Kelly maps as Las Vegas Paiute territory; Garcés in 1776 found a settlement of forty Chemehuevi west of Whipple Mountains—between this massif and the Turtle Mountains, in the heart of what Kelly delineates as nineteenth-century Chemehuevi territory. It is evident that the drift of Southern Paiute southward to become Chemehuevi had already occurred *within the desert* by the latter half of the eighteenth century; it was only the final move eastward to the river and its bottomland farming that dates from 1830-1840 and following.

I believe that it would be entirely legitimate to argue that it was this late push

to the river which did more to develop the Chemehuevi into the distinctive entity or nationality which they now constitute, than had the much older slow drift southward within the desert. It is clear that we cannot derive all the farming Chemehuevi on the river in the nineteenth and twentieth centuries from ancestors living in that southern part of the desert area which Kelly assigns to the Chemehuevi as distinct from the Las Vegas band. As just stated, all three of the Van Valkenburgh-Farmer full-blood informants were born north of Kelly's line separating the groups, and in Kelly's terminology would be Las Vegas Paiute.

In short, as far back as nearly two centuries ago, some Southern Paiute had already drifted southward both to the Providence Mountains region and to the Old Woman-Turtle-Whipple Mountains region, in both of which areas Garcés found them, called them "Chemehuevi" by their Yuman appellation, and considered them one people. The eastward push to the Colorado River, stimulated by the expulsion of the Halchidhoma by the Mohave, could not have got under way until after 1830, and it may have been begun by the nearer Chemehuevi of the Old Woman-Turtle-Whipple Mountains region. It was certainly also participated in by their kinsmen from the Providence-New York Mountains latitude. These more northerly people whom Kelly classes as Paiute have always been known to the Mohave as Chemehuevi also, and their movement to the river would have been southeastward rather than southward. It was the closer contacts of both these bodies with the Mohave and their riverine holdings that forged the ethnic nationality which modern popular usage, Kelly's study, and official U. S. appellation all designate as "Chemehuevi"—as distinct from the "Paiute" of Las Vegas-Pahrump from whom they were once undifferentiated and with whom they are still closely allied in speech; and as distinct also from the Moapa, Shivwits, and more easterly Paiute.

Kelly's valuable map shows the whole right bank of the Colorado, from Topock at the foot of Mohave Valley down to the Palo Verde Mountains, as if it were undisputed Chemehuevi territory. It was undoubtedly claimed by them, especially after their indecisive war with the Mohave in 1865-1867, and there is no doubt that they occupied and farmed a series of patches on the west bank. But of course the whole of this stretch, except Chemehuevi Valley in the narrower sense, was conquered by the Mohave in alliance with the Yuma, and parts of it on both banks were always, though somewhat variably, occupied by both Mohave and Yuma. The Chemehuevi seem to have come in on sufferance or were tolerated as a sort of poor relations or hungry friends from the desert. As late as 1858 they were found by Ives occupying only two stretches of the west bank, one from about or a little above Blythe to the southeastern end of the Big Maria Mountains, the other in Chemehuevi Valley proper. They seem not even to have claimed any territory anywhere on the eastern bank, except probably opposite Chemehuevi Valley. This contrasts with the true or long-time river tribes, the Mohave, Halchidhoma, and Yuma, who always claimed and used both sides indifferently, because they looked upon the great river as a basic internal and uniting element, not as a boundary or frontier as desert people would feel it.

Mukewiune, the oldest of the Van Valkenburgh-Farmer informants, born well north of Mohave Valley, not west or south of it, said that she was living at Beaver

Lake (opposite Fort Mohave) and near-by Hico ("white man's") Springs while the Fort was being built in 1858. She remembered the captive Oatman girls, and was a near age-mate of Olive, the elder, who survived. Although the histories of Olive nowhere mention her exact age, she was apparently not far from 16—less rather than more, I would estimate—when she was delivered at Fort Yuma in 1856. This would make Mukewiune born most probably around 1840 to 1842, and actually of the "over 90 years" attributed to her when she was interviewed in 1934. If Mukewiune was a "Paiute" in her youth, she was certainly a "Chemehuevi" in her old age; she had been living on the Parker reservation, where nearly all Chemehuevi are now concentrated, until she moved away in 1934. George Snyder, her son, who Englished her story about the Desert Mohave, is probably the same George Snyder carried on the 1955 Agency Roll as a half-blood Chemehuevi born in 1890 and living at Needles. He had learned the story from Mukewiune, who had it from her mother; it was told as having happened in the time of that mother's mother—George's great-grandmother. This might have been around 1780–1800, if the event was actual.

These considerations are gone into because it seems of consequence to know with precision the ethnic definition both of informant and of her group.

Here is the tale, with my emendations or explanations in square brackets.

MUKEWIUNE'S STORY

There were once a people called Turat Aiyet, which means Desert Mohave. There is none living: we killed them all off. They spoke almost like the Mohave on the river, and like them they burned their dead. But they dressed in mountain sheep hide, and they lived like the Chemehuevi.

They lived from the New York Mountains to Mohave River Sink [Soda Lake] and perhaps beyond toward Daggett [This last stretch was Vanyumé]. Many of them lived "below" [southeast of] Danby in the Old Woman Mountains.

Once the Chemehuevi men were out since early morning hunting sheep in the mountains west [actually nearly south] of Goffs (the Piute Mts.). Then they heard a woman calling from the great black lava peak there. They hurried over to hear. She shouted: "The Desert Mohave have come and killed children and stolen women. Your wife [the headman's] has been taken." The headman's wife was pregnant with her first child.

The men returned to their home camp and found the women all gone and many of the children dead.

They conferred and decided to pursue. They followed and overtook the Desert Mohave as these were roasting and tasting some of the headman's wife's flesh. They were singing: "They're cooked! They're done!"

The Chemehuevi attacked, there was a fight, and the Desert Mohave fled, heading for a spring in the [Piute?] mountains near Goffs. There they lined up before the spring to keep the Chemehuevi from water. The Chemehuevi chief urged his men to charge, there was a close-range fight, and the Mohave were scattered a second time. They fled south to a cave near Danby [in Ship, or Clipper, or most likely Old Woman Mountains]. The Chemehuevi piled wood and brush in front of the mouth of the cave to smoke them out. Some died inside; the rest were killed as they came out.

The headman was still angry about his wife. The people decided to get rid of all the Desert Mohaves once for all. They started hunting them. They hunted them from Nantapughwai [Kelly, Nantápiaxantⁱ, Turtle Mts., east of Old Woman Mts., Mahpuh-khive, Kelly Mamápu-kaibⁱ, west of Whipple Mts.] all the way to Clark Mountains, Oh-khy [Oxai, north of the Union Pacific R.R., in fact north of Ivanpah Mts. which are north of the New York Mts., in about 35° 30']. They killed them wherever they found them.

Only one got away: Tavavits, the chief. He made a short cut from Old Woman to Stepladder Mountain and escaped. [Stepladder Mt. does not appear on any map known to me.] This mountain is between Summit [perhaps Summit Springs in Providence Mts.] and Turtle Mountains. It is called Ouí-che-hanie [Aví-chehaní?] "Sliding Rock" by its Mohave name. The Chemehuevi almost caught Tavavits, but he got away and went to live with the River Mohave near Beaver Lake, opposite Fort Mohave. That was the end of the Land Mohave.

When they were hunting them out of the Turtle Mountains, a few Chemehuevi were cornered there. They got on top of a peak, and as the Mohave came up by the single way, they shot them down one by one. [I conjecture that this peak may be a domed or bell-shaped one standing free on the east flank of the Turtle Mountains and which the Mohave call Taha'. They tell that the Chemehuevi knew where it had water, and therefore some of them once took refuge on it and not only stood off the Mohave but held out longer and survived. When I was told this in 1953, I assumed it referred to the Chemehuevi-Mohave war of 1865-1867. However, a later incident may have been transferred to an earlier period, or vice versa, or an actual event have become incorporated into a legendary pattern.]

DAISY SMITH'S STORY

Isabel Kelly got essentially the same story from her Paiute informant Daisy Smith, who had been born at Las Vegas; and with Dr. Kelly's permission it is reproduced here.

It was at Intsingivaxantⁱ spring, north from Goffs, and west from Taxon-kwadid, a southern prolongation [unnamed on our maps] of Piute Range. There the Tir-aiyet killed a young Chemehuevi girl. Her grandfather, visiting there, saw it. The Tir-aiyet went on to "Eagle spring," Minímpaxant [at a small isolated peak, apparently to the east, probably near the Sacramento Mts.] and killed two more Chemehuevi girls. They cooked one and ate her, dancing, singing, and saying she tasted good. The girl's father was watching, together with another Chemehuevi. They became very angry.

The Chemehuevi then moved to Okwái, Ivanpah Mountain. Many of the men were off [hunting?] at Kavitsi, Awawatz Mountains, far to the west, when the Tir-aiyet came [to Okwai? or where?] and attacked and killed the old people and children—all who could not run away. One young woman escaped with [such able-bodied] men [as were there], got to the top of a mountain, and shouted in warning. Then the Chemehuevi men began to return; but there were more than a hundred Tir-aiyet and for a while they killed off the Chemehuevi one by one as they came up. But finally the Chemehuevi [got the upper hand and] killed off all

the Tir-aiyet, killed them on Anpani-kaiv. This means "call out mountain" [perhaps because of the woman's shouting in warning. It is north of Old Woman Mt. and south of Goffs, which in turn is south of the Taxon-kwadid spur mentioned above, which comes off from Pasa-kaiv, "farm mountain," which is our Piute Range, well north of latitude 35° and between New York Mts. and Newberry Mts. (Avikwamé); whereas Piute Mountains are between Clipper Mt. on the west and Sacramento Mts. on the east.].

Then the Chemehuevi divided among themselves all the mountains which the Tir-aiyet had had: Newberry, Piute Range, New York, Providence, Granite (Mo'kwa), Old Woman Mountains, and the spring Ma'upa in Turtle Mountains. Those places belonged to the Chemehuevi from then on.

So far Daisy Smith.

This is evidently a Vegas Paiute account of the origin or beginning of the separation of the Chemehuevi as a more or less distinct group. The Shoshoneans who were first abused and outraged by the Yuman Tir-aiyet and who then in exasperation turned the tables and exterminated their oppressors, are throughout referred to as Chemehuevi, not as Paiute. They are evidently thought of as having become Chemehuevi by taking the land of the Desert Mohave.

On the other hand, five of the seven mountain masses mentioned—in the desert the mountains are the nuclei of settlement because in general only the mountains have springs—lie in territory that Kelly's map shows as Vegas Paiute; only the last two are in her Chemehuevi territory. But also none of the mountains riverward from Turtle Mountains are mentioned, though these were used by the nineteenth-century Chemehuevi—the long stretch from Sacramento through Mohave, Chemehuevi, Whipple, and Riverside to Big Maria Mountains.

ETYMOLOGIES

Van Valkenburgh and Farmer used the two-word form Turat Aiyet, "desert Mohave, land Mohave," in which the final -t's are the absolutive noun ending. Kelly uses the compound form Tir-aiyet, slurred from Tiri-aiyet, the first element as also in Tiri-niwi or Tir-niwi, "desert people," denoting Chemehuevi not settled on the Colorado River, or Las Vegas Paiute in distinction from Chemehuevi.

Sapir, on page 683 of the Dictionary of his 1930-1931 *Southern Paiute*, gives the stem as tira-, desert, open expanse, bare, (spirantizing any following stop sounds); in absolutive form tira-vi or tira-phi, desert, prairie, plain, and in composition tira-niwi, desert person, person without home or clothes. Probably cognate is the stem tirayua, middle, center, as in tira'xoa-va'-nti, in the middle. These forms are in Moapa dialect of Paiute. The Moapa use Aiát'a, plural Aiát'a-ŋwi, for the Yumans most familiar to them, the Coconino or Havasupai. I recorded Ayát as the Chemehuevi term for the Mohave, as in *Handbook*, page 595.

HISTORY OR FANTASY

In giving general information or discussing actual territories and intertribal relations, the Mohave never mentioned to me any former branch of themselves in the desert that was wiped out, nor any Yuman-speaking people to their west.

Nor do I recall such a reference in any of the literature published on the Indians of the region. (There are some suggestions of parallels in two Mohave formalized legends or myths, which will be discussed below.) The question therefore arises whether the two accounts obtained in 1934 rest on a measure of factual events remembered by the Chemehuevi but by preference forgotten by the Mohave on account of the unfavorable outcome; or whether they are wish-fulfillment or similar fabrications whose significance lies in the cultural patterns of thought revealed and not in their historic authenticity.

However, there exists in the region of the Mohave Desert another tale of extermination of a desert tribe, this time within the framework of history. It seems profitable to analyze this second story before judging how far the Desert Mohave tradition may be authentic.

THE FATE OF THE VANYUMÉ

In the first decade of the present century I tried to pick up the trail of the Vanyumé or Mohave River Serrano whom Garcés had discovered in 1776, had called Beñemé, and quite properly had classified as Jeniqueche or Serrano in speech. He entered Beñemé territory from the east, with Mohave guides; a few months later he recrossed it from the west, then traversed the Chemehuevi holdings of Providence Mountains and eastward, and returned to the Mohave. By my time, there were no native resident Indians along Mohave River, though I heard reports of some "Chemehuevi" (who might rather have been Kawaiisu or Serrano) near Victorville. The Mohave, however, knew about the former residents on Mohave River, for their route to both the San Joaquin Valley and to the coast of southern California and subsequently to the missions and Spanish settlements, had followed Mohave River, as later an emigrant trail, then a horse express and freight route, and finally the Santa Fe Railroad followed it. Also, some of the Mohave song-cycle itineraries travel the same route. For the name of the old inhabitants of Mohave River, the Mohave volunteered Vanyumé, which is close enough to Beñemé, seeing that Mohave *v* and Spanish *v* and *b* are all bilabial. (As for *a* and *e*, I also heard Hakwiche for Garcés' Jequiche, and Hanyuveche for his Jeniqueche, denoting Cahuilla and Serrano.) In summary situations the Mohave tended to English Vanyumé as "Tejón" or sometimes as "Tehachapi Indians," much as at first they always called their former Halchidhoma neighbors Maricopa, trying to use familiar white man's terms. But when they became specific, and knew enough American geographical terms, the Mohave regularly put the Vanyumé on Mohave River below and above Barstow, and had other terms for the Tejón and Tehachapi natives.

The Mohave told me that an old Vanyumé woman was living among them, and I ran her down. She was called Moha. She knew as little English as I had learned Mohave, so communication between us was via Mohave who spoke English. She said, as per *Shoshonean Dialects*, (1907, pp. 70, 139), that she was called Hamuha (Hamuxa, Hamukha) after her birthplace near and west of Daggett on Mohave River. (In another context, a Mohave told me of a Vanyumé place north of Daggett called Ahamoha in Mohave.) It was from a similarity in sound of this birthplace name that Moha was given the Mohave gentile name Moha. (The Mohave frown

on marriages with persons that lack a *simulye* or clan. Once she was given a proper Mohave clan name, Moha would presumably be marriageable among the Mohave.) Moha's mother was from Avi-ahnalye, "gourd mountain" in Mohave; her father from Chokupaye, she said. Some of her old kinfolk had been related to the Mohave by marriage. These relatives brought her to the Mohave for safety, in a period in which the Vanyumé were otherwise virtually exterminated, "it is said by Mexicans." This would have been before the coming of the Americans, for she was still a little girl. She had lived among the Mohave ever since. She recalled certain words of Vanyumé with difficulty, but there is no doubt that the vocabulary I recorded from her (1907, pp. 71 ff.) was Serrano.

Moha knew that the Chemehuevi called the Vanyumé Pitanta. She called the Chemehuevi Yuaka-yam, the Mohave Hamahava-yim—a Yuman name with a Shoshonean plural. She had names of her own, Vanyumé names, for the Cahuilla, Kawaiisu, and Yokuts, which were different from the Mohave names. I understood her to say that the Vanyumé called themselves Mōhinyam or Mōhineyam (probably for Mih-); but this may be a confusion with a Serrano clan of wildcat moiety named Mohiyanim, cited by Gifford (1918, p. 179) as living with the Coyote moiety Morongo in and about Bear Valley in the San Bernadino Mountains (Strong, 1929, p. 11, calls this group Mohiatniyum.) The upriver Serrano in the San Bernardino Mountains, whom Moha and I identified by the Mohave term Hanyuveche, were "like brothers" to the Vanyumé and spoke the same dialect, but were a separate people. She put the division between them at Daggett, which seemed to me pretty far downstream as compared with all other sources. I ended (1907, p. 140) by doubting how accurately she was able to discriminate between the several Serrano branches.

A comparison of her fragmentary vocabulary with a double-informant Kitane-muk Serrano list from Tejon shows several differences. Thus "two" is wahi against Kitanemuk wo; "tobacco" is pivat against tsivut; the "whites" are Ayai-kik-am against Haiko-yam.

So far the relevant data as recorded fifty years ago from a Vanyumé survivor!

Now come two supplementary statements made thirty years later about the same desert group but by Chemehuevi or Paiute.

CHEMEHUEVI ACCOUNTS OF THE END OF THE VANYUMÉ

The first Chemehuevi account is again by Van Valkenburgh and Farmer, and runs thus.

The River Mohave set out against the Ahvanomet and found them at Newberry Springs. [These springs are east of Daggett, at the northern edge of hills looking down on Mohave River.] They killed all the Ahvanomet except one girl, Moha, who was taken captive and brought back to the Colorado River. Later she was married to a Mohave called Chigquata. This fight was between 1835 and 1850.

The second statement is by Kelly and tells the same story, though in brief form. Dr. Kelly was asking her Las Vegas informant Matavium about the Vanyumé or Beñemé. Yes, he knew of them, he said. His people called them Pitántⁱ. They lived at Barstow and spoke Maringiants, Serrano. Once Mohaves were traveling toward the California settlements and found these people all asleep and killed them.

The three accounts agree that there was a massacre of Vanyumé, and two mention the girl Moha as surviving among the Mohave in the twentieth century. The two Chemehuevi versions make the Mohave the perpetrators. Moha herself, or her Mohave interpreters, probably hedged on this point, as well she might after having been brought up and adopted and married among the Mohave. Hence my "it is said"; it would be convenient to blame Mexicans, but I evidently had some doubts.

As to motivation we have nothing. Since their 1865-1867 war there has been considerable ill feeling between Mohave and Chemehuevi, and they like to think of each other as having been wantonly murderous and treacherous. Better evidence is the statement that there had been intermarriage between Vanyumé and Mohave and that it was a kinsman or relatives who were part Mohave who brought the little girl Moha to the Mohave for safekeeping. This fits in with all that is otherwise known of relations between Vanyumé and Mohave. They were certainly peaceful in Garcés' time, when the Mohave traveled freely up and down the river of the Vanyumé.

The period estimated, 1835-1850, is probably right, both because of Moha's age when I saw her, and because the Americans who crossed the desert by traveling along Mohave River after 1850 make no mention of any resident Indians along it nor of a group identifiable as Vanyumé—Whipple in 1854, for instance.

But this brings us back pretty close to the date of secularization of the Franciscan missions, 1834. I had formerly assumed that the Vanyumé had been missioned, either at San Gabriel or at its San José station near San Bernardino, together with all or nearly all the other Serrano-speaking Indians. This can probably be ultimately verified or disproved from the rancheria names usually mentioned for each individual in the baptismal records of the missions, which are also dated; the account of the 1819 abortive punitive expedition led by Moraga against the Mohave contains Serrano-sounding names of stopping places all the way down the Mohave River. I had also assumed that after 1834 most of the Vanyumé survivors had stayed on and become merged in the increasingly Mexicanized ex-Mission Indians. But the three stories between them suggest that a body of them returned to Mohave River and met their end there some years after 1834.

Though it is a bypath from our immediate problem, it may be well to cite here the names and distances mentioned by Moraga in 1819, counting from Cucamonga, with identifications by myself.

Cucamonga

Amuscopiabit, 9 leagues, 25 miles.—Cajon Pass.

Guapiabit, 9 leagues, 25 m.—North of Victorville.

Atongiabit, 12 leagues, 32 m.—Near Barstow.

Topipabit, 8 leagues, 22 m.—North of Newberry, between Yermo and Manix.

Cacaumeat (Cacanmeat), 3 leagues, 8 m.—Perhaps Cody Springs.

Sisuguina, 4 leagues, 11 m.—Somewhere near Afton, perhaps at springs in Cave Mts. They would be not yet at Soda Lake.

Angayaba, 15 leagues, 40 m.—Perhaps Marl Springs; but it would be a very long day.

Total, 60 leagues, 160 miles.

Cucamonga was in Gabrielino territory, and the word has the Gabrielino locative ending -nga. The next four names have the Serrano locative ending -bit, or perhaps really -abit; the first is in Serrano proper territory, the next three in Vanyumé. The next, Cacaumeat, may have a version of the same suffix in its -meat. Sisuguina would be near the eastern limit of Vanyumé territory; I can do nothing with the word. Angayaba is surely well within Chemehuevi-Paiute holdings, though probably short of the Providence Mountains. I have not found it otherwise listed as a place name, but the -ba is almost certainly -va, locative in Paiute; and the first two syllables seem to be Paiute *aŋqa-*, red (as in *aŋqa-yīvi*, red pine (Sapir, 1930-1931, p. 549), and *Aŋkai-yunab*, red malpais, the Palen Mts., in Kelly Chemehuevi notes). It was probably just as well that the expedition turned back. A small force—dispirited, their animals short of both feed and water—might have been annihilated when a tribe like the Mohave came out against them.

FREMONT'S PRESUMED VANYUMÉ INFORMANT

The disappearance of the Vanyumé from their desert river, however, brings up John C. Fremont's eye-witness account of 1844 (published in 1854) when he was trying to return from central California to Great Salt Lake, and, drifting southward and eastward on the north flank of the San Gabriel Mountains, struck a horse path, "the Spanish Trail," that led him to and along the Mohave River. He reached the stream probably at or shortly upriver from Helendale on April 20, and left it for a northeasterly course on April 25, perhaps near the present Manix or Afton. He saw no resident Indians nor evidences of them; but on the evening of April 23, after passing the Calico Mountains and the present Daggett and Yermo he met a party of five long-bowed Mohave carrying gourd canteens, who were presumably traveling to the coastal settlements. With them was an Indian who could speak Spanish because he had been raised at a mission and who interpreted between Fremont and the Mohave. At the breaking up of the missions, that is, by 1834 or soon after, "he had returned to the mountains, where he had been found by a party of *Mohahve* (sometimes called *Amuchaba*) Indians, among whom he had ever since resided" (Fremont, 1854, p. 376).

Fremont does not say in so many words that the interpreter was originally from Mohave River, nor that he had gone back to his natal home, nor that he returned alone and encountered no one in the country until the Mohave found him; but all this or something close to it seems implicit in the statement. At any rate I cannot imagine any other hypothesis which would seem relevant to the words used. The mission-reared Indian called the leader of the little party his boss or patron, "mi amo."

He described the Mohave as living on the great Colorado, but added that "formerly a portion of them lived upon this river and among the mountains which had bounded the river to the [our] northward during the day, and that here along the river they had raised various kinds of melons. They sometimes came over to trade with the Indians of the Sierra" [that is, over the San Gabriel and San Bernardino Mts.]. Grammatically the reference of "this river" in the same sentence with "the Rio Colorado" might be ambiguous, but the following clause about the mountains they had passed that day can leave no serious doubt that "this" can

mean only the Mohave River and that the stretch in question is that between Barstow and Daggett. But what would Mohave have been doing, raising melons between Barstow and Daggett on an intermittent stream, when they owned the broad bottoms of Mohave and—by 1844—Parker valleys?

It has occurred to me that after the Mohave River country was emptied of its native Vanyumé by the missions, a few Mohave families might have moved in, at least intermittently and for whatever reason, to try farming there. Yet this would have been only two or three decades before, and, as a small-scale attempt that had been abandoned, it would hardly seem of interest to communicate to strangers.

I am therefore inclined to believe rather that this Mohave-adopted Vanyumé, or whatever he was in origin, was referring to a Mohave *legendary* tradition about Mohave clans or lineages that had gone west to live in the desert—the same idea that underlies the Chemehuevi stories about former “Desert Mohave.”

The beginning of my *Mohave Historical Epic* (1951, pp. 74, 77, 151) not only has two chiefs lead their clan lineage followings out of Mohave Valley, but brings them to this very same region—Kohoye near Barstow—and has them live there five years. Fremont's interpreter may have heard this precise legend or some general echoes of it, which, however, were likely according to Mohave habit to be quite precise in their localization.

The *Epic* is without accompanying songs and is at least pseudohistorical in its matter-of-fact tone and avoidance of supernatural elements. There were also some twenty or more Mohave song cycles, one of which, called Vinimulye-pacha, outlined in *Seven Mohave Myths* (1948), differs from all the others in that, together with the *Epic* alone, it also is without supernatural happenings and is therefore prosy in manner. It is further like the *Epic* in dealing with war and conquest, and it also resembles it in beginning with a Mohave group leaving Mohave Valley and settling in the west, although this west is less distant—in the Providence Mountains and in historic Chemehuevi instead of Vanyumé territory. At any rate it is evident that we have here, in the Vinimulye-pacha plot, a pattern of legend which the Mohave regarded as actual history, and which neo-Mohave like Fremont's interpreter in 1844 would so regard; and it is most likely this persistent floating legend that he had in mind in the information he gave Fremont.

Here also is where the threads of our two themes knot, the themes of the legendary though possibly also actual Desert Mohave and of the indubitably historic Vanyumé whose end as a group may have come to us in almost equally legendary guise. In both cases we deal with the desert—the desert west of Mohave Valley, and with an isolated group there, with their end by violence, with a lone, named survivor, and with Mohave being involved—though in the one case as the exterminated, in the other as the exterminators. The scene is in what we have erroneously, though more appropriately than might at first seem warranted, come to call the Mohave Desert surrounding the Mohave River.

MOHAVE-VANYUMÉ INTERRELATIONS

Why, if the Chemehuevi are right, the Mohave should have massacred the little Vanyumé group in their very homeland after ties of kinship and culture had been

built up between them would be hard to guess. However, we have the story recorded sixty to seventy years after Mohave and Chemehuevi had got themselves into a vicious little war of their own, and there was still bitterness and suspicion on both sides. It seems not impossible that after all it was the alleged "Mexicans" or someone else who really wiped out the Vanyumé. At any rate it was an event that would not have too much concerned the Chemehuevi around 1830 or 1840, whoever was responsible for the act. Yet thirty years later, dislike of the Mohave might well have combined with the floating pattern of legend of extinction of a desert group, to weave a story that was part fiction and part fact.

There are several indications of intertribal and intermarriage relations between Vanyumé and Mohave in native times.

First, the Mohave River constitutes part of a natural travel route between the Colorado River and the San Bernardino Mountains and the southern California coastal plain. For half the total distance the way runs between instead of across mountains, and there is water—intermittent and poor water toward the eastern end of the Mohave River, but sufficient. Spaniards and Americans both used the route, and railways and modern highways still follow it in the main. The Mohave were great travelers—from curiosity, for trade, from motives of adventure or prestige; and this was one of their well-worn trails. Garcés met a little party of them on the way in 1776 as Fremont did in 1844. Garcés' Mohave accompanied him without reluctance, though they ate Vanyumé fare of cattail roots with repugnance. Most older Mohave men I met between 1900 and 1910 showed a tolerable acquaintance with the farther half of the Mohave Desert. Either through song-cycle itineraries, hearsay, or having been there, they knew the names and the approximate directions and distances of the principal landmarks, not only along the Mohave River but beyond, south across the San Bernadinos and east to Tejon and Tehachapi. Apart from immediately adjacent neighbors, the Mohave were undoubtedly the most frequent visitors whom the Vanyumé saw. Intermarriages were likely to happen, and cultural influences to occur at least occasionally.

One such interinfluence of which we have record is the adoption by the Mohave of a Vanyumé songcycle.

The Mohave sing a cycle and story called "Tumánpa" in three versions. These they call respectively "long"; "short, odd, or crooked"; and "Vanyumé." Of the last variety, a Mohave called William Mellon sang a dozen songs for me at Needles in 1908. He said that this kind of Tumanpa was first sung—"dreamed" would be the typical Mohave way of putting it—by a man named Tavasqan who spoke Mohave but was a Vanyumé, though Mellon loosely translated this term as "Tejon or Tehachapi Indian." My experienced informant Jack Jones later corrected these statements. He said that the Mohave learned this singing not from Tavasqan himself but from his Mohave-speaking kinsmen. The words of Mellon's songs were not in Vanyumé, as Mellon sometimes affirmed, but in Mohave; Jack had himself sung them. He did not know that the full plot of the cycle was ever narrated among the Mohave, but the general outline of its itinerary was down the Colorado to end near Boundary Cone in Mohave Valley, after a detour east to Matevily-vove, upstream from Barstow on Mohave River.

The nature and history of this songcycle need not be pursued further here. But

it is evident that Tavasqan was a Shoshonean from the west, presumably a Vanyumé; that he had Mohave-speaking kinsmen, probably among the Mohave; and that these kinsmen imparted this kind of singing to the Mohave. (Incidentally, I have not been able to rid myself of the suspicion that Tavasqan is a Mohave corruption of "Sebastian." Now there is also the possibility that the name has a connection with the reported survivor of the Desert Mohave, "Tavavits.")

Next there is the old woman Moha, who still spoke Vanyumé for me sixty or seventy years after her kinsfolk, themselves related to Mohave, brought her to that tribe. As she married there, she probably left Mohave-Vanyumé descendants.

And third is Fremont's interpreter, who had gone to live permanently with the Mohave, presumably had married there, and may also have had tribally mixed children.

Of course it is possible that Tavasqan and Fremont's friend were the same person, or that one or the other of them was the kinsman of Moha who brought her to Mohave Valley. But in any event we have Vanyumé-Mohave intertribal ties and kinship through intermarriage indicated as established well before 1850, as well as resulting in the probable perpetuation of a strain of Vanyumé blood among the Mohave long after their disappearance as an independent entity.

THE LIKE-MOHAVES

There is one more relevant item in the border zone of fantasy and fact that we must consider. This is the belief by the Mohave, and in somewhat similar form by some other groups, that somewhere far to the west, but in territories where living men had traveled, there existed a people similar to the Mohave—evidently regarded as an offshoot of them. I said in 1925 in the *Handbook* (p. 612):

A curious and unexplained belief prevails among all the tribes in the Kitanemuk neighborhood, as well as among the Mohave, namely that there is in this vicinity a tribe that in speech, and perhaps in customs too, is almost identical with the Mohave. Sometimes the Kitanemuk are specified, sometimes the Alliklik, or again ideas are vague. The Mohave themselves speak of the Kwiahta Hamakhava or "like Mohaves" as somewhere in this region; they may have meant the Alliklik. There is no known fragment of evidence in favor of this belief; but it must rest on a foundation of some sort, however distorted. Perhaps it is the presence of an Amahavit group among the Serrano, as mentioned below.

And in *Shoshonean Dialects* (1907, p. 136):

[A] Yokuts informant . . . called [the Mohave] Amakhau; [a] Tübatulabal, Amakhaba; the latter regarded their language as similar to Kitanemuk, from which it is of course utterly distinct. Of . . . two Yokuts informants at Tejon, who also called them Amakhaba, one . . . classed their language as distinct, with some words somewhat resembling Kitanemuk.

The Amahavit group referred to is from Gifford, "Clans and Moieties," (1918, p. 179):

Amakhavit. A third Serrano clan (?) living north of the San Bernardino Mountains [the easternmost of three]. . . . Undoubtedly these Amakhavit are the mysterious Amakhau or Amakhaba of . . . Kroeber.

Benedict, whose map and information in her "Sketch of Serrano Culture," (1924), do not extend into Mohave River drainage, says nothing of Amakhavit; nor does Strong, in his *Aboriginal Society in Southern California* (1929). Strong

does equate (p. 11) his clan 9, Amutcakaiem (te=ch), situated at the northern base of San Bernadino Mountains, south of Victorville, and so shown on his map, with Gifford's clan 7, Amakhavit, presumably because of the similarity of the two first syllables and of the generic region being the same.

Gifford's Amahavit ["Amakhavit"] reduces to Amaha- plus the locative -vit; the stem is of course the Vanyumé place name Hamuha which served the Mohave as a springboard for bestowing on the Vanyumé girl they adopted the gentile female name Moha; it is also the same as the Mohave man informant's "Ahamuha," which he and Moha agreed in placing near Daggett—he to the west, she to the north.

All this, as Gifford says, makes my Shoshonean and Yokuts informants' statements about Mohave-Serrano likeness "take on a new color." Unfortunately, it is also a chameleon color. Several interpretations remain. The Vanyumé place name Hamuha-Amahavit may be of pure Serrano etymology, but its coincidental resemblance to the tribal name of the Hamahava = Hamak-hava Mohave may have caused either this people or the Serrano to think up a false explanation and talk themselves into a fictitious relationship. But what would Yokuts and Tübatulabál know of this etymology? What they obviously had heard of and believed was the fictitious tribal relationship. This belief then, and its wide spread, are the primary facts of consequence, whether or not the belief did originate in recognition of a coincidental homonym. The Mohave, too, are impressed by the idea of an unexpected resemblance of total speech and culture, not by a resemblance of sound of a name. They cited this name Hamuha not as evidence or explanation, but as a mere isolated geographical fact.

In summary, "like-Mohave" or "near-Mohave" group—settlement, clan, tribe, or nationality—somewhere in the quadrilateral bounded by the Mohave River (Vanyumé Serrano), upper Santa Clara River (Alliklik Serrano), Tejon Creek (Kitanemuk Serrano) and Tehachapi Pass (Ute-Chemehuevi-speaking Kawaiisu), was more or less believed in by these four groups, by their nearer Yokuts and Tübatulabál neighbors, and by the Mohave. Whichever group it is that in various places or times has been cited as resembling the Mohave, it is never in immediate contiguity with the Mohave, but for the whole of the historic period (nearly two hundred years long, back to Garcés) these elusive cousins have been separated from the Mohave at least by the Chemehuevi; and if the cousins are identified as having been the Alliklik or Kitanemuk or Kawaiisu, then the Vanyumé also intervened. In short, the like-Mohave tribe is always near the far western edge of Mohave knowledge, just as it is always members of the big tribe on the great river that visit their hill near-cousins, not the reverse.

HISTORICAL PROBABILITIES

An actual tribe of Mohave speech and customs in this western mountain desert is of course definitely fictitious within our historic knowledge. Even when a specific actual group is cited for the resemblance, the resemblance is imaginary, or limited to coincidences in isolated terms.

The Desert Mohave are known only through legend, and only through Chemehuevi legend, but they are placed immediately west of the river Mohave. It is

possible that the legend has a foundation in former actuality. The Mohave themselves have at least the two sets of traditions already cited that former clans or lineage groups went out from Mohave Valley to live in historic Chemehuevi and in historic Vanyumé desert. But certainly Garcés, who traversed this desert in 1776, heard nothing of any such group. It is just conceivable that very soon after Garcés a body of Mohave made the plunge, and perhaps lasted only a decade or two. This would place their career around 1780–1800 or 1790–1810—where indeed it can be calculated possibly to have lain, by reckoning back two 25-year generations to the grandmother of Mukewiune who was born about 1840. But if we reckon more or longer generations, Mukewiune's ancestress and the Desert Mohave episode would have to be put before 1780, and Garcés would have either heard of them or encountered them in the flesh. Besides, we have only a great-grandson's statement that it was three generations back of him, and not two or four. That means that it is possible to concoct a working hypothesis which neatly fits a brief Desert Mohave career into a gap in our documentary record. But this is only a possibility, and in my opinion a very slim (though intriguing) possibility: too many imprecise variables would all have to concur to make the reckoning come out right in time, place, and affiliation.

The near-extermination of the Vanyumé, that is, the end of their existence as a separate entity—for individuals of part Vanyumé ancestry almost certainly are alive among the Mohave and perhaps among the Serrano today—this massacre before the taking over of California by Americans is almost certainly a fact, since no one has reported the existence of such a group since 1850, and since Mohave, Chemehuevi, and the survivor Moha all attest a slaughter. What is uncertain is who perpetrated the massacre, its motivation and antecedents, and the size of the group that was wiped out—whether still a sizable tribelet, a lineage, or a remnant of a few households.

Finally, that special relations once obtained between Vanyumé and Mohave, in intercourse, marriage and kinship, even in ritual singing, and across the intervening Chemehuevi, is also clear; though it would be good to know more about the extent of the connections and influencing and its precise period.

Many of the data here reviewed have been minute, and their examination may have become tedious. Their interest is in revealing a group of certain interrelated patterns of thought which live in a twilight zone between knowledge of fact and imagination. To the natives they are straight history. To us, they have a historical appearance: much of them might be true. But on analysis and comparison it is evident that many of the stories are legend—fantasy history. Ways of thinking create a history, which is none the less interesting for being so largely fictitious. What is tantalizing is that even where there is an underlying foundation of fact—as I am confident there is in several spots here—this has been so worked over by patterns of fantasy that we can no longer segregate it out, except where an external, foreign history (our own) occasionally happens to come to the rescue.

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ABBREVIATIONS

AA	American Anthropologist
IJAL	International Journal of American Linguistics
SWJA	Southwestern Journal of Anthropology
UC	University of California Publications
-PAAE	Anthropological Records
-AR	American Archaeology and Ethnology
UCAS-R	University of California Archaeological Survey, Reports

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