

THE ANTHROPOLOGY OF CALIFORNIA

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[Annotated by D. W. Hymes and R. F. Heizer]

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For many years California was among the regions of North America of which anthropologists knew least. The early traveler touched it, the missionary occasionally left a valuable but fragmentary record,<sup>1</sup> and the resident at times described the native people who were thrown under his observation.<sup>2</sup> But the anthropologist and the trained investigator sought other fields of exploration, and the fact that extensive archeological collections had been formed from one restricted region<sup>3</sup> contributed very little to a knowledge of the general anthropology of the state. Of recent years these conditions have been entirely altered. Several institutions have formed systematic collections or carried on researches,<sup>4</sup> until now the anthropology of the region is nearly as well known as that of most parts of the continent, and certainly presents less obscurities than some. It seems fitting, therefore, to undertake at this time a review of the principal results of study, and of the new problems that these results inevitably open up. It might seem that the student of aboriginal people should be little concerned with the arbitrary limits of a modern political division such as the present state of California. As a fact, however, these limits coincide so nearly with the natural physiographical and ethnographical boundaries, that the artificiality of such a limitation, in an anthropological consideration, is apparent rather than real.<sup>5</sup>

The anthropologist needs no justification for directing his attention first to language. Not only is language recognized as the necessary means to a really exact understanding of the life of any people; it is also the most generally useful instrument of anthropological classification, and one of the aids to historical knowledge which is at times of the most fruitful service when all other methods, even archeology, fail.<sup>6</sup> Particularly in a region like California, where the multiplicity of languages is so marked, and where an absence of other means of segregation and grouping is customary, does an understanding of the linguistic relationships become indispensable.

Thirty years ago the number of distinct linguistic stocks in California was pretty accurately given as about twenty,<sup>7</sup> and Powell's great systematizing work fifteen years later determined the number as only slightly larger.<sup>8</sup> Since then no entirely new languages have been discovered. We may therefore say with certainty that the number of native linguistic families will never be regarded as greater than it is now. On the other hand, recent studies show very little tendency to reduce the total number of stocks. The Shasta and Achomawi have been found related, and this affinity had been at least suggested many years before.<sup>9</sup> That here and there languages, such as Pomo and Chimariko, have certain important words in common with others, such as Shasta, or with one another, is not

necessarily an indication of relationship.<sup>10</sup> It seems that almost every stock in California has at least a few words in common with neighboring or more distant languages, but that such words represent a borrowing or diffusion. This is established not only by the small number of such words, but also by the fact that they are frequently common to more than two languages. It would seem as if words of certain significances had been particularly liable in California to spread as loan words to unrelated languages.<sup>11</sup> Even if some further unifications of languages now considered distinct should in future be made by the students of California linguistics, there is no reason to suppose that such a reduction in the number of stocks will be proportionately greater than elsewhere in the North American continent.<sup>12</sup> It is only necessary to recall that a number of conservative scholars believe, or have proclaimed, the relationship of Natchez with Muskogi, of Selish with Kwakiutl, of Sahaptin with one or more of the neighboring languages, of Seri with Yuman, of Shoshonean with Piman and Nahuatl, to become convinced that any analogous conclusions which may be reached in California will not be special, but will form part of a general reduction in the number of distinct linguistic branches, which is almost certain to take place as knowledge gradually increases.

As regards the minor divisions of language, system has pretty well replaced chaos in California. It can not be pretended that all the dialects are even tolerably well known, but at least the number of dialects has been definitely determined in all regions where the practical extinction of the Indians has not made such a determination almost impossible. We know not only the total number of divisions of each linguistic family at the time of first contact with the whites, but also something of the relative degree of divergence of these divisions. The loose statements formerly sometimes made that the number of unrelated dialects of one stock was often very great, and that these dialects showed a gradual continuous change from one end of the territory of a family to the other, have been found to be entirely erroneous. In most cases the divisions of each family are few in number—with two or three exceptions never above six or eight—and they are usually well marked. Over a certain area the speech is identical or practically uniform. In leaving this area for an adjacent one, an abrupt change to another form of speech is encountered, which in turn is uniform over its entire extent.<sup>13</sup> In many cases the changes met in passing from one of two adjoining areas to the other are so great that it would perhaps be more correct to designate their forms of speech as related languages than as dialects.<sup>14</sup> A number of the smaller families are monodialectic. The total number of distinct languages or dialects in the state, excepting those whose divergence is comparatively inconsequential, was not over a hundred, and more probably about seventy-five. The total of distinguishable forms of speech may have numbered twice this.

Structurally the languages of California are usually characterized by a certain simplicity or transparency. As has been pointed out, there are, however, two regions, one in the northwest and one in the southwest, where this morphological quality is lost, and in which certain other qualities seem to be common to the several languages of the area. It has therefore been possible for some years to distinguish a northwestern, a southwestern, and a large central morphological group of languages.<sup>15</sup> The most recent investigations confirm this classification; but it is necessary not to endow the grouping with too much significance. As each language is studied individually, and becomes more thoroughly known, it is obvious that it must prove to possess certain peculiarities that separate it from all others, even of the same morphological type; and from the standpoint of any given language such peculiarities are of course of more importance, and of greater value to the student, than the more vague similarities to the type, which it is plain can be based only on a few characteristics of either an essentially external nature or of the most general kind. The existence of the morphological groups is evident, but it must also be clear that they are only morphological groups of languages that are unrelated, and that therefore the bare circumstance that a certain language forms part of a particular group, furnishes no understanding, of that language, that is more than skin deep.

As to the significance of the morphological groups, it is clear that the lesson to be drawn from their determination is not a belief in the ultimate relationship of the languages constituting a group, but the emphasizing, by fresh examples, of the principle of territorial continuity of characteristics.<sup>16</sup> This is not the occasion to discuss the much debated question of whether vocabulary or structure is the more reliable criterion of linguistic relationship.<sup>17</sup> However this question be answered, the similarities as yet found between the languages of the three California groups are not of such a nature as to be of bearing on the consideration of their genetic unity. The importance of a proper conception of the frequency and influence of territorial continuity of characteristics is still too little recognized, especially among linguists, though instances of its occurrence are numberless. No one impressed with the prevalence of this historic principle would, for instance, dare to affirm, as eminent men have done, the relationship of the languages of southeastern Asia because they are isolating, or of Japanese with Ural-Altaic merely because both use suffixes in abundance.

The probable cause of the multiplicity of linguistic stocks in California may be said to be becoming a little clearer. The division of many of the stocks into sharply distinct dialects or languages indicates how many of them may have originated by a mere process of divergence, continued until practically all traces of original relationship have now become obliterated. While, as has been said, there seems at present no great prospect

that we shall ever obtain conclusive evidence as to such original unity of linguistic stocks now apparently unrelated, it is clear that if the processes which have more recently been at work dividing original stocks into distinct languages, have been operative in this region for a considerably longer period, as is only natural, there must have been some such result as the gradual formation of what we now call distinct families. In any case nothing has ever been discovered that supports the so-called fish-trap theory, according to which the multiplicity of languages in California is due to the successive crowding, into this more desirable habitat, of waves or bands of unrelated immigrants from less favorable territories, to which none of them were ever willing to return. While this theory is at once simple and plausible, it has never been anything else than purely hypothetical.<sup>19</sup>

It is sometimes thought that areas of diverse native languages can be pretty closely correlated in California with areas that are physiographically distinct. Nothing is more erroneous. True, as there are so many forms of speech, the great majority of them can extend over only a small territory, and it is only natural that a small territory should often be confined entirely to a certain physiographical area. But there are numerous instances where not only linguistic families, but even dialects, run counter to all natural boundaries. The Shoshoneans and Washo have both spilled over the high crest of the Sierra Nevada. The Pomo west of the main Coast Range have an offshoot in the Sacramento valley, and the Wintun of this valley occupy territory west of the Coast Range. The Yurok are in part an ocean people, like their neighbors the Wiyot, and in part a river people on the same stream as their neighbors the Karok. Shoshonean people lived in the timbered Sierra, in the Great Basin drainage, the hot deserts of the interior of southern California, the fertile parts of the coast region of southern California, and shared the Santa Barbara islands with the Chumash. The southern Maidu dialect was spoken in the Sacramento valley plains, in the foothills, and in the high Sierra. The northern and central Pomo dialects were each spoken on the immediate coast, in the open Russian river valley, and in the intervening heavily timbered mountainous redwood belt. In certain instances where languages or dialects correspond to physiographic areas, these physiographic areas lack any separating barrier. Thus among the Yokuts and Miwok the dialects of the level plain of the San Joaquin valley are with scarcely an exception quite sharply distinct from the dialects of the adjacent foothill country of the Sierra; and yet the change from plain to hills is so gradual in some parts as to be scarcely visible. It is clear that in such cases the direct cause of the difference of speech is not the environment itself, but a difference in association and mode of life dependent upon physical geography. In fact it is even going too far to name these dialectic divergences as effects and other factors as causes; we are really only justified in saying that the differentiation of speech

seems to be causally related with other factors, and that these are immediately cultural and historical, and only indirectly physical and environmental.<sup>20</sup>

Much the same is true of the demonstrable relations of culture and environment. There are instances of the effect of environment on culture in parts of California, which could not well be more vivid; and yet these same instances show also the narrow limits which are imposed upon environmental effect by culture and history. On Tulare Lake, in the southern part of the great interior valley of California, live the Tachi and Yokuts tribes. On Clear Lake, in the northern Coast Range, are the eastern and southeastern Pomo. On the Klamath and adjacent lakes, in northeastern California and Oregon, are the Klamath Lake and Modoc people. All three groups of people have developed certain aspects of their material culture in a very similar direction through the use of a material furnished by their lake environment, the tule or bulrush. Not only houses, mats and boats, but clothing, footwear, cradles, baskets and games are made of this abundant and useful material. A glance at a museum collection from the three regions not only seems to reveal a practical identity of culture, but would make it appear that the eastern Pomo and Tachi Yokuts were culturally more nearly akin to each other than to their respective Pomo and Yokuts neighbors and kinsmen. But the moment the social and religious institutions of these people are considered, the resemblances in industries and arts are counterbalanced and as it were nullified. In ceremonies and habits and customs the eastern Pomo are as distinctively Pomo as any other branch of the family; and so the Tachi are as good Yokuts in religion, in beliefs and in social organization, as they are in language. Even on the material side of life environment is not the only causal factor. The Modoc twines his tule basket, the Tachi coils it, because those are the characteristic textile processes of the culture region in which each lives.

Of course even social life and religion will be colored by environment, and their development can extend only within a certain compass given by environment. But this is self-evident. No one, whether anthropologist or historian, has denied the significance of physical nature as a cultural condition; but the attempt has too often been made, sometimes expressly, more frequently by implication, to derive and explain a culture entirely from geography and climate; and nothing is more unfounded. For the sake of argument it may be granted to those who so wish, that in the ultimate analysis everything historical and everything human is the effect of physical nature. But, on the other hand, too strong a protest can not be made against the assumption which is often unwarrantably and illogically made from this view, that the actual immediate specific causes which have shaped the life of any given people can be sought and found in their particular environment. A body of people, neither at present nor at any time

in their history, are ever a clean fresh slate ready to be inscribed by nature. No matter how rude their civilization, it has always a long historical background and is deeply rooted; and it is only upon this complex institutional life that a particular environment can begin to act. In time, no doubt, environment will partially modify all the institutions with which it is brought in contact. But institutions have a life of their own, influence each other, and undergo their own developments and histories. They must be always affected but can never be controlled by nature. Change of environment can destroy an institution by making it unnecessary or impossible, or can be the stimulus which develops a new institution; but in either case something cultural, an existing body of institutions, is present and is acted upon by the stimulus; and this body of culture is in turn dependent upon previous factors that are both cultural and environmental. To look to physical environment for the explanation of cultures is to mistake condition for cause.<sup>21</sup>

The three regions of generally distinct culture which have been recognized in California seem to be substantiated by further researches. Of course any culture-area or ethnographical province is relative. It rarely has sharply limited boundaries. To hold that what is important about it are not its external limits, but its internal center of dispersion, is good doctrine, but impracticable, in most cases, owing to lack of historical material. Thus, as compared with the rest of America, California seems a well-marked and well-defined province. In a broader view of the peoples of the world, its distinctive characters largely disappear, or are seen to coincide with such as are typical of the whole of America. On the other hand, when California is viewed by itself, the northwestern, the central and the southern areas contrast strongly. But the moment each of these three is considered alone, culturally well-defined groups of tribes are evident within it.<sup>22</sup> This does not weaken the value of the recognition of culture-areas. The genus breaks up when we consider species. Even the species seems no longer a unit when attention is allowed to be given to races. But the differences between genera become insignificant when the family and the order are in view. Neither the order nor the species, the race nor the genus, is, therefore, unimportant or unreliable. A biology recognizing only species is a scientific impossibility; but a biology dealing with nothing lower than genera would be equally impossible. The culture-area, broad or minute, has its value, and in fact is indispensable, as a means to a historical understanding of its components; but it has value only so long as its relativity is recognized.<sup>23</sup>

The northwestern culture-area of California may in some respects be considered the most southerly extension of the distinctive and rather highly organized culture which centers on the Pacific Coast north of Puget

Sound. The Yurok and Hupa and Wiyot house is the same in plan as the plank house as far north as Alaska. Immediately to the south, among the Yuki, Wintun, and southern Athabascans, it is replaced by the central California brush or bark hut or earth-covered house. The same tribes of northwestern California are the most southerly among whom a well worked out system of social organization dependent on wealth exists. All through California the rich man was the chief; but only here was every one's standing in the community, and the value of his life and of his children, definitely regulated and expressed in terms of wealth. In this northwestern region, too, is marked the southernmost extension on the Pacific Coast of the prevalence of culture-hero and transformer myths. Immediately to the south, creation myths begin. Nevertheless we may well hesitate before counting northwestern California within the North Pacific coast culture. In general scope and tone, life was at least as similar to that of central California as to that on the lower Columbia or in the vicinity of Puget Sound. In addition, there has clearly taken place in this region an independent local development which has more or less influenced the entire culture. The implements, the ceremonies, the beliefs, found only in this region, are exceedingly numerous, and seem to reach the highest development among the Yurok, the Karok, and the Hupa.<sup>24</sup> The remaining Athabascans, the Wiyot, the Shasta, and the Chimariko, who surround these three more highly organized tribes, belong to the same general culture while lacking many of its most individual features.

In southern California, at least three sub-areas of culture, connected largely with environment, are distinguishable. Unfortunately, the people of what was perhaps the most interesting of these, the Santa Barbara coast and archipelago, were ethnologically extinct long before ethnologists visited their territory. We know of them only from brief notices of travelers and through the less perishable artifacts they have left in their village-sites. As a more or less maritime people, their mode of life must have been quite different from that of the other Indians of southern California, and no doubt their institutions and beliefs also showed much that was peculiar but which we can not even speculate upon.<sup>25</sup>

The people in the fertile and semi-fertile mountain and coast regions of southern California were the most similar, of those in the south, to the central and northern Californians. Their habitat was not essentially different from the greater part of California. Their mode of life is, therefore, naturally also similar to that of central California. In religion, however, especially in the matter of beliefs, there is much that is either distinctive or shows relations with the Pueblo culture. Even the arts are not free from resemblances in this direction.

It is therefore the more surprising that the agricultural Yuman tribes of the Colorado River, to the east of the last group of people, and there-



fore so much nearer the Pueblo region, evidence no great approximation to Pueblo or southwestern life, even though they are in many respects typically un-Californian.<sup>26</sup> Even such of their cultural features as they appear to have acquired through Pueblo influence, as, for instance, their pottery, have a non-Pueblo character. Their religious life is especially distinct, lacking even certain traits which their California neighbors to the west share with the Pueblos and other tribes to the east. In the ceremonies of the Mohave are found no masks, no altars, no painting or carving of ceremonial paraphernalia, the simplest of regalia, no seasonal observances, no societies, and no initiation; and all this in spite of the fact that they maintained some degree of intercourse with the Hopi.

In the great central region of California cultural uniformity is stronger than in the south, not so much through the persistence of certain special positive features, as in a fundamental similarity that is varied only locally. Thus the weaves, the shapes, the patterns and the materials of baskets differ, but basketry is everywhere the most developed and most important art, nowhere replaced by pottery or working in wood. Creation myths and mourning ceremonies vary in form from district to district, but everywhere dominate mythology and public religious expression. Too great a uniformity will not be expected when it is realized how limited the geographical knowledge and intercourse of most of the California Indians were. It is probable that the southern Yokuts did not more than know of the existence of the southern Miwok. These in turn knew no more of the southern Maidu. The southern Maidu may not have been aware that there was such a people as the Shastan Achomawi of Pit River. Again, the Maidu of the higher Sierra did not know more than the easternmost Wintun. These appear to have come in contact only with the easternmost Pomo. The eastern Pomo had but little to do with their western kinsmen on the coast. Whether one traveled from south to north, or from east to west, through the central province of the state, he would, therefore, encounter, in aboriginal times, at least two or three groups of people mutually ignorant of each other's existence; and this condition was probably more marked in north-central than in south-central California. In this respect central California differed as a culture-area from such much more extensive but better interconnected regions as the Plains, or the district of the Great Lakes and Alleghanies; or probably even the north Pacific coast and the southwest, where at least the majority of tribes had some communication with the majority of others. With the restricted intercourse in central California, common cultural traits should be chiefly general, or of a negative character, and local divergences numerous. The degree of uniformity which exists is, therefore, the more significant.

From the first, archeological investigation in California has concerned itself with questions of time more than with those of culture. It was inevitable that this should be so from the sensational if as yet unsubstan-

tiated discoveries of a generation ago.<sup>27</sup> Of recent years there has been rigorous search for evidences of the geological antiquity of man, and positive results from which would have been the more reliable from the fact that the work has been controlled by geologists.<sup>28</sup> It can not be said, however, that more has yet been shown than that there are good prospects for the ultimate establishment of the existence of man in the state at an early period. But a clue is not a discovery, and probability and opinions represent precisely the status of the question which it is desirable to leave behind. Of recent years no one has ventured to assert positively the human origin of the possible artifacts dating with certainty from Quaternary time, or the geological antiquity of finds of unquestionably human origin. Until such an unequivocal statement of faith is made by those most inclined to a favorable opinion, the skeptically disposed will doubt. The work that has been done is encouraging; but proof of the geological antiquity of man in California remains to be made.

Rather unexpectedly, investigation of shell mounds and deposits on San Francisco Bay has resulted in evidences of antiquity sufficiently great to be geologically observable. In a number of mounds on the immediate shore-line, the base has been found to be from three to twelve feet below the present water level.<sup>29</sup> On the other hand, there is at least one case of an extensive shell deposit at a point more than a mile from water and at some elevation above sea level, the presence of which it is difficult to explain except on the assumption that the shore-line has undergone a corresponding elevation.<sup>30</sup> Of course the question at once arises how great a time would be required to effect such changes in a region subject to seismic disturbances.

On its cultural side archeology seems to show above everything else that, broadly speaking, the civilization of California is of some age, and has scarcely changed during the period, perhaps of thousands of years, through which the accumulating finds take us.<sup>31</sup> There is no trace of pottery in former times where it has not been found in the historic period. There are no evidences of agriculture or of architecture in stone. The plummet-shaped charmstones are found chiefly in regions where their use by the Indians has been seen, or explanations as to their employment have been had from the Indians. The straight tubular pipe is as characteristic of the prehistoric as of the present native inhabitant of the state. The peculiar hooked stone adze handle, the large obsidian blade, the perforated stone, the pestle ringed near the bottom, are found buried in village-sites, and in use by the Indians of today, in northwestern California. The more specialized of these forms, such as the adze and pestle, are observed by both archeologist and ethnologist only in this region. It would thus appear that even local cultural characteristics are of considerable age. Scarcely any unexplained types of implements, and no forms of art unpractised at the present day, are found by the archeologist. Even where minor changes have

taken place, they are superficial. The bowl-shaped stone mortar is the commonest archeological find in California. The great majority of the tribes met by the whites did not use such mortars, but a flat slab, or exposed bed-rock, with a mortar or hopper of basketry. Both the prehistoric and the recent people, however, it is clear, lived principally on vegetable food that needed pounding, no doubt acorns above all; and they used the same types of pestles.

Such a close correspondence of the results obtained by the prehistoric archeologist and by the ethnologist investigating present-day conditions, is not a new phenomenon nor confined to California. It recurs in the southwest, on the north Pacific coast, on the plains, to a considerable extent in Mexico, in fact, broadly speaking, over the whole of North America except part of the region between the Mississippi and the Alleghanies. The wide-spreadness of the correspondence, however, makes it particularly interesting and important, as it seems to show either that all American culture is comparatively recent, or that its principal forms, differentiated a long time ago, have been maintained by a strong conservatism.

The physical anthropology of California is yet in its infancy, but whether it will ever far outgrow this stage seems doubtful. Over great parts of the state prehistoric material for investigation is wanting, owing to the prevalence of the custom of cremation of the dead. In other parts the recent people have become extinct without being measured or photographed. In some regions, such as the Salinas Valley, there is neither recent nor ancient material. The map of California will, therefore, presumably always contain large blanks so far as physical anthropology is concerned. At present studies are further restricted through the comparative scarcity of information in most of the surrounding parts. No general correspondences of racial types with cultural or linguistic divisions have been established. In fact, the observed instances more frequently show a lack of correlation. There does not appear to have been any very considerable physical diversification within the limits of the state.<sup>32</sup> Whether a few scattered areas showing aberrant types, such as the long-headed people of the upper waters of Eel River and of the southern Santa Barbara islands, are to be regarded as ethnic islands in which an earlier continuous but now otherwise submerged race has maintained itself to the present in comparative purity; or whether they represent migrations of distinct types from a more remote habitat; or whether they are local developments from a single widely spread and originally uniform type, must yet be considered uncertain.

It may be asked what are the specific problems of the anthropology of California. The most important questions have been outlined in the summary of results that has been given. While something has been done, and

some problems have been solved or brought nearer solution, they have only served, as is always the case, to open wider problems. If it has been determined that dialects do not form gradual transitions, but present abrupt changes, a point is gained. But the question at once arises what the conditions are that have brought about and maintained this state. While the structure of some languages is fairly well known, generally through the study of one selected dialect, there are more of which we have only the most superficial conception. If these less-known languages show resemblances among each other, or to the better-known languages, either in content or in form, we need more information than exists in order to follow out the promising comparisons. If half a dozen shell mounds on San Francisco Bay show varying subsidences below sea level, there are three hundred others, on the shores of the same body of water, whose subsidence should be similarly investigated to make possible a final determination of the age of the culture of this region; and this is only one region of many where similar archeological phenomena can be studied. The cry of the physical anthropologist is for more material—material which is in part no longer obtainable. The ethnologist is beset by the same difficulty. There is not a people in the state whose institutions and religion have been ascertained with such exhaustiveness as is desirable for purposes of comparisons alone. We know that the Maidu and Wintun had certain ceremonies in common. It now appears that other groups, such as the Pomo and Miwok, also practised certain of these ceremonies. We have some idea of the form which these ceremonies took among the Maidu, with whom they do not seem to have been original; but we lack almost all knowledge regarding them among other tribes—and this is knowledge which can still be secured. There is no doubt that something of the history of the aborigines of California, in broad outlines, but in the specific sense of the word history, will be revealed by the continued pursuit of the various phases of anthropology; but what is needed in all domains of the anthropology of the region is more knowledge, more information, and more facts. However gratifying the results of research have so far been, they show only more clearly the greater results that are possible, and emphasize the means by which alone these results can be attained, which is: more work.<sup>33</sup>

A. L. Kroeber

## EDITORS' NOTES AND REFERENCES

1. A. L. Kroeber, A Mission Record of the California Indians from a Manuscript in the Bancroft Library (University of California Publications in American Archaeology and Ethnology, Vol. 8, No. 1, 1908). The one outstanding account of California Indians written by a Franciscan missionary is "Chinigchinich," by Father Geronimo Boscana. This was first published in Alfred Robinson, Life in California (New York: 1846), pp. 227-341. The most recent study of this missionary's account of the religion of the Indians of Mission San Juan Capistrano is by Kroeber, "Problems on Boscana," Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 47 (1959), pp. 282-293. This article is useful in summarizing the literature on Boscana and for its discussion of the two versions (one of which is now lost) of the original account.

2. One such resident was Hugo Reid whose twenty-two letters, under the title "Los Angeles County Indians," were written by him for publication in the Los Angeles Star, 1852. In slightly variant form they have been reprinted several times: by W. J. Hoffman in Bulletin of the Essex Institute, Vol. 17 (1885), pp. 1-33; by A. S. Taylor in the California Farmer [newspaper], Vol. 14, Nos. 19-23, Jan. 11-Feb. 8, 1861; by A. M. Ellis, Hugo Reid's Account of the Indians of Los Angeles County (Los Angeles: 1926); and by S. B. Dakin in A Scotch Paisano (Berkeley: Univ. of Calif. Press, 1939). The original manuscript resides in the Los Angeles County Library. A useful project would be to publish a single annotated edition of the Letters.

3. The author is referring to the Santa Barbara region where extensive collecting for museums was common. Among the early workers may be mentioned Paul Schumacher, who worked for the Smithsonian Institution; H. C. Yarrow, who was a member of the Wheeler Survey Party; Léon de Cessac, who collected for the Musée d'Ethnographie du Trocadero (now the Musée de l'Homme); S. Bowers and H. N. Rust (the latter a onetime member of J. D. Whitney's Geological Survey of California party), who collected for their own interest and for personal gain; and Philip Mills Jones, who collected for the University of California under the sponsorship of Mrs. Phoebe Apperson Hearst.

4. Kroeber here had in mind investigations like those of R. B. Dixon for the American Museum of Natural History.

5. Kroeber returned to this consideration in his classic Cultural and Natural Areas of Native North America (Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 38, 1939).

6. The importance of linguistic classification to historical anthropology was steadfastly maintained by Kroeber, and he continued to make contributions of his own to the subject throughout his career. Studies in classification were almost his only linguistic research during some of the ensuing decades, and were central to the increased linguistic activity of his last years, as witnessed by his linguistically-based study "Recent Ethnic Spreads," Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 47 (1959), pp. 259-281, as well as by a remarkable number of significant papers on the method and interpretation of classifications. His interest was especially in those with a quantitative basis, such as resulted from the new methods of glottochronology and lexicostatistics, or were implied by new typological indices, quantitative methods having been important in his own previous work. He championed the importance of the new methods and contributed to their development with both positive studies and critical advice. The bloc of papers includes: "Linguistic Time Depth Results So Far and Their Meaning," International Journal of American Linguistics, Vol. 29 (1955), pp. 91-104; "Romance History and Glottochronology," Language, Vol. 34 (1958), pp. 454-457; "Reflections and Tests on Athabascan Glottochronology," Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 47 (1959), pp. 241-258; "Typological Indices I: Ranking of Languages," Int. Jour. of Amer. Lgstcs., Vol. 26 (1959), pp. 171-177; "Statistics, Indo European, and Taxonomy," Language, Vol. 36 (1960), pp. 1-21; "Semantic Contribution of Lexicostatistics," Int. Jour. of Amer. Lgstcs., Vol. 27 (1961), pp. 1-8; "Three Quantitative Classifications of Romance," Romance Philology, Vol. 14 (1961), pp. 189-195.

7. Reference here is presumably to the first general work of major importance on California ethnology, namely, Stephen Power's Tribes of California (1877).

8. Powell's contribution is the 1891 classification issued by the Bureau of American Ethnology in its Seventh Annual Report.

9. R. B. Dixon, "Linguistic Relationships Between the Shasta-Achomawi Stock," Proceedings of the International Congress of Americanists, XVth Session, Vol. 2 (1906), pp. 255-263. See also Dixon and Kroeber, "New Linguistic Families in California," American Anthropologist, Vol. 15 (1913), pp. 647-655.

10. This is a suggestion of what was to be later recognized as the Hokan family. By 1925, with the publication of Kroeber's Handbook of the Indians of California, these linguistic relationships had been fairly well established.

11. See, for example, W. Bright, "Animals of Acculturation in the California Indian Languages," Univ. of Calif. Publs. in Lgstcs., Vol. 4

(1960), pp. 215-246. See also Dixon and Kroeber, "The Native Languages of California," *American Anthropologist*, Vol. 5 (1903), pp. 1-26, esp. pp. 16-17.

12. This statement did not envisage, presumably, the reduction to six "super-stocks" that E. Sapir presented in Encyclopedia Britannica (14th ed.), Vol. 5, pp. 138-141; reprinted in D. G. Mandelbaum (ed.), Selected Writings of Edward Sapir (Berkeley: Univ. of Calif. Press, 1949), pp. 169-178. Kroeber, in his summary of the Tozzer volume (The Maya and Their Neighbors, New York, 1940, pp. 460-487) refers to Sapir's 1925 classification as a "brilliant but flimsy gossamer web of prophecies." This discussion (pp. 463-470) is one of Kroeber's most explicit on the matter of what can be inferred from comparative linguistics. Cf. also his remarks in "Some Relations of Linguistics and Ethnology," *Language*, Vol. 17 (1940), pp. 287-291 (cf. p. 289), and in "The Work of John R. Swanton," *Smithsonian Miscellaneous Contributions*, Vol. 100 (1940), pp. 1-9 (cf. p. 7).

13. Well marked, abruptly changing divisions were an important finding. Gradual continuous change would have been expected, and apparently was asserted, on the basis of the situation in such Indo-European language families as Germanic and Romance. That such gradualness was not found in such comparatively small segments of territory was an indication that the divergence must be very old and that the individual cultural-linguistic groups must have been stable for a very long time.

14. Only recently has the full force of this finding been taken adequate account of in American ethnology. The tendency has been to treat the conventionally recognized linguistic units in California without regard to their internal differentiation, and hence in effect as if they were all on a par with single languages. But a number of these, Maidu, Wintu, et al., are not single languages but small language families. This finding, like that annotated above (n. 13), also points to considerable age for the divergence of the cultural-linguistic groups in California.

15. Precise documentation here would be tedious. The principal and original paper is Kroeber and Dixon, "Native Languages of California," *American Anthropologist*, Vol. 5 (1903), pp. 1-26. Cf. also Kroeber and Dixon, *Science*, Vol. 37 (1913), p. 225; and Dixon and Kroeber, "Linguistic Families of California," *Univ. of Calif. Publs. in Amer. Arch. and Ethnol.*, Vol. 16 (1919), pp. 47-118, as well as Kroeber, "Phonetic Constituents of the Native Languages of California," *Univ. of Calif. Publs. in Amer. Arch. and Ethnol.*, Vol. 10 (1911), pp. 1-12. The work was the first systematic typological classification of a set of New World languages, inductively arrived at and with some degree of detail.

Previous typologies had been gross, tending to treat all New World languages as a whole as of one, perhaps two, classes, and having a large component of the a priori.

16. The work of Kroeber and Dixon in this regard may have been the first recognition of the principle of areal continuity of linguistic traits across language boundaries in anthropology, antedating Boas' well known discussions of it by some years, and anticipating the great place which the principle was to have in the theory of the Prague school of Trubetsky and Jakobson a generation later. In European linguistics the principle of such territorial continuity was long a subject of controversy with many ramifications and guises. In American anthropology at the turn of the century it seems to have been accepted as what an ethnologist would expect, to judge from Kroeber's reference to "fresh examples." That the point was not accepted in linguistics is implied by Kroeber's subsequent remark, "still too little recognized, especially among linguists." It is an irony of intellectual history that Boas' controversy with Sapir over remote genetic relationships made well known his advocacy of the significance of diffusion (and hence continuity of traits) across language boundaries, whereas the work of Kroeber and Dixon seems to have been forgotten, although they had enunciated the principle before Boas, and published empirical studies demonstrating it in some detail, as Boas never did. Boas and Jakobson later discovered each other as independent allies, after Jakobson's emigration to this country (see Jakobson, "Franz Boas' Approach to Language," *Int. Jour. of Amer. Lgstcs.*, Vol. 10, 1944, pp. 188-195), yet the statement, "The importance of a proper conception of the frequency and influence of territorial continuity of characteristics . . . ." would have delighted the Prague school theorists of the 1930's even more precisely, had they known of it. Conversely, American anthropologists seem to have remained largely unaware of the Prague school work until Jakobson's coming to this country during the Second World War.

17. Kroeber subsequently stated his position in favor of the primacy of vocabulary in "The Determination of Linguistic Relationship," *Anthropos*, Vol. 8 (1913), pp. 389-401, and in other writings of the period, e.g., in "The Languages of the Coast of California North of San Francisco," *Univ. of Calif. Publs. in Amer. Arch. and Ethnol.*, Vol. 9 (1911), p. 415. The issue had been drawn long before in American ethnology by Powell and Brinton, Powell arguing for exclusive reliance on vocabulary in the Introduction to the classification that bears his name (Bureau of American Ethnology, Annual Report, Vol. 7, 1891) and Brinton advocating the importance of structural evidence, e.g., the Preface of his book The American Race (1891). The issue became notable again as between Boas and Sapir from the 1920's onward. Historical accounts and critical explications can be found in Morris Swadesh, "Diffusional Cumulation and Archaic Residue as Historical Explanations," *Southwestern Journal of Anthropology*, Vol. 7



(1951), pp. 1-21; Swadesh, "The Culture Historic Implications of Sapir's Linguistic Classification," A William Cameron Townsend en el XXV Aniversario del Instituto Lingüístico de Verano (1960), pp. 663-671; Hymes, Review of "Papers from the Symposium on American Indian Linguistics," *Language*, Vol. 32 (1956), pp. 585-602; and Hymes, "Na-Déné and Positional Analysis of Categories," *American Anthropologist*, Vol. 58 (1956), pp. 624-638.

18. Some of the structural similarities recognized did turn out to have genetic significance (e.g., Miwok-Costanoan, Yurok-Wiyot, perhaps Yuki-Yokuts ultimately), but the critical test in Kroeber and Dixon's postulation of new genetic relationships a few years later was to be the interpretation of resemblances in basic vocabulary (see Dixon and Kroeber, "Linguistic Families of California," *Univ. of Calif. Publs. in Amer. Arch. and Ethnol.*, Vol. 16, 1919, pp. 47-118, esp. p. 50).

19. Kroeber does not mean to deny by this statement that representatives of several of the major North American linguistic stocks are present in California (e.g., Algonkian in the form of Wiyot and Yurok; Athabaskan in the form of Tolowa, Hupa, Wailaki, etc.) and that their California presence is to be accounted for as a result of migration. His point seems to be that an over-readiness to accept migration may discourage attempts to seek for relationship of highly differentiated languages. Such a sense for gradual differentiation, for time-depth, as opposed to easy appeal to migration or diffusion to explain diversity, would seem to have been an unusual gift at the time. At least it seems to have been weak or absent among many anthropologists, such as Goddard and Boas. Boas sometimes spoke as if he wanted such a percentage of lexical resemblances for proof of genetic relationship as would have made impossible any but the most obvious connections. Goddard challenged Sapir's postulation of Na-Déné on the grounds of the need to explain the differences (not the resemblances) among the languages; Swanton began a study to indicate the relationship of Tlingit and Haida by giving long lists of words that had no relationship (to show the difficulty of the task). It is as if genetic relationship was seen in the flat, as purely yes or no, black or white, all or nothing, and not properly in depth, a matter sometimes of degree and of hierarchical ordering, such that there would be more evidence for relationships closer in time, less evidence for relationships more remote in time. Sapir's work seems to show him as having seen the matter clearly right from the start, but this statement by Kroeber, made at a time when Sapir was just completing his degree, was unusual for the period, and perhaps the best and only clear recognition of gradual differentiation as a primary, often adequate explanation of contemporary linguistic diversity. (Sapir was Research Fellow at Berkeley in 1907-8, so that some mutual influence cannot be ruled out.) In keeping with this is Kroeber's memorable statement a few years later on the primacy in genetic classification of whatever positive evidence could be mustered

("Linguistic Families of California," Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 16, 1919, p. 113), one subsequently quoted several times in recent work.

20. Full documentation of this point would be overlong. See C. Hart Merriam, "Distribution of Indian Tribes in the Southern Sierra and Adjacent Parts of the San Joaquin Valley, California," *Science*, Vol. 19 (1904), pp. 912-917. On Merriam's paper, see Kroeber's comments in the Preface to Merriam, Studies of California Indians (Berkeley: Univ. of Calif. Press, 1955).

21. This is probably Kroeber's most explicit and longest statement of his views on the relationship of culture and environment. Even his book Cultural and Natural Areas of Native North America (Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 38, 1939) does not contain a more specific avowal of his position than this paragraph.

22. For a survey of this see A. B. Elsasser, "The History of Culture Classification in California," Univ. of Calif. Archaeological Survey Report No. 49, Paper 74 (1960), pp. 1-10.

23. Kroeber's concept of culture areas is most carefully worked out in his Cultural and Natural Areas of Native North America (refer to n. 5).

24. While this conclusion was one which any field investigator who had worked in the larger northwestern area could be expected to arrive at, it indicates further that the idea of "hearth tribes" was already forming in Kroeber's mind. This concept is most specifically formulated in his "Area and Climax," Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 37 (1936), pp. 101-116.

25. That the Chumash in 1908 may have been "ethnologically extinct" is no doubt true, but Kroeber here understates the amount of information on Chumash culture which has been salvaged. The voluminous records of John P. Harrington may, as they are published, make the Chumash one of the best rather than least known California tribes. See also Univ. of Calif. Anthropological Records, Vol. 7 (1942), pp. 1-46; *ibid.*, Vol. 15 (1955), pp. 85-202.

26. For a full exploration of this problem see W. D. Strong, Aboriginal Society in Southern California (Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 26, 1929); Strong, "An Analysis of Southwestern Society," *American Anthropologist*, Vol. 29 (1927), pp. 1-61.

27. This reference is probably to the famous hoax called "the Calaveras skull" and to subsequent finds believed by their discoverers or

proponents to be coeval with the deposition of the auriferous gravels of the Sierra Nevada. For citations see Heizer in Univ. of Calif. Arch. Survey Report No. 2 (1948).

28. Kroeber has in mind mainly one geologist, John C. Merriam. For Merriam's published writings on this subject, see reference cited in note 27.

29. Specifically the Emeryville shellmound, first excavated by M. Uhle and later by W. E. Schenck, and the Ellis Landing shellmound, excavated by N. C. Nelson.

30. A number of shellmounds lying many miles from salt water are known. It is unnecessary to invoke in these cases elevation of the land, but rather transport of shellfish from the shore to interior villages. For citations see Univ. of Calif. Arch. Survey Report No. 9 (1950), p. 12.

31. Kroeber repeats this in his "Archaeology of California," Putnam Anniversary Volume (1909), as well as in the Handbook of the Indians of California (Bur. Amer. Ethnol., Bull. 78, 1925). A more hopeful note is struck in his "Prospects in California Prehistory," American Antiquity, Vol. 2 (1936), pp. 108-116.

32. What differences in the available data, based upon the living and skeletal material, could be determined are given in E. W. Gifford, "Californian Anthropometry," Univ. of Calif. Publs. in Amer. Arch. and Ethnol., Vol. 22 (1926), pp. 217-290.

33. Now, in 1962, some fifty-four years after this article was published, it may be noted that a great deal of work was done. A steady flow of monographs dealing with California ethnology continued to appear in the University of California Publications in American Archaeology and Ethnology series. The ambitious "Culture Element Distributions" survey was brought to completion by the publication of the twenty-sixth contribution in 1950. This series was largely published in the Anthropological Records of the University of California. Intensive archaeology began in 1948 with the formation of the Archaeological Survey at Berkeley. A good deal of linguistic work was done over the years, much of it still of value, especially in cases where the language or dialect has since become extinct. A systematic linguistic survey of California, employing modern descriptive techniques, was begun at the University of California, Berkeley, in the Department of Linguistics in 1953. Now, after almost exactly two centuries of Caucasian settlement, the aboriginal ethnology is finished; the Indian languages are still in part preserved, and the archaeology is incomplete and large segments will forever remain unknown as a result of increasing activity by unscientific collectors and expanding agricultural and urban development.