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1. WHAT ETHNOGRAPHY IS

I have been asked to preface these ethnographic interpretations by a statement of what ethnography is.

By usage rather than definition, ethnography deals with the cultures of the non-literate peoples.

The time is past when peoples and races could be confounded in a scholarly context. Race is a biological concept, people a social concept. Occasionally a race and a people coincide; sometimes they overlap; always they are distinct aspects of human populations.

In a strictly social sense, peoples are the units into which the population of an area is grouped—its bands, tribes, nations, or stocks. These may or may not have native names; their cohesiveness may be weak and transient or strong and enduring; but always there is a degree of cohesiveness—of in-group recognition—by definition. The determination and classification of the sociopolitical groups of an area are necessary for identification of whose history and qualities it is that are being studied ethnographically. In themselves, however, such identifications have a relatively narrow and special factual interest: they lend themselves readily to speculations as to "origin" (which usually is undiscoverable with certainty) and on connections, but hardly to broader conceptual formulations. Keane, Radcliffe-Brown, and some others have tried to make historical ethnology consist wholly of such searches or speculations as to tribal or national identities. This is a gratuitous view and a depreciatory one. It leaves culture out, in order to reserve it to the seekers of laws of social structure.

The culture of a population or society is evidently a matter of greater import ordinarily, a larger and more significant thing, than the origin and migrations of the population, its alternating coalescence and redivision into tribes, nations, or other groups.

The culture is the distinctive ways of behaving of a group and its distinctive products—its customs, beliefs, ideals, and achievements. Its culture is what a society can leave behind it after the society is dissolved away or absorbed. It is what the society contributes to the history of the world. Essentially, or most significantly, ethnography is concerned with cultures, even if mainly they be those of a lower order.

In principle, the cultures of the world, past and present, form an interconnected continuum, and it is somewhat arbitrary to dichotomize this continuum on the one specific issue of whether particular cultures do or do not have writing, and to call them civilized or uncivilized accordingly. However, our minds are so constituted that they like such big, sweeping bisections; and while it is of course ordinarily less sound, because less in accord with the usage of natural science, to base classifi-

cation on one characteristic than on many, it must yet be admitted that if we do limit ourselves to one criterion, it is hard to see which distinction within human culture would be more significant than that of literacy.

At any rate, it is with writing that history can begin to be recorded, and some degree of the practice of historical recording does usually soon follow. History, as actually practiced, can be defined—not too amissly—as that branch of learning which deals with written documents about those actions of men which are also social events or result in general conditions.

Ethnography, on the other hand, does not find its documents; it makes them, by direct experience of living or by interview, question, and record. It aims to grasp and portray sociocultural conditions: merely summarized at first, and often moralized, as by Tacitus and Herodotus; but, with luck, proceeding in some degree to treat of recent individuals also. It can occasionally deal with persons who lived as far back as Napoleon is from us, but probably never as remote as Luther, because without documents history rapidly dissolves into culturally patterned fictive creations. We call these creations legends and myths, and they have ethnographic and almost always some literary value, but their historical validity diminishes with each generation at an exponential rate.

II

The result is that ethnography primarily portrays conditions of a moment, or culture seen synchronically, as a people's culture is organized into more or less coherent patterns. The staticness of such a view is transcended in two ways: microscopically and telescopically.

The microscopic approach adds "depth" to the basic culture patterns, embroidering or enriching them through interest in persons and their motivations, and in pertinent individual events. This predilection also enables short-term changes of culture to be observed and partly analyzed in terms of individual influences. With individuals and motivations deliberately included, a color of personality psychology is imparted to the results; which, however, most psychologists regard as at most a marginal tincturing by their subject. The observing of individually motivated actions also allows of a causal or dynamic interpretation being given to the flow of culture. Such a dynamic presentation can be called "historical" because it records change as well as pattern. It is however a very brief-term history that is obtained in this way, because of the fallaciousness of interpersonal memory unsupported by written records. Nor may such a one-generation-long "history" be taken as typical and repetitive, because while basic human nature is undoubtedly repetitive, the very change observed results in altered culture patterns, in which the repeating human nature must operate differently; not to mention broad superpersonal influences—environmental, populational, contact with cultures of different antecedents—which may never be permanently disregarded. Every historian knows intuitionally that no one generation or segment of history can ever be accepted as being exactly repeated elsewhere, or even of having its particular combination of activating processes repeated.

Instead of refocusing from the culture of a society to the persons involved in it, the telescopic approach gathers in other cultures and compares them with one

another as cultures, with emphasis both on exact feature of pattern ("typology") and on occurrence in geography ("distribution"). In this way a degree of long-range historical reconstruction can be effected for specific items of culture. However, features of culture are potentially so independent of one another—paper manufacture is transmitted by China to Europe, but type printing is not—that these special histories of fragments of culture add up very slowly and imperfectly to generalized histories of whole cultures. It is true that typology and distribution will yield probable classifications of cultures which carry considerable historical implications. But historians are wary of or discontent with mere implications, and scholars intent on demonstration of casual mechanisms are impatient of classification.

III

It has become clear that in this matter of a reconstruction of the larger and long-range movements or developments in global human culture, the ethnographer or ethnologist needs the help of the archaeologist. The ultimate purposes of the two are the same: to discover the history or evolution of culture; but their instruments and methods are quite different. The archaeologist also operates with typologies and distribution, but he adds a third procedural mechanism not open to the ethnologist, to whom it is an end but cannot be a means: namely chronology, or distribution in time. Relative distribution in time is what the archaeologist can hope to discover by his own archaeological techniques; but these results are then often potentially convertible into absolute time, either by cross-secting with inscriptions or the datings of history, or by calling in the aid of techniques from the underlying botanical, geological, chemical, or nuclear physical sciences, such as coördinated counts of tree rings, varves, chlorine, or carbon-14 residues.

The archaeologist's data consist of preserved objects, especially cultural products, plus information on their position, topologically and geographically. Both these sets of data are objective, and if sufficiently numerous and precise they are incontrovertible. They also contain subjectively apprehensible information, for instance on style qualities and style changes. And they allow of inferences on the conversion of space relations into time relations of relative sequence. Archaeology accordingly is by nature strong where ethnology is weakest—in the objectivity of its data and in direct historicity—although it is restricted to securing immediate data on only part of any total culture. The two approaches supplement each other so gratifyingly because they approach a common purpose with quite distinct methods.

IV

What the ethnographer is alone in doing within the "social sciences," and almost alone in anthropology as the word is used in English, is two things. He tends to envisage his problems or objectives holistically; and he prefers to acquire his data by holistic contact, person to person, face to face, by word of mouth plus his own observation. This last is not ordinarily true in history, study of government, economics, or sociology, where already compiled documents, censuses, codes, trade reports, and such are the usual primary sources of information. Sociology alone supplements these in considerable degree with the questionnaire, a document

elicited in individual manifold, but extracted with minimum or no direct personal contact, and necessarily subtending a rather narrow angle.

As we have already said, the ethnographer makes his documents as he works. He knows their occasion and context, he can more or less judge their bias, he can extend or reduce the scope of his inquiry, he can return with fresh insight to recommence it. In entering upon an uninvestigated people or topic, he is literally enjoying satisfactions of discovery and charting new terrains of knowledge—activities at least akin to creativity. Even where he has had predecessors, there usually remain new extensions and supplements to be made. The choices whether to turn here or there, where to extend and where to intensify understanding, are manifold, are exhilarating like all discovery, and call for alert mobility and generalized resourcefulness rather than steady, orderly progression, though considerable overarching patience is also a desideratum. If inquiries are pursued with tact to avoid engendering needless fears or hostility, they can go on almost endlessly. Novel situations are always turning up, and there are ever further informants to be tapped who are specialists in some domain of their culture, or who can ably express their world view.

Just as there are no natural stopping places, once one has embarked on uncovering an unknown language, until its limits are reached, so with a culture. The freedom of the person-to-person approach somehow leaves the investigator always aware of the larger whole in which the items he has so far acquired are imbedded. And while he senses that he may never attain complete control of the totality, its relevance is constantly obtruded. Thus the method of acquiring information, and the tendency to deal with cultural wholes, go hand in hand.

V

In recent decades there has been an inclination to shift ethnographic studies from Tikopians, Hopi, Nuer, and other nonliterates and primitives to linguistically or geographically defined communities within literate civilizations. It is evident that this is a transfer of method: the inquiry remains face-to-face and is applied to a totality, but the totality is a sociocultural unit or subunit.

As to the advantage or disadvantage of the transfer to new material, the shift has in its favor that it extends the ethnographic method to fields where it may perhaps always remain subsidiary to dealing with documents already extant, but to which it might certainly make new contributions, whether these be only supplementary or ultimately become primary also. Biologists take for granted that primitive as well as advanced forms, and advanced as well as primitive, must be considered in their endeavors to understand life, even though many techniques and particular problems may differ in the two domains. It would be equally unfortunate to admit only objective documentation as serving understanding of advanced societies and cultures and only subjective observation and recollection for the nonliterates.

Yet it is plain that face-to-face community studies alone will never suffice for an understanding of a culture as large as that of China, of a nation like the French, or even of a smaller one like the Danes. A thousand towns, communes, or parishes known in this way would not add up to knowledge of France or Denmark. Though

they might present precious concrete detail and illuminate numerous humble aspects of life remaining subliminal in the documentary records, they would reflect most incompletely, if at all, the larger heightened elements, the most pervasive and general aspects, of the total culture—its national power, leadership, guidance, genius, resource, and achievement.

To this may be added that not only these greater things but the whole of a society and culture are founded in and produced so largely by its past, that an adequate understanding of its present necessarily involves knowledge of its past. To this past, ethnographic primary investigation, being essentially synchronic in its approach, can attain only indirectly and very imperfectly.

There would be something insensate in the notion that any number of "community studies" made by ethnographic field methods could add up to more significant knowledge of France or Denmark—or the United States—than is already available from the social scientists, officials, intelligentsia, and general citizenry of these countries. I do not believe that any of the social anthropologists or sociologists now making such studies delude themselves with the belief that their studies, even in the aggregate, will bring about a revolution of man's understanding of his larger social selves.

Why then the drive to conduct such studies, if at best they only supplement or illustrate larger generalizations obtained in other ways, by adding intimate, close-up, and detailed pictures, of tiny samples of greater fabrics, which it is hoped they may illustrate typically and concretely?

As a matter of fact, to date many more such studies, and allied acculturation studies, have originated in dissertations for the doctorate than have been produced by the scholars who direct and accept such dissertations. The studies can usually be made with little travel and little cost, in one's own country and often in a community that one knows or has lived in. They can ordinarily be conducted in English or among some ethnic minority which one can reach with a language like Spanish, or through American-born Orientals. If the group is an immigrant one, it is not necessary to know the culture of Sweden or Portugal or Greece, but only as much as the immigrants themselves can impart about their former home customs and values—because the subject of a study is not an alteration of the culture of Sweden, but how a group of Swedish peasants, or Greek sponge-fishermen, have adapted to American life. The latter is already familiar; and of what was Swedish or Greek only so much is relevant as the elders in the Greek community recall by way of contrast or have preserved. The requisite equipment of knowledge or method on the part of the investigator is really quite modest. A certain amount of new information is readily acquired, and has the merit of being relatively novel to most readers and of being given a degree of coherence through the fact that it depicts the way of life of a group selected for retaining a measure of coherence. The investment and risks of time and knowledge are really small, but the returns—apart from a probable professional degree—are important to the student in that he enters his profession with a record of "field work done," of having used the face-to-face method of inquiry on a group. Students are quite correct in appreciating the value of having had this experience: it is the crucial hallmark of the anthropological investigator.

VI

It is rarely that community studies get above the descriptive level—a fact which to be sure they share with ethnographic studies of primitives. There is however one difference.

An ethnographic description of a primitive people may be performed out of intrinsic interest or a sort of infatuation. Yet it is also a brick that gets built—by others if not by the author—into a structure, namely, the record and understanding of all human culture through time and area, which makes it potentially more than just another tribal ethnography. This is an objective which I want to say more about later; but it is at any rate an end that aims to take us out of ourselves, our day, and our own puddle. In the community study the main result recorded is mostly how others become more like ourselves; which seems an ethnocentrically tinged interest for an anthropologist. There may be compensating motivations and values which I am not the one to discover. If so, I hope some proponent will become eloquent about them.

Acculturation studies in particular, at any rate as they are conducted in America, seem particularly monotonous and depressing, equally so whether the acculturees are ethnic minority immigrants or ethnic remnant aborigines. These unfortunates always emerge from the process as bottom-level members of our own society and culture. Perhaps they are lucky at that: most of the immigrants seem to think so, if most of the Indians do not. Yet each study appears to be the repetition of a principle akin to the one that when a bulldozer meets the soil that nature has been depositing for ages, the bulldozer always and promptly wins.

It is often said that the switch of interest away from primitives to minorities and communities in civilization is due to primitives fast losing their primitiveness or dying out, the world over. This diminution is no doubt actual, but it does not seem to be the main reason for the growing neglect of old-fashioned ethnography. It costs no more in time and money to study the native culture of an Indian tribe as it survives in the memory of the older members than to study the acculturation of their children and grandchildren, or the facet of our own civilization exemplified by a neighboring white community or sect. But the native study does require, as a precondition of success, more knowledge of ethnographic background, and more skill in eliciting significant data instead of expectable obviousnesses. Such knowledge and skill are becoming rarer among our anthropological students. And why should they trouble to acquire them when they receive the same professional certification and recognition for more commonplace knowledge and thinking? Ethnographic descriptions can also be very dull reading even for an anthropologist, if he does not know enough to fit them into an intellectual structure.

VII

This attitude in turn is strengthened by the increasing acceptance of anthropology as a regular member of the social sciences instead of remaining a hard-to-place oddity or an assemblage of somewhat queer specialties. By and large, social scientists and their public have assumed that their studies were useful because they were practical, that they made for the more efficient conduct of public and private

affairs. Often they have even intimated that if this were not so, sensible people would not waste effort on social studies. But this was not the least true of ethnological anthropology in its beginnings, when it sprang out of a combination of naturalistic and humanistic impulses. However crude its efforts may sometimes have seemed to naturalists and perverted to humanists, those efforts were obviously actuated by intellectual curiosity, by a wish to expand the horizons of understanding, not by any seeking for immediate utilities. With such beginnings, the acceptance and swamping of ethnology and the rest of cultural anthropology by social science aims and attitudes could only result in a dilution, in a lowering of sights from former targets.

From its inception the Social Science Research Council has excluded from its grants support for studies in ethnography, in archaeology, in languages and linguistics, as it has essentially excluded ancient history, non-Occidental civilizations (until the rise of "areal programs"), the development and theory of arts and literatures. Most of these fields have received some encouragement, though out of much more slender resources, from the Council of learned societies devoted to the humanities; but ethnography has been omitted by both; and except transiently, by the Council for natural sciences. There has been support for ethnography from universities, museums, and special research foundations; and it may perhaps be properly maintained that no vital branch of learning should be too much dependent upon donations. I make no complaints: ethnography survives; I believe it will come into its own when it develops the needful fire and initiative. But I also believe the Councils must accept partial responsibility for largely overlaying it, during the past three decades, with forms of allied research activity whose targets were more immediate and intellectually lower.

VIII

Some special relations of ethnography to fields of study wholly or partly outside the social sciences deserve consideration.

Psychology has always been considered wholly nonhistorical in its aims and methods, which are avowedly concerned with process.

Social psychology seems to have grown partly out of an interest in suggestion and suggestibility, imitation and mob influence. It was developed in France by jurists like Tarde and LeBon, in America about equally by sociologists and psychologists, and is still shared here between the two disciplines. It seems characteristic of social psychology that the societies it ordinarily deals with are not units occurring in nature or history, or determinate by definition, but *ad hoc* units, varying with the situation. This would mean that the real topic considered is interpersonal influences, multiple or singular, rather than the relation of the individual to the social unit as a natural or historic phenomenon.

The largest unit which straight psychology treats holistically is the organic individual. Even this unit is usually dealt with only in clinical or psychiatric or personality psychology, which differs from all the rest of psychology in being nonexperimental and nonquantitative. It is essentially this personality psychology that has been injected into the study of culture in recent years.

If this is the essential situation, then psychology, after detaching itself from

philosophy and setting out to be an experimental and quantitative natural science dealing with precisely defined and narrow fields, strictly controllable, has now let itself be led to a considerable extent into the realm of social science, whereas the largest unit with which it, qua psychology, actually operates, still is the individual person.

I do not understand this contradiction, except that the given material of psychology evidently is unusually recalcitrant to fertile natural science treatment. The yield of ore seems to run low per ton of effort. The results do not appear to add up to much wide significance—in situations selected for their testability; especially not accumulatively. On the contrary the one method of psychological thought that has proved originative and fruitful, the Freudian, is untestable scientifically, the line of division in it, between what is probable and instrumentally useful or merely speculatively possible, having to be made by common-sense human experience.

Incidentally, Freud's and his successors' ventures into the causality of culture are now generally accepted as in the second category. On the other hand, his generally accepted insights do bear at least on the life history of the individual. It may be argued that so repetitive and minimal a "history" as this, is not really history at all, in the sense that we can speak of the history of a species, or of life on our earth, or the history of the earth's crust, or of the solar system. There is certainly a difference here. And if my doubts are accepted and sustained—that a life history is not significant history but is exemplary definition of recurrent types—like cells or crystals or pebbles—then the reason for psychology remaining wholly nonhistorical is the sound one that the historical potentialities of psychological phenomena already find expression in documentary history, ethnography, archaeology, and the social sciences generally so far as these do not aim at being "nomothetic."

Only, in that event, the formulation of the relation of psychology to natural and to social science is in current need of revised definition.

IX

Linguistics is almost always reckoned a humanity, although in precision and rigor of techniques and breadth of fundamental method it probably transcends all other humanistic and social studies. At that it is only now manifesting the first impulses toward quantitative analysis; it has mainly operated with qualitative precision of definition of pattern, sharpened in recent decades by successful use of the principle of contrast. I accept Hockett's definition of linguistics as a natural history of languages. It uncovers patterns that exist in speech, although for the most part covertly and unconsciously, and that allow considerable degree of predictability as to elements present but not yet discovered.

Philological linguistics is distinguished from general linguistics by restriction to concern with written languages and by association with literary problems; also, in the past, often by normative interests. It also began, nearly two centuries ago, to work out the comparative history of the Indo-European languages, and later of other ethnic groups having each a common speech origin.

General linguistics is interested in the pervading principles to be found—with

enormous variation in detail—in all the languages of mankind. Every language is therefore an exemplification, to greater or less degree, of particular or novel patterns of structure, quite independent of the number of its speakers, their importance in history, their literary or other cultural achievements. On examination a language may prove to be as distinctive from everything previously described as a kiwi or platypus, or as unexpectedly out of place as a marsupial opossum in America. This comparison may validate the appellation of linguistics as a "natural history."

The recording and description of never-before-written languages had of course to be devised gradually, much as were ethnographic approaches and renderings. It pays to remember, however, that the beginning had to be made by starting with the scripts and grammatical concepts of the "high" languages of philology, and that analytical originality of an elevated order was involved in the first grammars extricated, those of Sanskrit and then of Greek. All subsequent language descriptions were patterned on these—if soundly done, with reformulation of pattern instead of its mere repetitive application.

The description of previously undescribed languages carried the process a step farther, especially when all the speakers were illiterate; and the outcome has been a reconceptualization of the range of elements and forms of language, which has allowed philologists to distinguish better between what is intrinsic form and what the caked scum of idiosyncratic history, in the presentation of the languages of high civilization. Thus general linguists and philologists are less far apart than sometimes still seems to them.

X

In the gathering of data on newly discovered languages, ethnologists took part with ardor from the first. From the time anthropology was first conceived as a unified field of inquiry by Tylor and then by the organizing founders like Powell and Brinton in America, language has always been recognized as an integral part of this field, specially associated with ethnography; and cultural anthropologists like Boas and Sapir have been influential in pure linguistics, and their students after them. Linguists on their side can fairly be said to recognize language as within culture, though as constituting a domain which can be autonomously treated.

Reconstruction of an earlier or "original" form of several languages now diverse but still related has long been practiced on Indo-European, then on other families of Old World languages of which at least some had been written. Such reconstruction is now in full swing for a number of aboriginal families in America, all of them unwritten by their own speakers. Thirty years ago I called attention to the fact that by contrast, reconstruction of former cultures was often being frowned upon in ethnography. The reason for the difference is more easily seen now: the patterns of language are sharper and can be defined with less residue, perhaps because they come more interwoven in a semiautonomous nexus. Geographic and social environment impinges more heavily on culture, and this therefore normally contains more nonconformable components.

Linguists are also more rigorous in not commingling synchronic and diachronic treatments. Both treatments are legitimate to them; but the problems are differ-

ent—statically descriptive as against comparative or dynamically historical—and therefore best kept separate. Ethnographers would do well to observe the same distinction throughout—we do in some measure. It is enough to recall for instance how infantile the attempts of Cushing seem today who thought he could explain the causal origin of almost any interesting phenomenon he encountered.

Bloomfield is still being followed by nearly all linguists, at least in America, in "casting out mentalism." Not that anyone claims that there is no psychology involved in language, but that the business of the linguist is the discernment and interrelating of the forms of languages, and that the causal explanation of these in psychological terms is the business of psychologists and is best left to them. This principle has been much less straightly announced and followed by ethnographers for culture, but tacitly it is being observed considerably. Few ethnologists today would account for cultural forms by unblushing appeal to psychology as Tylor and Frazer did. We might be more likely to bring "human nature" into a situation to suggest where one cultural form finds an inherent limit, or why another conceivable form fails to occur at all.

Psychologists on their part seem quite unable to bring their resources to bear to achieve anything with either linguistic or cultural forms. For instance, a broad and positive result of modern linguistics is that the basic units of speech are the phoneme and the morpheme. The syllable is recognized, but secondarily and incidentally. On the other hand, in the historic devising of phonetic writing and in metrics, two activities which are perhaps essentially cultural though resting directly on language in that they operate with linguistic forms, the syllable is important but the phoneme and morpheme are incidental. These are positive findings from a growing series of historically attested cases, with few or no exceptions. The reasons are not known. Linguists and ethnologists could guess at them but do not. Psychologists might conceivably know or devise an attack on the problem; but they have not done so in my cognizance. In fact, modern psychology seems unable or unwilling to come to grips with any established phenomena in the wider history of mankind. This is of course in line with its genuine antihistoricity, its feeling that to become scientific it must avoid traffic with qualitative forms. It is only within the very last few years that efforts have been made from psychology to approach language, and they are still limited.

XI

Another element that general linguistics is trying to eliminate as far as possible is meaning. It is recognized that this cannot be wholly dispensed with—separate elements that are homonymous would be treated as one, if it were omitted—but meaning is left out where it may be omitted. This is again a sharpening of objective and method. Meaning is not denied of course: it is only treated as intrinsically irrelevant to the working out of relations of linguistic form. This ignoring obviously cannot be continued indefinitely; sooner or later there will have to be reassociation of aspects that occur associated in the phenomenon. But meanwhile their discrimination makes progress possible.

Here again ethnography, though dealing with analogous and related phenomena, lags behind. It is not even clear precisely what the cultural counterpart of lin-

guistic meaning is. It is obviously somewhere in the range of *use, meaning,* and *function* as these were distinguished from one another and from *form* by Linton and then by Homer Barnett. But no student of culture seems as yet to have discovered how to make really operational and constructive use of this fourfold discrimination. At any rate, we do not know what we would cast out if we wanted to emulate the linguists. My unsure guess is that it would first of all be *function,* which looks suspiciously like a covert or ulterior *purpose;* perhaps cultural *meaning,* also, whose boundary with function runs somewhat fluid.

A last new method of linguistics that as yet has no counterpart in ethnography is glottochronology, or lexicostatistics used as a measure of time lapse. This is still under fire from most general linguists; but they are slowly giving ground, and in a limited way some of them are beginning to use the method. The procedure rests on the assumption that the rate of linguistic change is constant, or perhaps that among the factors which produce linguistic changes there is one factor that is a constant. This working hypothesis has not been proved, but the assumption has the merit of arousing interest, so that in five or ten years it may be proved or disproved, or we shall know better how far it is true. There may be too many conjoined variables to allow the absolute age of separateness of languages to be reliably computed, as has been hoped by some. But it seems already reasonably sure that the method will yield at least a relative time classification or phylogeny within groups of languages visibly akin as members of a family.

The analogous assumption might profitably be made for culture, if anyone could devise a technique. The devising would obviously be more difficult. Language is a smaller part of "total culture," more narrow in the range of its phenomena, and more autonomous than the remainder; its forms, as already said, are sharper in definition; and certain operations are therefore more easily carried out on linguistic phenomena. The more reason why students of culture should watch its developments and successes.

So much for linguistics; except for one general remark. It is clear that it began its separation from philology by making discriminations of subject and method and adhering to them, and that it has continued to find and observe new distinctions as it progressed. If such is a road of scientific development, it carries a suggestion for ethnography and all other studies concerned with culture: to distinguish conceptually with clarity the social aspects from the cultural within the sociocultural range, and to be ready, whenever the phenomena or the situation allow, to deal separately with them for greater intellectual advance. Resynthesis can come later, and will come if it is feasible; but there can be no resynthesis made of elements or qualities imperfectly discriminated.

XII

I have said that I consider ethnography a natural science dealing mostly with phenomena usually assigned to the humanities. More exactly it would be a form of natural history, or scientific description, narration, and classification. In precision, too, as long as ethnography is restricted to phenomena from nonliterary cultures, it cannot be said to deal with wholly humanistic materials—only with

data which, if they had emanated instead from literate cultures, especially certain ethnocentrically approved ones, would then be construed as humanistic.

Considering myself half humanist in spiritual ancestry and proclivities, I proceed to try to define the relations that exist and should exist between cultural anthropology, of which ethnography is probably the core, and the humanities. In principle I see no difference: We are all concerned with the products of human culture, big or little, developed or rudimentary; with special emphasis on creative products. As part of this concern we deal with the discernment, discrimination, formulation, comparison, and history of values. I hold that such concern with values is potentially a natural history of values, and actually becomes such as soon as the base of consideration is broad enough to be nonexclusive as regards values as phenomena. What that attitude however does exclude is normative activities which aim to exclude, to withdraw certain phenomena from analysis and comparison. I suspect that more humanists than is ordinarily thought would subscribe to some such creed as this. In any event, we are not far apart.

The natural history to which I see ethnographic activity related is theoretically that of the human animal. But this being imperfect, fragmentary, and disturbed, and human history substituting for it inadequately by limiting itself, to date, unduly to the documentary, the natural history of the animal kingdom must partly take its place. Considerable as is the distance between a single species with speech and culture and all other animal species without, the real rapport of ethnography seems to me for the present to lie with animal structure and behavior rather than with human psychology in its momentary disappointingly limited phase.

The difference in this respect between zoölogical science and psychology appears to be one of degree of orderly development and organization. Zoölogy began with description and went on to classification on a broad base: configurative, anatomical, and behavioral description and classification of the entire kingdom so far as known. Knowledge of physiological processes, which are largely internal, followed later, and reached full development with laboratory experiment and linkage with chemistry. Then came the revolutionizing idea that the whole array of life did not have to be viewed only statically, but could and must be seen dynamically, diachronically, also; what had been mere classification therewith became an evolution as well. But without the classification having been worked out before—and as a "natural" classification, that is, one derived from and conforming to the totality of the phenomena instead of from ideas derived independently of the phenomena—without this previous taxonomy, Darwin would have been unable to substantiate the origin of species as effectually as he did, nor in fact could he have thought about it fruitfully. But with a vast and ordered array of information standing ready when the nineteenth century finally got ready to venture on a diachronic view, the issue was swiftly decided once it was seen that the old static classification was not destroyed but was largely conformable to the new dynamic point of view. It was like a nation's marshaled army going over to a revolutionary cause.

Then, when a generation later the idea of genetics and the first evidence for it were rediscovered, there was the same body of ordered biological knowledge—considerably increased, in fact—available as a foundation and soil for the new science to grow in. Besides its novel point of view, genetics introduced experiment

of a new though simple type; yet all its successful proliferation rested upon the previous, ordered body of knowledge of biological forms.

Even the Darwinian revolution could not have happened in classical times, in spite of Aristotle and Lucretius having entertained the germinal idea, because the relevant knowledge was too meager and miscellaneous, so that the seed was unable to sprout even in its authors' minds.

Possession of an organized corpus of knowledge of phenomena is the precondition of any soundly growing science. The first step after the acquisition of new knowledge is its ordering or classification. In fact the very ordering will induce further acquisitions, by revealing what seem to be gaps or, on the other hand, growing points. We need not worry too much about interpretation: it will always be attempted. Much of the first interpretations will be imperfect; but they will grow cumulatively in soundness and insight with the body of information.

The distinction is false, both in natural science and in humanistics, between a higher theoretical understanding and a lower order of merely informational knowledge. The one does not grow without the other. True, new ideas are rarer than new facts, and harder to develop or acquire; but there are more facts, and more are needed. To originate a new idea, to observe a novel distinction, is no doubt a more creative act of intellection, and one we all would take more pride in having achieved, than to find and report an indefinite number of new phenomenal items. But science is the interaction of the two things and therefore needs both. And I am of course speaking only of natural science, not of poetry, philosophy, or even mathematics.

XIII

How about the corpus of knowledge assembled by ethnography?

It is obviously smaller than that of zoölogical science. But there have been fewer workers, and some of those more given to ideological distraction than is wont in zoölogy. But it is a growing body of information, and increasingly coherent. We cannot be too boastful about it but need not be apologetic. When I recall the knowledge available in my own field of North American ethnography as I entered it around the turn of the century, there were far more gaps than content. Our total positive knowledge now is several times as great, and the areas of total absence of exact information are much shrunken. Continuity of knowledge is much more adequate. Ordering and understanding have kept pace, whether in terms of segments of culture, of spatial distribution, or of historic developments and successions. In this last domain, once American archaeology found itself and faced the time factor, some thirty years ago, progress has been phenomenal.

In one respect we lag behind the biologists, owing to a difference in our subject matter. The history of life is like an ever-branching tree. Once life forms have differentiated beyond a certain narrow limit, they cannot reunite. The direction of their evolution may alter, but its continuity is irreversible. A form may die out, or persist, or branch into several, but it does not merge. There are millions of lines of descent in the history of life, but each has been neat and distinct, if we can only recover the facts about it. But in culture we get constant fusing as well as separation. Lines split, but also merge, and the actual course of events is therefore far more intricate, its conceptual representation more complex.

On the other hand, biologists are thrown on their own resources for exact data, but ethnographers and cultural anthropologists can draw help and sustenance from almost all segments of the humanities. Wherever humanistic knowledge and understanding are being acquired, we can enormously profit by their use, whether it be in straight history, art or religious or institutional history, or the history of languages.

XIV

So much for what the kinship of ethnography is, what it aims to do, and what it actually is. Now about the particular studies to which the foregoing discussion serves as preface.

They all have relation, as a whole or in part, to the California Indians. However, California is construed somewhat broadly to include any peoples and cultural features in some way relevant to the aboriginal California situation. Where this relevance was clear, I have not hesitated to use materials whose reference was largely extra-Californian. The order of arrangement is a roughly geographical one in sequence from north to south within California.

Some of the studies here included, or in preparation to follow, contain previously unpublished data, but in none does the presentation of such new data occupy more than a minor fraction. In general, data are summarized, and the emphasis throughout is on discussion and interpretation. Detailed data here excluded but of sufficient significance to warrant being put on record, are reserved for subsequent publication, perhaps in Anthropological Records.

The second and the last of the present studies take off from data of my own collecting. The remainder are based wholly or chiefly on the data of others: as numbers 3 and 4 on printed publications of Omer Stewart and Madison Beeler. Number 5 rests on an unpublished census of the Indians of most of California made by C. E. Kelsey for the United States government, extant only in typescript.

That the approaches exemplified are more varied than in a holistic monograph descriptive of one people and culture seems fitting.

I express thanks not only to the friends and professional colleagues individually mentioned, but to the Wenner-Gren Foundation for Anthropological Research which made me a grant in support of prosecution and presentation of California ethnology, and to the Ford Foundation, Division of Behavioral Sciences, for an unrestricted personal grant in support of any study.

2. AD HOC REASSURANCE DREAMS

THERE IS a simple type of dream which may be called "reassurance dream," in that it tends to allay or dispel anxiety which is consciously realized and which has genuine objective basis. Dreams of this type accordingly do not spring from repressed or disguised anxieties but from real and overt ones. They meet the problem of the anxiety directly and with a minimum of symbolism; we would expect them to occur mostly in simple personalities, or, if in more complex ones, when these are confronted by all-over, basic problems such as those of health and life.

By way of illustration I may cite the dreams of informants as told in *Walapai Ethnography* (Kniffen *et al.*, 1935). When "K" went to sleep while herding, he dreamed that his horses had strayed but that he had found them again; when he was sick, he dreamed that he would get well; when beginning to age and be unwell, he dreamed that he was told he would have one more severe and long illness but would recover and then live to be very old. He was a simple, forthright, practical, tradition-bound character. His wife "L," sensitive and pessimistic, tended to have dreams of fatal outcome if her children were ill. Walapai "B" had some reassurance dreams, but they were longer and more involved than those of "K," were full of concrete imagery, and evidently drew on levels deeper than those concerned with the immediate situation.

I want now to present some reassurance dreams from two California tribes of quite different culture and personality type, the Yurok and the Mohave.

YUROK

Dreams do not figure very prominently in Yurok life, perhaps because the culture is so forethought-oriented that it tries to make advance provision for every contingency, and anxieties tend therefore to be drawn into waking consciousness.

A youngish middle-aged man of Weitchpec, known by Yurok custom as Wohpiso from the name of his house and called Lame Billy in English, was so crippled by disease in 1902 as to hobble about only on crutches. This condition had gone on at least several years, but in his youth he had had normal health and strength. He was a naturally good informant, made better by not being distracted by the need or habit of physical activity; and he had begun to develop some of the "sharpness" of permanent cripples.

He had been telling me how women became shaman-doctors, how he had once accompanied a doctor candidate up the mountain behind Weitchpec to guard her as she danced through the night until she came into trance, of the rock-walled pits there in which such candidates danced, and how the beginning of their power was a dream that they had been given and had swallowed a disease pain, snake, or such. Then he went on to tell me—as a man, he was not a doctor—how he had had three dreams when he was sick, and "after each got better."

"The first dream came when I was very ill, the second when I was expecting to die, the third last winter when my legs swelled.

1. "I was on Kewét Mt. back of here and saw one of the doctor-making 'houses of stone.' A dog was lying asleep on top of it. I went up closer, looked inside the wall, but no one was in it. Then I saw the dog eyeing me, and I thought, 'If it starts to jump at me, I will jump it.' Then the dog did come at me—and I awoke."

Acute fear presumably prevented completion of the dream. There is nothing to show directly that the dog stood for his illness, except that the informant had put the whole series into the context of his health.

2. "Two white men came in where I was lying. One of them said, 'What is the matter with you?' I said, 'I am sick; I am going to die.' He answered, 'No, you won't die. I can see when Indians will live and when they will die. Look here!' The back of his hand was hairy. With thumb and finger he took hold of this hair and drew it out and out, perhaps two feet long—the ends hung down to the ground. 'This is my medicine,' he said. 'If an Indian is about to die, I save him by brushing him with the ends of this hair. But you don't even need it'—and he hastily stuffed all the hair back into his sleeve, and went out, saying, 'You will get well.' "

His associations with the long hair would have been most interesting, but ethnologists in California were not asking for them in 1902.

3. "An oldish white man was coming, holding in each hand a galvanized bucket with fire in it. 'Get up!' he said.—'I can't.'—'Yes you can,' and he poured the coals of fire over my head out of one bucket; and as I tried to draw away, he held me and said, 'Be still!' So I tried to stand it. Then he poured the second bucketful over me, seized me by the back of the neck, and said, 'Get up!' I awoke and found myself sitting up."

The informant went right on and added a fourth dream.

"Then, during this past summer, I could hardly eat. I had no hunger, felt nauseated, but could not vomit. Then I dreamed as follows:

4. "I saw a little Indian boy, who said, 'I will hit you by throwing this at you.' —'What for?'—'To make you vomit.' I thought, Well, let him do it. I had thought he was holding stones, but now saw they were eggs. He threw and they struck me on my upper chest, on the right side; and I awoke.

"I started to go out doors, but vomited black before I could get to the door. Then I drank, and vomited again. After that a half-breed advised me to drink salty water, which I did and vomited a third time. Since then I have eaten with appetite."

I am sorry to say, though it will not be unexpected, that poor Billy died within a year or two.

Mohave

Earlier in the same year of 1902, an old Mohave at Needles, Nyavarup, told me the Mohave story of the origin of the world, which he had, in correct Mohave fashion, dream-experienced. As an old man and a curing doctor he was entitled to tell it. Yet there was evidently some doubt in his mind about narrating supernatural things to a white man. A day or two after telling me the first part of the origin myth, Nyavarup said to the interpreter—not to me:

1. "The night after I began to tell him, I dreamed I was in front of Mastamhó [the creator institutor] with Kroeber. Mastamho was wearing black-willow bark. Then he said to me: 'That white man is all right. He knows something.' "

Without this reassurance, or if it had been contrary, the story would presumably not have been resumed next morning.

For contrast I give a non-*ad hoc,* formalized dream, required by Mohave culture

to validate almost any power claimed. This one Nyavarup also told to the interpreter.

2. "I saw Mastamho. He spread his arms and took four steps, and shook his arms: now one forearm was broken. Then he called to me: 'Come, little boy'—as I then was—'Blow on it!' So I blew, four times, and his arm was whole again. And he said, 'Now you can do that. If one falls and breaks a bone, or gets it broken, you are the one that can cure it.' "

I cite some further Mohave dreams told me in 1953 and 1954 by Robert Martin, an intelligent and literate Mohave in his fifties, living near Parker.

He dreamed this in the 1918 influenza epidemic:

1. "I was flying, soaring along with my arms spread, and saw the earth opening and boiling up, with people lying dead, scattered around. I came down close to it, but rose again and flew on.—An old kinsman later told me it was a good dream because I was up and out of the death and destruction."

The gaping and boiling earth has an uncertain symbolism, but the scattered dead in a time of epidemic, and the dreamer's rising away from them, are of course a simple and directly comforting device.

2. "Another good dream I had was that I was shot through with bullets. I felt of the holes the bullets made, and where they came out, but, sensing no pain, wondered and was not anxious."

The symbolism is again so direct, at least on the upper level, that no comment is necessary.

A third dream told me by Robert had to do with a part he played in a funeral commemoration or Nyimitš, corresponding to the Yuma Kerruk. In this commemoration for warriors of renown, eight men run back and forth each carrying an ukwilye. This is a double-pointed, feathered, short, non-flight lance; in battle, if its carrier is wounded, he sets it in the ground and someone else takes it. It is given only to young men fully adult, because of the danger to which the carriers are exposed if they violate any of a series of taboos. If they look back at the shade roof when this is fired at the conclusion of the commemoration, they will become blind. But they must dive into the river four times with their eyes open. During rests in the running, they may gargle water but not drink it; they wait standing, not sitting. They must eat no salt for four days, nor sleep in the daytime, and must remain chaste.

Robert had been warned so much about these taboos that he was evidently worried that he had transgressed one or the other. Then he dreamed:

3. "I was carrying the ukwilye on my shoulder. I could hear its feathers rustle as I went along. The path rutted deeper and deeper, and finally there was just a deep hole ahead. When I got into this, the vertical bank ahead of me began to cave down. I planted the ukwilye, which got completely covered up; but I was standing clear, up on top.—When I told this dream to an old man, he declared it a good dream. The ukwilye being buried meant it was finished and would never cause me trouble. But if I had been buried in the dream, it might have meant that I would die."

It need hardly be said that to a believing Mohave the dangers of sickness or

death from ceremonial taboo transgression are as real as physiological illness, so that the element of genuine self-reassurance is indubitable.

At the same time, falling into a hole and swallowing earth appears to be among the Mohave a standardized dream symbol for impending death, although the Mohave do not bury but burn their dead. This is shown by an incident in my *A Mohave Historical Epic* of 1951, page 101, section 157 of the long tale, in which the great war leader Hipahipa tells his assembled allies of a bad dream he has had. He pushes, as he did when young, against a big rock; but now, when it falls down the bank, he falls with it, and in the hole he "eats earth-tongue." The narrator could give no explanation of what earth-tongue meant or was; nor is it said why he had his hero tell this dream to his friends before setting out on an expedition of conquest. But there is no doubt that to both the narrator and his hero the dream presaged death, and as the tale develops, Hipahipa is killed in battle though his side wins the war. Martin's dream therefore falls back on stock Mohave dream apparatus in its use of earth, cavities, and falling, but on his personal waking experience (and source of anxiety) as regards the ceremonial lance, and in his grasping a simple wish-fulfillment reassurance for himself.

I am not implying that this type of dream is peculiar to Indians or unlettered peoples, but rather that they also have it. I might mention a distant old relative of mine who had been a railroad express guard for years without ever meeting violence; but in his extreme age, with his final illness creeping up on him, he dreamed he was again in the express car and it was held up by bandits, but he shot it out with them and won and saved the company's bullion.

3. COEFFICIENTS OF CULTURAL SIMILARITY OF NORTHERN PAIUTE BANDS

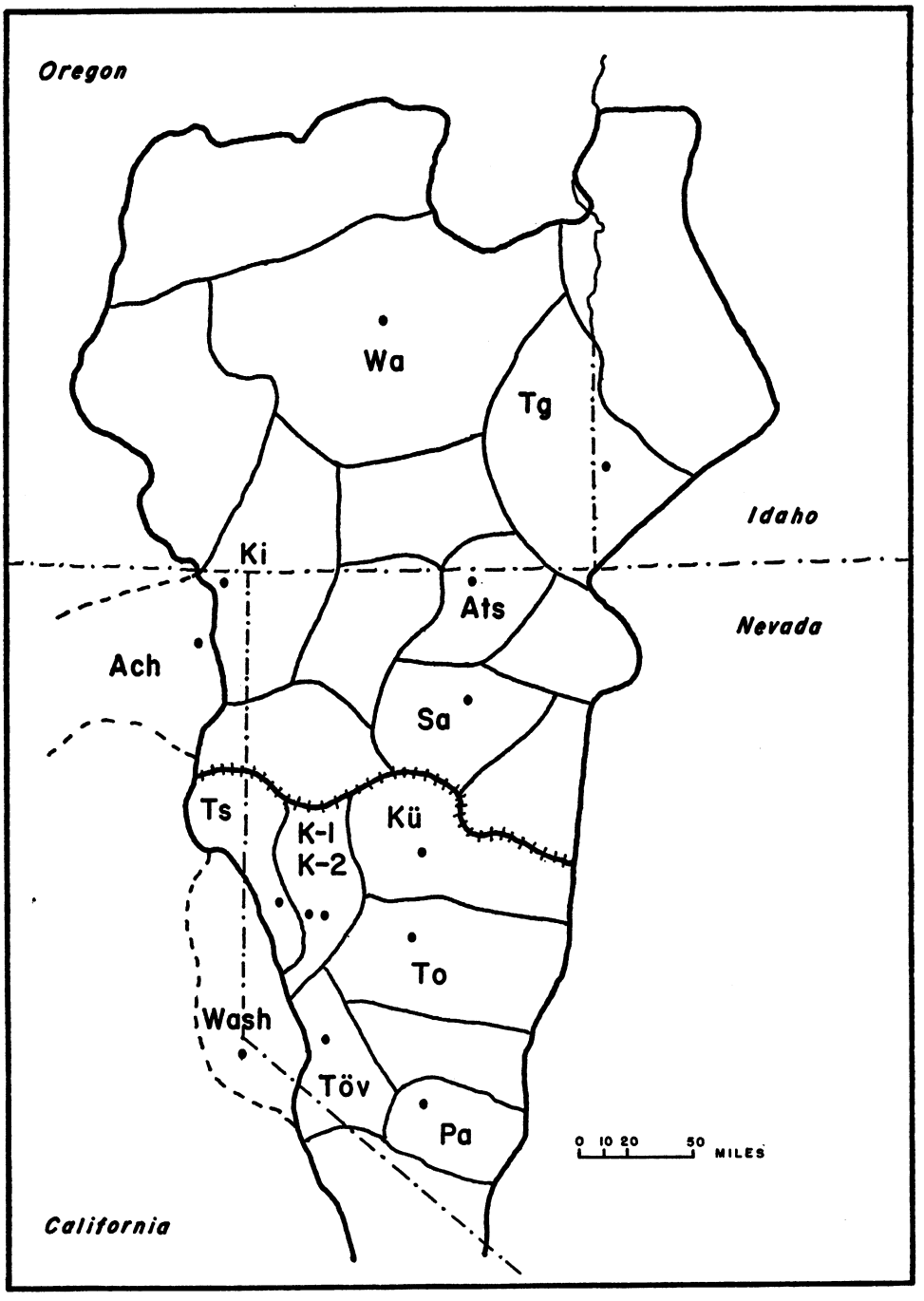
IN THE UNIVERSITY of California Culture Element Survey of Native Western America of 1934 to 1938, which was based on check list or questionnaire field work (plus notes) among 279 tribal groups, 13 investigators took part and made 20 trips to as many areas. The basic data have been published, beginning in volume 37 of American Archaeology and Ethnology, and continued in volumes 1, 4, 6–9 of the University's Anthropological Records series.

One of the twenty sets of lists, named "Northern Paiute," was filled by Omer Stewart and constitutes *Culture Element Distributions: XIV,* in volume 4 of Anthropological Records. The informants interviewed were mainly in Nevada, with some overlap into California, Oregon, and Idaho. They represented 12 groups or bands of Northern Paiute (Paviotso) and 2 non-Shoshonean groups speaking wholly distinct Hokan languages, namely, Eastern Achomawi and Washo. The distribution of these 14 groups (or strictly, 13, because there were two Kuyuidökadö informants) is shown on the accompanying map, simplified from Stewart. There were some 10 or 12 other aboriginal groups living within the range of Stewart's study, all speaking Northern Paiute, from whom lists of culture elements were not obtained. No names or symbols are entered on my map for these unvisited groups, in order to make reference to the relevant groups readier. The symbols for the groups represented are the arbitrary abbreviations adopted for the Survey (see Kroeber, Anthropological Records: 1[7]: 435–440, 1939). These abbreviations stand for longer stem forms given in the first column of the present coefficient table; but these stems in turn must be supplemented by the addition of an element -tuviwarai (or "mountain-tuviwarai") where the stem is followed by ":" in the table; or if it is followed by "-", by addition of -dökadö or a similar form, meaning "eaters"; these supplements are necessary to reconstruct the full native name of each group. Heavy dots represent informants' native homes.

Works Progress Administration employees tabulated the present and absent (plus and minus) frequencies as between each pair of the fourteen lists; from these they computed the Q_2 intergroup coefficient of correlation. This formula is $(ad - bc)/(ad + bc)$, where a is frequency of traits shared, d is frequency of traits absent in both societies, b is traits present in the first but absent in the second, c the reverse.

The arrangement of groups in the coefficient table is designed to cluster coefficients most significantly. The main natural clusters or classes are boxed, and the higher values are italicized, the highest in black-face type. The coefficients are rounded to the nearest percentages. I have also added the mean of the coefficients for each group.

The total number of present and absent entries in the fourteen lists was 32,216, an average of 2,301 per "tribe," with a range from 2,211 to 2,331. Absences exceeded presences as 52.6 to 47.4 per cent. The variation between bands or tribes is not great—from 45.2 to 53.2 per cent absences—an even enough balance—in all except two cases. For the Tövusi- Paiute the proportion of positive responses was down to 39.6 per cent and for the Washo "tribe" as low as 34.8, or little better than



Map. 1. Distribution of Northern Paiute bands. (After Omer C. Stewart, 1941.)

one-third. The Washo, being both "foreign" in speech and marginal in the area, also show the lowest mean of intertribal coefficients (see table); but in general, there seems to be no marked correlation of these features. For instance, the Paiute group whose interband coefficients average the highest, the Kidü- (mean 64), have a slightly below-average proportion of positive answers, 46.9 per cent.

Although the Q_2 range is from 1.0 to –1.0, the interband or intertribal coefficients are all positive, from 0.27 to 0.86. On the basis of experience, this is a

TABLE 1

NORTHERN PAIUTE INTERBAND Q_2 COEFFICIENTS IN PERCENTAGES

	Wash	Ts	K-1	K-2	Kü	To	Töv	Pa	Sa	Ats	Tg	Wad	Ki	Ach
[a]Washo	..	29	35	35	35	48	48	51	40	27	29	39	49	30
Tasiget:	29	..	*74*	*68*	53	61	50	58	52	52	34	29	52	38
Kuyui- 1	35	*74*	..	**84**	58	64	57	62	61	58	46	40	*74*	50
Kuyui- 2	35	*68*	**84**	..	*73*	*71*	64	*68*	47	43	35	38	60	43
Küpa-	35	53	58	*73*	..	**76**	*67*	54	51	49	35	33	51	36
Toe-	48	61	64	*71*	**76**	..	*74*	*71*	60	47	35	41	64	37
Tövusi-	48	50	57	64	*67*	*74*	..	59	56	39	43	38	54	41
Pakwi-	51	58	62	*68*	54	*71*	59	..	57	48	41	42	56	41
Sawawa:	40	52	61	47	51	60	56	57	..	**79**	*73*	*71*	*75*	56
Atsa:	27	52	58	43	49	47	39	48	**79**	..	*68*	*69*	*71*	52
Tagö-	29	34	46	35	35	35	43	41	*73*	*68*	..	**86**	**81**	56
Wada-	39	29	40	38	33	41	38	42	*71*	*69*	**86**	..	**79**	57
Kidü-	49	52	*74*	60	51	64	54	56	*75*	*71*	**81**	**79**	..	*69*
[a]E. Achomawi	30	38	50	43	36	37	41	41	56	52	56	57	*69*	..
Mean	38	50	59	56	52	58	53	54	61	54	51	51	64	47

[a] Not Northern Paiute.

more or less expectable range for contiguous social groups of similar level of culture, living in the same type of environment, and mostly speaking the same basic language. The highest coefficient was 0.86, between the only two groups from Oregon; the next, 0.84, between two informants of the same Tasiget: band. Theoretically, this last situation should have resulted in a perfect correlation of 1.00. The lag from this is expectable owing to the imperfection of the questionnaire technique: misunderstanding of question or of answer, emphasis on different aspects of the same item, occasional admission of traits known to informants from neighboring groups but not really characteristic of their own, actual changes in this respect within memory, and so on.

The inferences to be drawn from the coefficient table are as follows:

1. The Washo are the most divergent group of the lot, with a coefficient mean of only 38, and range from 29 to 51. (For convenience, the decimal point will be omitted hereafter, as in the table: thus, 38, not 0.38.)

2. The next most divergent are the Achomawi. Their mean coefficient is 47; the range, from 30 with Washo and 38 with the Tasiget: Paiute to 69 with the adjoining Kidü- Paiute. The Achomawi informant was subsequently utilized by Erminie

Voegelin on a somewhat different questionnaire used by her in northeastern California and published as *Culture Element Distributions: XX,* in volume 7 of Anthropological Records in 1942.

3. The mean of the 66 inter-Paiute coefficients is 57, with range from 29 to 86, as against a 12 Paiute-Achomawi mean of 46 and Paiute-Washo of 39. The sharp, in fact total, linguistic differentiation between Paiute and non-Paiute is therefore accompanied by a moderate but perceptible cultural differentiation. This means that the Washo and Achomawi peoples have not been in contact with Northern Paiutes since time immemorial, because in that case they would presumably have been assimilated to the Paiute cultural mean to the same degree as the Paiute divisions are interassimilated (or undifferentiated). But whether the interspeech contacts of culture have been going on for millennia or for a few centuries only, we have no means of estimating, because we do not know the time rate for change.

4. Washo is somewhat more differentiated from Paiute than is Achomawi. This may be because the Washo and Paiute became neighbors later, or perhaps because much of the Washo territory is high, timbered, and relatively well watered whereas the Eastern Achomawi habitat is arid and scrub-timbered much like most Paiute habitat. This latter is likely to be the principal factor in the cultural differentiation, but the former is also possible. That the Achomawi-Kidü- coefficient of 69 is 12 points higher than any other Achomawi-Paiute coefficient, and 21 points higher than the mean of these, merely reflects greater proximity of Achomawi and Kidü-.

5. The dozen Paiute lists fall quite evidently into two divisions: a northern group of five bands, and a southern of seven (six), this latter including a further minor subgrouping. The coefficient table leaves no doubt of the main split. The differences are not very great, but they are consistent.

The northern group comprises the Sawawa: and Atsa: of Nevada, the Tagö- and Wada- of Oregon, and the Kidü- of both states, also presumably the intervening or adjacent Kamö, Moa or Agaipaniva, Makuha, Yamosöpö, Tsösö'ödö, and "Yahuskin" bands.

The southern group comprises Küpa-, Toe-, Pakwi-, and Tövusi- in one subgroup, and Kuyui- 1 and 2 and Tasiget: in the other, but with "Kuyui- 2" participating almost equally in both subgroups.

The line between the two main groups runs between Winnemucca and Lovelock, or Sawawa: and Küpa-, and thence presumably westerly to the California boundary with the Kamö- band on its north, the Kuyui- and Tasiget: on the south. I know of no specific ecological, historical, or linguistic reason for the boundary; but its existence seems indubitable.

Omer Stewart suggests that my line between the southern and northern groups of Northern Paiute bands is rather close to one that Willard Park drew in 1938 in his paper on habitat of the Paviotso bands (*American Anthropologist,* 36:622–626, with sketch map). Park uses "Paviotso" for the Northern Paiute of central western Nevada, and distinguishes them from those of northwestern Nevada and Surprise Valley in northeastern California, whom he calls Northern Paiute. He cites no culture traits that differentiated the two groups, but says that his Paviotso "regarded themselves as an entirely distinct group," and had "an incipient feeling

of nationality." The clustering of coefficients based on Stewart's element lists indicates that there was a basis of concrete cultural difference underlying this sense of subnational difference.

Park's "Paviotso" area coincides pretty closely with my southern subarea, except for extending northward on the east to Winnemucca. This is because he unites into one band the "Hapud-eaters," Stewart's southern subgroup the Küpa-eaters from around Lovelock, and Stewart's northern subgroup of Sawa-eaters at Winnemucca; both of these bands of Stewart center on the Humboldt River. Park is more summary throughout. Thus he fails to mention Stewart's Pakwi- or Tövusi- or Tasiget: bands. (Of course Park's "Paviotso" is a special usage; the name is the one applied by the Nevada Shoshone to the Northern Paiute generally, and therefore is an outright synonym for the Northern Paiute as a whole.)

As the six bands of the southern subgroup are situated abreast the Washo (in the same latitude), but four of the five northern ones abreast the Achomawi, it may well be that the intra-Paiute differences between the two subgroups are in part a reflection of their varying extra-Paiute contacts to the west.

6. The smaller subgroup of the southern division occupies a limited, compact area centered on Pyramid Lake and lying northeast of Reno and the Washo territory. Again I am not aware of any reason for the distinctness.

7. The Washo are in direct contact only with the southern division of Northern Paiute. Their coefficients are perceptibly higher for the three bands of this division southeast and east of them (Toe-, Tövusi-, Pakwi-, mean 50) than for the four (three) (Tasiget:, Kuyui- 1,2, Küpa-, mean 33.5) to their northeast. Again there is no evident reason for the difference. Even with the five interviewed bands of the northern division the Washo coefficients run higher (mean 37) than with the nearer bands to their immediate northeast!

8. In a general way, increased distance is reflected in decreased similarity among the Paiute, indicating a tendency to continuity of distribution and therefore presumably of diffusion and acceptance. This is evident on examination of coefficients of the most marginal bands in the study. Thus, the Wada- are the most northerly: their coefficients with the other bands of the northern division run from 69 to 86, mean 76; with the southern division, from 29 to 42, mean 37. Similarly, the Pakwi-, the most southeasterly band, run 54 to 71, mean 62, with the southern bands, but 41 to 57, mean 49, with the northern ones. All this is of course to be expected in a general way: it is departures from a steady correlation of proximity with similarity that would suggest that special influences had been at work, as in the difference between the southern and northern division of Paiute, and the northwesterly subgroup within the southern division.

9. There are some unexplained non-concordances with geography, as between particular bands. Thus Sawawa: has a coefficient of 51 with Küpa- to its immediate south, but 60 with Toe- farther south. In some instances special ecological or other local factors may have been at work to bring such irregularities about. But unless such are known, it is wiser not to hypothesize them, because the lists are not a fine enough instrument to make small differences reliable unless they are consistently cumulative over larger areas. For instance, the only coefficient above 70 to occur outside the three box frames of the table is Kidü- with Kuyui- 1,

74. Now the distance between these two bands happens not to be great; but with Kidü- and Kuyui- 2 showing only 60, it is evident that accidents and errors can account for several percentage points in the coefficient, for the two Kuyui- informants were brothers! The coefficient of 84 between these brothers is surpassed by the coefficient of 86 between the Atsa: and Wada- bands, and these are not even in contact, the Tsösö'ödö intervening. Of these two Kuyui- informants, no. 1 shows the higher coefficients of similarity with the Tasiget: to the west and with the five northern bands; no. 2 with the remaining four southerly bands; the "lean" each way averaging 9 or 10 points. It would seem that one brother had associated more with his western and northern fellow-Paiute or had been influenced by them; the other more with those to the east and south. No doubt similar personal influencings occurred in other cases but are not evident because the record is restricted to single informants.

It is only when the results show a repeated and consistent drift for a number of local groups that it is safe to draw inferences from the figures.

4. SOME NEW GROUP BOUNDARIES IN CENTRAL CALIFORNIA

IN 1954, Madison S. Beeler published in *Western Folklore,* 13:268–277, "Sonoma, Carquinez, Umunhum, Colma: Some Disputed Place Names." For each of these he suggests a new origin and etymology. His evidence and arguments seem to me wholly probable for Sonoma, Carquinez, and Umunhum, and at least as probable as any other for Colma.

SONOMA

Sonoma, appearing also as Sonomi or Sonon in data of 1816, is derived by Beeler from Suisun Patwin *sonom,* "nose," for the semantic use of which as designation of a geographical feature there is precedent in other California languages. The usual Patwin word is *sono,* but Arroyo de la Cuesta in 1821 recorded *sonom* as the equivalent in the Suisun dialect. Of this, Sonoma seems a Hispanicization.

Now Chamisso and Choris both recorded a statement that five tribes speaking the same language constituted the majority of the neophytes at San Francisco in 1816. Beeler shows that three (or four) of these tribelets spoke Coast Miwok, one (or none) Plains Miwok, and one Costanoan. For the Huimen or Wimen, he cites a Coast Miwok vocabulary by Arroyo; the Tamal of Choris are evidently from Tomales Bay; and Olompali was a Coast Miwok village near Petaluma, according to Barrett. In place of Tamal, Chamisso cites Saclan, who were interior Miwok according to another Arroyo vocabulary. The Sonoma, according to Mariano Vallejo in 1850, were "originally called" Chocuyen, but later Sonoma after their chief upon whom this name was bestowed by a missionary. But we have a Chocuyen or Tchokoyem vocabulary from the head of Sonoma Valley preserved by Gibbs and printed in Schoolcraft (3:428), and reprinted in Stephens Powers, page 555—and this is Coast Miwok! Finally the Utschiun or Uchiun are probably the Juichun for whom Arroyo again has a vocabulary, which however proves to be Costanoan. It is not unlikely that these Costanoan Juichun learned Coast Miwok from their fellow converts at San Francisco, and that this fact was the basis of the error of Chamisso and Choris's Spanish informants that the five tribes spoke alike.

Beeler says that acceptance of his etymology seems to require the assumption that the Sonoma region was once inhabited by Patwin speakers who left behind a name which was subsequently given to the Miwok Chocuyen living there in historical times. I would construe rather the reverse: What we call Sonoma Valley was Coast Miwok territory, with Chocuyen the native name of the principal settlement and tribelet. This is because the basic uncontested fact is that the Gibbs Chocuyen vocabulary is Coast Miwok. Next in importance is that Sonoma is a Patwin word, applied to this same Coast Miwok group. That the Patwin name was first applied to the chief of this group, that it was applied by a missionary, that it referred to the moon (which is not stated by any one else and is corroborated by no vocabulary of any native language), all this sounds like the cock-and-bull kind of improbable story that whites have a way of inventing or confusing about Indians. The tale is even internally inconsistent. What sense would it make for a Spaniard to give a chief the name "valley" of the moon? And in fact old Vallejo's son corrected his father by saying that *sono* meant nose and that moon was *sano;*

which agrees with the available vocabularies. Thus Barrett, *Ethno-geography of the Pomo,* 1908, page 81, Patwin *sunar,* moon.

As a matter of fact, ownership of the Sonoma Valley was left dubious as between the Coast Miwok and the Patwin by our two prime authorities on linguistic boundaries in this part of the state, S. A. Barrett and C. Hart Merriam. Barrett seems more inclined to give it to the Miwok (see his *Pomo,* p. 286, n. 357), Merriam to the Patwin, at least as far west as lower Sonoma Creek. But neither expressed himself positively, probably because neither was able any longer to find Indians claiming sure knowledge or descent from the Sonoma tribelet. The fact that a Coast Miwok vocabulary was secured from a Sonoma Valley Indian a half-century before Merriam and Barrett definitely weights the scales in favor of the inhabitants of the tract having been Coast Miwok.

As between the "tribal" or place names Chocuyen and Sonoma, the choice here is much less significant, because they were evidently the Miwok (or Costanoan?) and Patwin names of the same tract and group. The Indians of each group usually had names in their own language for places known to them in the territory of their neighbors. The Patwin evidently had Sonom as their name for Coast Miwok Chocuyen. It was no doubt applied by them from where they lived in the Vallejo-Suisun area; it is unnecessary to assume that they were dispossessed from Sonoma Valley by Coast Miwok but left their name behind. In fact the taking over of an old place name by a new language would be unusual, because the language new to the area would already have a name of its own ready through acquaintance with the tract before it was occupied.

In short, the Gibbs vocabulary is decisive. Sonoma Valley must be construed as belonging to the Coast Miwok at the time the Spaniards came to know them; but the Spaniards happened to adopt the Suisun-Patwin name for the valley and its tribelet instead of adopting the native Coast Miwok name.

As Sonoma Creek technically flows into the lowest tidewater part of Napa River and not directly into the San Pablo segment of San Francisco Bay, it is quite possible that the holdings of the Chocuyen-Sonoma people included lower Napa River. Or possibly this broad sluggish estuary itself served as boundary between them and the Suisun; such demarcations occur, though not as frequently as watershed divides. But I know of no direct evidence bearing on ownership of either side of lower Napa River.

Our transfer of Sonoma Valley from Patwin to Coast Miwok native proprietorship makes the total Miwok distribution less scattered than it has been on our maps. Sonoma Valley is nearer to the Lake Miwok south of Clear Lake than are the mainly salt-water-bordering tracts previously allowed the Coast Miwok. Sonoma Valley is also nearer to the Plains Miwok. And this Coast Miwok–Interior Miwok gap may be still further reduced by Arroyo's Saclan vocabulary turning out to be Miwok and not Costanoan, a new dialect intermediate between Northern and Plains Miwok. While the exact habitat of the Saclan remains to be worked out documentarily, it is at least possible that Interior Miwok extended farther west than our maps have heretofore allowed—beyond the Waipa ("Wipa") whom Merriam puts on the downstreammost inter-Sacramento–San Joaquin island.

Carquinez

Carquinez is evidently a Spanish plural designating the tribelet whose principal settlement was Karki-n, which Beeler satisfactorily analyzes as "trading place." This name is appropriate because the strait was the waterway connecting bay and ocean with the Interior Valley river drainage, and because the half-mile-wide strait is the easiest crossing for north-south connections.

Beeler gives the Karki-n people both sides of the strait. He suggests that the main village might have been at Crockett on the south shore or at Glen Cove on the north. And he cites a 1781 Cañizares map, not naming the Karki-n-es but showing four villages of friendly Indians trafficking in tobacco and fish, on *both* shores of the strait, between San Pablo Bay and the beginning of the delta.

This makes it look very much as if native usage extended to the brackish water of Carquinez Strait, the usage—followed all along the Sacramento River down to the Nisenan Maidu—of groups consistently *owning both* sides of the stream. In short, the Karki-n tribelet evidently treated the strait as a continuation of the river, as indeed it is.

This then gives us at any rate one Costanoan tribelet that reached north of San Francisco Bay.

Also, the Patwin sustain a second loss of territory from our maps: first Sonoma Valley, now the north side of the strait. This looks as if their farthest southern and eastern tribelet were the Suisun-es, whose focus is indicated by modern Fairfield-Suisun, and which presumably extended west to Vallejo and to the Napa River.

South of Fairfield-Suisun are the Suisun marshes and the open water of Suisun Bay, which is wider than Carquinez Strait. In fact, it is too wide a body for one tribelet to have easily occupied both northern and southern shores, as the Karki-n did on the narrower strait beyond.

The revised distribution, however, still leaves one anomaly on our maps. From somewhat below Sacramento City to the beginning of Suisun Bay, the main channel of the Sacramento is still represented as separating Patwin on the west and north from Plains Miwok on the east and south. This holds even for C. Hart Merriam, our chief proponent of Plains Miwok extension. My feeling is that the pattern of native occupation would be more consistent if the west and north bank of this stretch of river could be taken away from Patwin and assigned to Plains Miwok. It remains to be seen whether there exists any historical or ethnographic evidence to sustain such a conjecture.

5. CALIFORNIA INDIAN POPULATION ABOUT 1910

IN THE *Handbook of Indians of California,* which was completed in 1917 and published in 1925, chapter 57 was devoted to population size, primarily in the native period (1770) but also in recent times (1910). The figure of 133,000 for the earlier period has been commented on in detail, especially by S. F. Cook (*The California Indian and White Civilization,* I, II, 1943, Ibero-Americana Nos. 21, 22), who raised it somewhat—by 7 per cent—which is within what I had suggested as the range of probable fact, namely 120,000 to 150,000.

However, present concern is with the number to which the Indian population had shrunk by the United States census year of 1910. This census made a genuine effort to enumerate the California Indians fully and by recognizable tribal names, of which a list which I had prepared was provided to enumerators in Indian districts. The total came to 15,000 individuals of identifiable ethnic group, plus 1,000 of doubtful or unreported affiliation and 350 non-California Indians living in California in 1910. This agreed well with C. Hart Merriam's estimate of 15,500 in 1900, a figure which showed a definite flattening of the curve of decrease as the nineteenth century progressed. Thus, 1849, 100,000; 1870, 30,000; 1880, 20,000; 1890, 18,000; 1900, 15,500.

However, there are additional data available which show that 15,000 or 16,000 was too low a total for 1910 and even for 1905, and that the true figure was in the neighborhood of 20,000. This is in line with what has since become evident for the United States Indians generally, namely, that the population of most groups fell to a minimum around the turn of the century, or a few years later in the far west, after which a slow but gathering increase took place. The figures refer to persons regarded as Indians, or counting themselves as such, irrespective of non-Indian admixture, provided this did not exceed seven-eighths of the hereditary "blood." It is obvious that there might be, and palpably often was, dilution of the proportion of Indian blood alongside an increase of the total number of individuals containing some proportion of Indian blood.

The data which mainly establish the larger figure of around 20,000 even before 1910 are from a census made personally by C. E. Kelsey in 1905–1906. Kelsey was an attorney in San Jose who some years before had been appointed to survey the landless non-reservation Indians of California, their needs, and what might be done for them, and on whose recommendation various small tracts—measurable in acres rather than in miles—were purchased and reasonably homogeneous remnants of Indian groups established on them. I believe that altogether about $150,000 was appropriated for this purpose by Congress. Some of the land was farmable and some was not, but it provided at least homes from which the occupants could not be evicted as mere squatters on sufferance. Some of these "reservations," as they generally came to be called, which I have seen were for Kato and other Athabascans near Laytonville, and for Patwin north of Colusa, at Cortina, and near Rumsey, and so on. All in all, humble as the benefits were, they at least alleviated some acute distress and misery at a time when non-reservation Indians in the more remote or rural districts of California were mostly not admitted to public schools, poor relief, or old age benefits.

Kelsey was an intelligent, conscientious, and industrious agent whom I came to respect, and in 1905–1906 he traveled over most of the state enumerating non-reservation Indians by name, at least by household heads, and grouped by "rancherias" or districts, and these grouped by counties. These lengthy schedules were typewritten, and Merriam and I both ultimately received copies of them, which were resurrected in connection with studies for the 1954 hearings on California before the Indian Claims Commission.

In preparation for these hearings, I had the totals added by counties and especially by ethnic groups and stocks, in regard to which Kelsey was as meticulous

TABLE 2

NON-RESERVATION INDIANS IN CALIFORNIA, 1905–1906, KELSEY ENUMERATION

	Heads of families	Persons
Without land	2,302	7,928
Holding land	885	3,015
	3,187	10,943
Mixed bloods	199	812
	3,386	11,755
On forest reserves in Humboldt, Siskiyou, Mariposa, Madera, Fresno, Kern counties	329	1,206
	3,715	12,961
Not visited; reported in U. S. Census in Marin, Merced, Sacramento, S. Benito, S. Joaquin, S. Luis Obispo, S. Mateo, S. Cruz, Stanislaus counties	.....	340
Estimated by enumerator as missed	.....	60
Total non-reservation	.....	13,361

as the circumstances of his work allowed; so far as I can judge by my own experience in many localities, he was substantially accurate. As the figures are not part of any controversy before the Commission, I am at liberty to publish them for their demographic significance.

The Indians formally attached to reservations and having rights there are of course summarily enumerated in the annual reports of the Commissioner of Indian Affairs; and the figures for the years ending in 1905 and 1906 are shown in table 3.

I have omitted the 229 Modoc on Klamath Reservation because there is nothing to show how many were descended from Modoc in California or in Oregon. The 508 Indians under the Parker Agency were presumably all in Arizona, but 30–40 per cent of the ancestors of the Mohave among them, and 100 per cent of the Chemehuevi, were once California Indians. The 675 Yuma were all on their reservation in California; originally the Yuma lived mostly west of the Colorado, but not wholly or consistently so. Those Mohave who stayed in their ancestral valley instead of removing to Parker reservation as they were entitled and expected to do, were apparently counted neither by the government nor by Kelsey. I have

guessed them at 500 adults and small childen, on the basis of the 200 children reported at Fort Mohave School and confirmed by what I saw of them between 1900 and 1908 at and near Needles. Some of them were attributable to California, some to Arizona.

All in all, this reservation count is a somewhat sorry showing statistically, especially with its inclusion of rounded numbers and omissions for one year or the other. But the over-all total of about 6,500 is probably not too far off. It shows what has always been assumed, that the reservation population of Indians was much less than the non-reservation population—less than half as great, in fact, in 1905–1906.

TABLE 3

RESERVATION INDIANS, 1905 AND 1906

Groups	1905	1906	Figure used
Hoopa (Hupa)	412	420	420
Hoopa Extension (Yurok)	.....	600	600
Pit River Indians at Klamath, Ore.	59	59	59
Modoc at Klamath	223	229	.....
Round Valley (various tribes)	615	.....	615
Tule River (Yokuts, mostly)	154	153	153
Mission, San Jacinto	1,097	1,097	1,097
Mission, Pala	1,654	1,578	1,654
Yuma	675	.....	675
Colorado River, Parker, Ariz. (Mohave, Chemehuevi)	508	494	508
Chemehuevi in Chemehuevi Valley	.....	55	55
Fort Mohave school children	.....	200	200
Mohave at Needles and in Mohave V., not reported, estimate by A.L.K.	.....	.....	500
Total	.....	.....	6,536

The two totals added together, 13,361 and 6,536, make 19,897 for California, on and off reservations, or within a few dozens of 20,000. This is 4,000 to 5,000 more—20 to 25 per cent more—than Merriam and I had figured for five years earlier and later, respectively.

Evidently two things happened. The decrease in the final years of the nineteenth century had slowed beyond its estimate; and this decrease had been converted into the beginning of a slow increase some years before 1905–1906.

If so, the true figure by 1910 would have definitely exceeded 20,000, on projection from 1905; this conjecture is confirmed by the continuing increase since then.

The official data in the annual reports of the Commissioner of Indian Affairs are in some ways less satisfactory than Kelsey's lone-handed enumeration. Even if the numbers are equally reliable, it is often less clear to what groups the official reports refer. On Round Valley, I found in 1910 that unrelated tribes were paired, like Yuki and Wailaki, and given allotments in a certain quarter of the reservation, and that thereafter any further allottees in that quarter were likely to be carried on the rolls as "Yuki and Wailaki" even if they were Concow or Pomo. As the

Bureau of Indian Affairs was administered for many decades, exact tribal or ethnic identifications were likely to be regarded as not only troublesome but without significance.

Kelsey, on the other hand, went directly to the Indians, and only to the Indians, for his data. He had a purpose, and the Indians would be coöperative, even if not too optimistic, at the possibility of getting land. He worked singlehanded but consistently, and face to face. Many times the Indians would not know their ethnic affiliation in white man's terms, but Kelsey showed practical intelligence in correctly converting their local designations into the "stocks" which Powers had introduced, and we at the University had begun to subdivide and map more accurately. The one point at which Kelsey figures were weak is the delicate one of "mixed bloods." I am confident that by 1905 these constituted more than the 7 per cent of the total Indian population that he allows (the 1910 census allows nearly 30 per cent), and that Kelsey knew it. But inquiry into paternity is not easy to press, and it would have been of only secondary relevance to the purpose of his engagement.

Since 1905, the California Indian population has increased to approximately double, confirming the reversal of trend that seemingly took place toward the turn of the century. At least, such doubling is a fact if one counts as "Indian" anyone having any traceable Indian blood. What the average proportion of Indian heredity is in the socially or legally Indian population of California would be interesting to know. Ultimately, it could probably be calculated with fair accuracy from the applications for enrollment in connection with the land claims cases. My impression is that the proportion would be not over one-half Indian. That is, the approximately 40,000 fullblood and mixed-blood California Indians alive in 1955 would "reduce" to the mathematical equivalent of 20,000 fullblood Indians, or about the same number as full and mixed bloods constituted fifty years before.

The 1910 Federal Census on California Indians

A special publication by the 1910 Census is entitled "Indian Population in the United States and Alaska." This monograph was by Roland B. Dixon, though his name is not on the title page. The Library of Congress catalog number is HA302 A316 1910.

Page 10 gives the total number of Indians in the United States, without Alaska, by decades, which is reproduced here.

Year	Federal Census	Commissioner Indian Affairs
1910	265,683	279,023
1900	237,196	250,000
1890	248,253	228,000
1880	.....	244,000
1870	.....	278,000

The low point is in 1900 by Census, in 1890 by report of the Commissioner of Indian Affairs. The differences, which run from 5 to 8 per cent, and in opposite

TABLE 4

1910 Federal Census Figures for Groups Wholly in California

Athabascans	1,154
Tolowa	*121*
Hupa	*639*
Mattole	*116*
Whilkut	(*76*)
Saiaz	(*6*)
"Mattole"	(*34*)
Wailaki	*227*
Kato	*51*
Chimariko	31
Chumash (35 S. Inez!)	38
Costanoan (all S. Cruz!)	17
Karok	775
Maidu	1,100
Miwok	699
Coast	*22*
Lake	*7*
Interior	*670*
Pomo, various	1,193
Salinan (all S. Antonio)	16
Shasta	353
Achomawi-Atsugewi	1,225
Hat Creek	*240*
Pit River	*985*
Shoshoneans wholly in California	3,308
Mono	*1,448*
Kern River	*105*
Kawaiisu	*23*
Tehachapi	*2*
Panamint	*10*
Chemehuevi	*353*
Serrano	*118*
Gabrielino	*11*
Juaneño	*16*
Luiseño	*467*
Cahuilla	*755*
Wintun stock	710
Patwin	*186*
Nomlaki	*125*
Wintun and Trinity	*399*
Wiyot	152
Yana	39
Yokuts	533
"Yokuts"	*302*
8 named tribes	*229*
Yukian	198
Coast Yuki	*15*
Huchnom	*15*
Wappo	*73*
Yuki	*95*
Yuman Diegueño	756
Yurok	668
Total	12,965

directions, are characteristic. It is clear, however, that decrease was progressive at least to 1890, but from 1900 to 1910 there was an increase of around 12 per cent according to both sources.

For California, the increase was less in this first decade of the century, around 6.5 per cent. The totals are, by Census: 1890, 16,624; 1900, 15,377; 1910, 16,371; for 1890–1900, 1,247 decrease; for 1900–1910, 994 increase. These figures suggest that the nadir may have fallen around 1901–1902. If the enumeration of 1900 was the more careless of the two, the low point may have been reached in 1900 or shortly before. There is little to suggest, however, that, as regards mere completeness of enumeration, 1900 fell seriously short of 1910.

There was a special effort in 1910 to secure more accurate information about Indians than ever before. The matter of tribal or ethnic affiliation was stressed. Special schedules were to be used for Indians whenever there was an expectable aggregation of them. It was this endeavor that made me, for instance, a special agent for the Bureau of Census in 1910, to aid enumerators in Indian districts, to provide tribal names which the Indians would not misunderstand, and so on. In California, more than 90 per cent of the Indians were *de facto* registered on these special schedules. Only 1,377 were entered on the general population schedules. These were mostly Indians living scattered among the white population.

On page 15 of Dixon, table 8 gives the stock and tribe figures for the United States. It is on these that the rounded figures are based in the tabulation in my *Handbook* which compares the 1770 and 1910 Indian populations of California. Rounding for 1910 seemed desirable because it was of course inevitable for the estimates for 1770. Also, I had learned the difficulties which unfamiliar ethnic names made for inexperienced enumerators, especially with Indians who were accustomed to be known either as generic "Diggers" or by locality designations which were parochial enough to be accurate but often difficult to attribute ethnically. Where a tribal and a regional name were the same, there was sure to be confusion. Thus the Census reported 353 Shasta Indians; but some of these were really only Shasta County Indians, probably mostly Wintun. In my table I therefore made the best corrective guess I could (perhaps aided by Kelsey's specific Shasta figure of 150), and reduced the Shasta from 353 to 100, but increased the total for the Wintun stock from 710 to 1,000. This may have been over-correction but was probably nearer the actual facts than were the Census returns. The tribes that overlapped the state boundary had also to be assigned somewhat arbitrarily. Finally, there were 22 ethnic groups for which from 0 to 76 persons were reported, but many of these figures were so out of correspondence with my personal knowledge, and the ethnic designations were so foreign to the enumerators, that I thought it less misleading to put an asterisk than to cite figures, and instead, to insert 450 as the total of the 22 asterisked groups. This 450 was rounded from an addition of 461.

I cite in table 4 the actual figures of the Census so far as they refer to groups wholly within California.

As against this Census-based total of 12,945 for all groups wholly in California, my table totals almost the same figure, 13,000, for the 23 larger surviving groups (those reckoned at 100 or more); it then adds the 450 estimated for the 22 minia-

TABLE 5

KELSEY AND 1910 CENSUS COMPARED, BY ETHNIC GROUPS

Groups	Kelsey[a] 1905	Census 1910
Tolowa	177	121
Athabascans Humboldt Co. excl. Hupa	155	116
Athabascans Mendocino Co. excl. Round V.	71	51
Wiyot	160	152
Karok (744 + 200 estim. on Forest Reserv.)	944	775
Shasta	150	353
Achomawi	832	985
Atsugewi	229	240
Yana	62	39
Pomo, total	1,374	1,193
Yukian total excl. Round Valley Res.	113	103
Wintun, Shasta, and Trinity counties	691	399
Tehama, Glenn counties (= "Nomlaki")	156	125[b]
In Patwin area	189	186
Maidu, total	1,344[c]	1,100
Coast Miwok (Kelsey, Sonoma Co. only)	28	22
Lake Miwok	41	7
Plains Miwok (Alameda Co.)	42[d]	670
Hill Miwok	631	
Yokuts excl. Tule R. Res. and Kern Co.	663	533[e]
Salinan	79	16
Washo in California	343	[f]
N. Paiute, Modoc, Lassen counties	303	[f]
Mono, Monachi W of Sierra	477	1,458[g]
Mono, E of Sierra, Mono Co.	332	
Owens Valley Mono and Panamint	1,031	
Tübatulabal, Kawaiisu, Kitanemuk	303	130[h]

[a] Excluding "mixed bloods," who constitute 7 per cent of the total.
[b] Including those on Round V. Res., whom Kelsey did not count.
[c] Because there is difficulty separating Maidu from Miwok in Eldorado and Amador counties, I have arbitrarily counted 220 of the 440 Indians there as Maidu and 220 as Miwok.
[d] The remnant at Pleasanton, much mixed, but Plains Miwok predominating. The Census does not separate them, though it reports 41 Indians in Alameda County.
[e] The Federal Census figure is for all Yokuts, incl. Tule R. reservation.
[f] Not separately given.
[g] Includes 10 Panamint.
[h] To which would have to be added the unknown Kitanemuk part of 118 Serrano counted.

ture or asterisked groups, so that my first total comes out 13,450 instead of 12,945. My count then proceeded:

Total, wholly in California	13,450
N. Paiute in California, estimated	300
Washo in California, estimated	300
Mohave, census, 1058, rounded to	1,050
Yuma, census, 834, rounded to	750
	15,850

I suspect that my estimates of 300 each for Northern Paiute and Washo in California may have been aided by Kelsey's figures of 303 and 343.

From the total of 15,850, however, it was necessary to deduct the Yuma and Mohave in Arizona. I estimated these—too low for 1910, I now think—at 850.

Deducting:	850
	15,000

To these I then added from the Census: non-California Indians in California in 1910, estimated at 350; and affiliation doubtful or not reported, estimated at 1,000. This figure of 1,350 seems in turn to have been a rounding of the 1,377 Indians reported in the census on general population schedules, for whom the tribal affiliation was likely to have been given differently or not at all.

Adding:	1,350
	16,350

Yet as the *Handbook* dealt with California Indians, it seemed fair to deduct the 350 "foreigners," which left 16,000 as the relevant final total for 1910.

However, this Census-based total of 16,000 in the *Handbook* is now shown to have been too low by about 25 per cent, even for four to five years earlier, by the combined Kelsey-Indian Affairs total presented above.

COMPARISON

In table 5 I have juxtaposed, where the groups visited or recognized allow it, the figures in the two enumerations. Kelsey did not visit or count Indians on reservations, in southern California, or in nine counties in central California.

It is apparent that Kelsey's figures run consistently higher, except for a few groups in northeastern California, although for Wiyot, Patwin, and Interior Miwok the numbers given in the Census are but little behind. I do not know why Achomawi and Atsugewi run somewhat contrary to the prevailing trend. As for the Shasta, the reason is plain and as suggested above: the Wintun-speaking Indians in Shasta County were simply classified as Shasta by the federal enumerators. Hence the "Shastas" jumped from Kelsey's 150 to 353 in 1910, but the northern Wintun fell from 691 to 399.

The Census lists 819 Washo in California and Nevada; Kelsey, 343 in California. By increasing the former figure by the average Census lag of 25 per cent, we get about 1,025 total, of whom Kelsey's 343 constitute almost exactly one-third.

I hesitate to apply a similar computation to the proportion of all Northern Paiute living in California, because the actual inclusiveness of "Paiute" and "Paviotso" by Census enumerators is too uncertain.

6. MOHAVE CLAIRVOYANCE

IN THE *Handbook,* pages 772–775 (Kroeber, 1925), there is given a fragment of a "Hipahipa legend," cited as a variant example of the Mohave "Great Doings" (Itškanavek) type of story represented by my *Mohave Historical Epic* of 1951. In the Hipahipa legend, a young man disappears because he has been killed and his body fastened to an underwater stump. His father travels about inquiring, and is finally referred by a very aged man to his son who "has dreamed like myself." As the father returns, he sees this youth following him, and then not following because he is traveling underground. This was noted as a motif frequent in myths, but otherwise lacking in both the Hipahipa account and the *Epic,* whose prevalent tone is "historical" precisely because it avoids magic. I added that when the youth arrived, he practiced a patterned piece of clairvoyance which was of ethnographic interest because not previously reported for the tribe (*Epic,* pt. 7, no. 3). I am summarizing the incident from *Handbook,* page 774.

Before the clairvoyant arrived, a little hut had been made of black willows set in the ground with their tops tied together. He stood before the structure, stamped, leaped up on it so it shook, then leaped up in the air from it, and back onto the ground. Then he sang four songs whose words were:

1. In the dark I see clearly.
2. (My soul) speaks and tells me.
3. In the dark I see brightly.
4. I shall tell it here.

Then he entered the hut and spoke. He told the name of the missing man, told of the four people he had met and where, and how he fed them fish, after which they killed him and hid his body by pinning it with a stick underwater against a stump.

The account continues that the corpse was found as described, and this led to the war which is the main subject of the legend.

Recently I inquired among the Mohave at Parker about this kind of clairvoyance, and the practice was immediately recognized and called tšekamitšk. Added features were mentioned, especially eating sand to bring on unconsciousness and trance, and possession of the clairvoyant's body during trance by the spirit of mountains. I give two statements, the first in 1953 by Robert Martin, the second in 1954 by him together with Perry Dean and others sitting in.

(*1953:*) Tšekamitšk is what a man is called who can see enemies ten or fifteen miles off, or report that there are none. Such a man has his mouth filled with sand, and a hut of arrowweeds is built over him. Then he smokes in there. When he is through "looking," he is "dead" [unconscious] ; but being turned over and shaken, the sand runs out of his mouth, and he comes to. Then he tells what he has seen. This gift runs in families.

It was such a seer that predicted the coming of Ha'iqo, the whites. He called them "Night fliers," Tinyám kuyárem. People asked, "What are those?" Later, when white people came, they knew.

(*1954:*) Tšekamitšk are "prophets." They are not singers but they perform, usually like this:

Four piles of sand are heaped up. Then the tšekamitšk eats the sand until he

keels over and stops breathing. He is calling his spirit. A small hut is built over [for] him, and those who want to hear crowd around and ask questions for the straight truth. His spirit comes into him and answers. It is the spirit of Avikwame Mt. that enters him and talks. He may tell how a sickness will end, whether enemies are approaching, and such things. This is called mat-kanavek, "himself talking." [The prefix mat- is reflexive or has automatic reference; the meaning is that it is the spirit in the "prophet" that is answering, not the prophet in his own right.]

Another spirit may also come. This is the one from a mountain in the Maricopa country called Aví-wáva. He is the enemy of Avikwame, and they fight; the bystanders hear it; it is like a prizefight; but Avikwame always whips the other. [According to Spier, *Yuman Tribes of the Gila River,* 1933, p. 23, Vi-váva, "solitary mt.," is Pima Butte, near Sacaton.]

It is Roadrunner, Talypo, who drags the tšekamitšk out [rescues him]. Sometimes he does this before the Avikwame spirit arrives; then the bystanders do not get to hear anything.

It is said that as the prophet eats the sand it turns to water in him and he just swallows that.

XY at Needles recently professed to be one of these tšekamitšk, but when he was challenged, he evaded a demonstration.

So far the 1954 account.

There are some inconsistencies between these accounts and between them and the incident in the legend: the hut built for the clairvoyant beforehand, or over him after he falls unconscious; the hut of willows or arrowweeds (they may both have been used at times); sand running out of the revived seer's mouth, or turning to water which he swallows (again, both may have been believed); smoking after his mouth is filled, as against eating sand until unconscious; telling after his trance what he has seen in it, as against a spirit possessing him and doing the speaking; jumping onto the hut, and singing before it about his power, both mentioned only in legend, but sand-eating not mentioned. Roadrunner's function is also not too clear. He seems to want to protect the prophet's life and sometimes spoils the seance by pulling him out of trance too soon.

However, we do have a striking pattern of performance, characterized by:

1. The shaman's hut, reminiscent of the Northeastern Woodland
2. Sand-eating to bring on trance for clairvoyance
3. Possession by spirits, who speak out of the performer
4. Spirits those of mountains, who contend with each other

None of these four features—hut, sand-eating, possession, spirits of mountains—appear in all the remainder of Mohave shamanism and dream power. And though general clairvoyance is credited by the Mohave to many powerful shamans, it is a direct power, without special circumstance or preparation, and is incidental to their power of curing, bewitching, and the like; contrariwise the tšekamitšk has his special techniques, exhibits them, but does nothing else beyond "seeing."

It would therefore seem that the particular tšekamitšk constellation is out of step with the larger configuration of Mohave dream powers and general shamanism. This suggests that it may be an import into the culture from outside. Indeed, there are Maricopa, Yavapai, and especially Walapai parallels.

Walapai

Walapai shamanism, as described on pages 185–192 of *Walapai Ethnography,* published in 1935 by Kniffen and others under my editorship, shows all four of the criteria of Mohave clairvoyance exhibitions, though their coördination and emphasis are different.

1. The "hut" is replaced by the small steam sweathouse (which the Mohave lack), similar even to its frame of bent-over willow poles; also perhaps by a buckskin tied over the performing shaman's head.

2. The curing shaman swallows four double handfuls of earth. This makes him "die"; thereupon he has the covering tied over his head and is laid in the sweathouse.

3. He is definitely possessed by spirits, which enter his heart and speak from his mouth.

4. The most powerful spirits are those of mountains. This shows in two ways: acquisition of shamanistic power, and its use. After beginning with dreams, some Walapai shamans clinched their power by spending four nights in a cave in Kwi-nya-wa or Akwi-nya (Mohave Ikwe-nye-va, Clouds' House), in Wi-nya-kaiva or Wi-nya-kwa'a Mountain (Artillery Peak near Signal). There they dreamed of the mountain's spirit. When a shaman with such a spirit cured, this spirit of the mountain entered him and sang from his mouth. If this was insufficient, the Kwi-nya-wa spirit visited the spirit of Avi-kwame (Wi-kame) across the Colorado and asked his help. (This aiding between mountains replaces the Mohave contending.) It is after this that the earth-swallowing took place in the sweathouse and the doctor "died," though his soul was heard talking with the soul (spirit) of Avi-kwame, until the doctor burst out of the buckskin and sweatlodge, with the earth running out of his mouth and ears. After he quieted, he—or rather the spirit of Avi-kwame in his heart—sang, spoke, and sucked the patient.

From the vivid and full detail given, this seems to be the dramatic climax of all Walapai supernatural power and curing, not merely a special exhibition of ventriloquism and clairvoyance as with the Mohave. It both subsumes and caps the total shamanistic pattern of the Walapai, instead of standing apart from it. It would appear that the Mohave took over the spectacular performance of the Walapai, but already possessing their own well-established system of curing which, moreover, was interramified with their mythology and general philosophy, they limited it to the exhibitionistic specialty of clairvoyance and ventriloquism. It is notable that clairvoyance is not mentioned in the Walapai account.

Yavapai

The Northeastern and Western Yavapai (Gifford, 1936, AAE, 34: 307–317) show a reduction of Walapai traits. An instance is mentioned of one Western Yavapai shaman "deceiving" a "ghost" (spirit?) by "simulating death" after having his mouth stuffed with dust and a cover tied on. As he lay in a hut, the ghost talked with him, as an awed audience listened outside; finally the ghost threw the shaman out the door: everyone heard the thud of his body in the darkness. This is obviously the same performance as the Walapai but in a reduced version or context

and without mention of mountains. In fact, it is presented as if it were a particular example of a more general practice of summoning "ghosts" to inquire from them about patients' illnesses. Yet the roadrunner of the Mohave belief crops up interestingly. Sometimes the wrong ghost came: "a Walapai or Mohave, or one making a sound like a roadrunner"; then this ghost was chased away with dust thrown at him (p. 316).

There is no reference to spirits of mountains in the long Yavapai shamanistic account, except indirectly in description of Akaka or Kakaka spirits (Northeastern and Western groups, p. 308). These live in caves or rock shelters, are heard at night as they fly from peak to peak, and may help shamans to cure. Four mountains are cited as abodes of these akaka: Granite Peak, Mingus Mountain, Four Peaks (Wi-kedjasa) in Northeastern territory, and Kofa Mountain (Wi-kasayeo) in Western. Some of these recur in Maricopa beliefs, which are more systematically concerned with spirits of particular mountains.

Maricopa

For the Maricopa, Spier relates an enemy-locating performance by a shaman (*Yuman Tribes of the Gila River*, pp. 162, 292) which parallels the Mohave account. It includes eating four piles of dirt, unconsciousness, a little hut, the coming of successive spirits of mountains. Possession or speaking from out the shaman are not mentioned. Details are: the seance is in the public meetinghouse; the shaman stoops to each pile and sucks it up, groaning, then falls dead; the hut is built over him and he is given a rattle; the audience feels a draft of air from the spirits of the mountains as they enter the dark assembly house; the spirits shake the shaman's rattle inside the hut and talk; a man beside the hut questions them. There is no mention of a spirit entering the entranced performer or speaking from him, nor of the spirits contending in this seance; rather the spirits confirm each other.

On the other hand, the idea of spirits of mountains being partisans, protectors, and omen bearers of tribes was well developed among the Maricopa (Spier, pp. 252–254). Four were particularly significant for them:

Vi-vava, "solitary mt.," Pima Butte
Vi-alyxa, "berdache mt.," Sierra Estrella
Vi-kwa-xas, "greasy mt.," Salt River Range
Xa-ga-vicaδo, "water divider," SW of Salt-Gila junction

These, as it were, framed the Maricopa settlements. Four others seem also to have been friendly or at least neutral to the Maricopa; they were at or beyond the boundary of Maricopa territory.

Vi-ka-tcakwinya, "granary basket mt.," Mohawk Mts., halfway to Yuma
Vi-'iδo, "black willow mt.," S of Prescott
Vi-akavanan, "ridgepole mt.," NE of last
'Ikwimkwimatc, "dancing with horns," E of last

Spirit mountains on foreign soil and guardians of foreign tribes:

Yavapai	[Gifford: Wi-kedjasa]	Four Peaks
Yavapai		N of Four Peaks
Yuma	Vi-alyxa, another "berdache mt."	Near Yuma
Mohave	Vi-kwame	Avikwame beyond Needles

Of these, Four Peaks with his partner to the north contended for the Yavapai whom they "owned," against Pima Butte and Sierra Estrella for Maricopa, Pima, and Papago. They "debated"; whichever won was happy, for his people would win in battle.

The two berdache mountains gambled with each other (pp. 242, 254) for warriors, who became berdaches (Alyxa probably means "coward" as well as "transvestite" in Maricopa as in Mohave). Presumably, says Spier, this was also a contest for Maricopa or Yuma success in impending battle. Mohawk Mountains, halfway between, took no side, but agreed with the winner as to which people should die.

It is evident that the Maricopa have developed most fully this image of great mountains identified with tribes and contending in one way or another on behalf of their peoples and influencing their survival.

This special position is also implied by what Spier says on page 252. "The mountains as spirits differed from those of birds and animals in that they had no powers to grant" [i.e., personal powers—their interests were tribal]. "But that they figured as spirits is certified by the way they appear in the clairvoyant act by which the whereabouts of the enemy was discovered."

Another Maricopa specialty is the relation of a particular mountain to transvestitism. "The Sierra Estrella had a berdache living inside, hence the name.... Another mountain at Yuma, called by the same name, had the power to transform men. As one sees them in dreams, the two mountains are young girls." They challenge each other to dice games. If the Yuma mountain lost, the Yuma lost a man [in battle]; he became a berdache.

The Mohave have nothing like this. Berdaches become such because they have dreamed of being with Mastamho on Avikwame and being inducted into the status by a simple ritual, which is thereupon actually performed for them (*Handbook,* p. 748; full account readied for publication). The two patterns are quite different: the Maricopa emphasizes war and berdachism as opposites; the Mohave validates the status by bringing it into relation with the cosmic myth.

Besides the sand-eating clairvoyance, the Maricopa practiced another kind on war parties, to locate the enemy (Spier, p. 176). The clairvoyant stood and smoked four cigarettes, seeming to lift off the ground higher with each one, until after the fourth he dropped like dead. When he sat up again he told how Buzzard had taken him to "the highest mountain" (unspecified) to show him the enemy and tell him how to approach them. Such men had dreamed of Buzzard, as might be expected, or of Coyote, and war parties against the scattered and restless Yavapai tried to include one of them.

As the Maricopa and Pima had the same enemies and often fought in alliance, I have looked in Russell (BAE, 26, 1908) for war practices and beliefs corresponding to those discussed but have not found much. His account of their warfare shows little of the preoccupation and formalization of the Yuman tribes. Evidently the Pima considered war a disagreeable business that had to be done, not an opportunity for a release. The two formal features that Pima and Maricopa shared, according to available information, are the four nights of set speeches on the way and the sixteen days of purification by those who killed a foe. (Russell, pp. 201, 204; Spier, pp. 167, 169.)

Maricopa and Mohave

Spier points out (pp. 242–259) that all Maricopa supernatural power came in dreams, usually beginning in childhood; that nearly always it was unsolicited, was the basis of all success, and was expressed in songs—very much as among the Mohave (p. 255).

I agree that the Maricopa-Mohave similarity in beliefs about the experience and significance of dreaming is very great. But there are also certain clear differences, especially in the systematized theory into which dreaming is fitted. The Mohave forced practically all their most meaningful dreaming into a mythology. The dreams referred to the beginning of the world and began at the god Matavilya's sacred middle of the world Ha'avulypo or at the god Mastamho's sacred mountain Avikwame. From these deities and holy spots the particular animal, bird, or insect that is the protagonist of a dreaming branches off or travels out until his transformation; his doings are the subject of a narrative, and also are sung in long series of variations on a particular melodic theme recognized by the Mohave public as to its reference. Such systematized dream stories vary somewhat according to the individual dreamer, but they are not private: they conform to a pattern, which is an established myth in a tribally recognized mythology.

The myths of the Maricopa are much less centralized and coördinated; and so is their dreaming. As Spier says, page 249, "the spirits of which they dreamed were primarily birds, with some animals and insects, the mountains, certain stars, lightning and thunder, and possibly the rain." The birds mentioned were mockingbird, blackbird, buzzard, eagle, horned owl, crow, killdeer, duck; the animals, deer, coyote, jackrabbit, cottontail, dog, frog, bat; the insects, cricket, gnat, small fly, and xalkwatat. There were also fish, the local mountains, thunder and lightning, the morning star, Orion's belt, and two undescribed spirits called Cilyaitcuwan and Kukupura.

The Mohave have never given me a corresponding list of the spirits they dream of. They list the spirits about whom there is a standardized story and a standardized song cycle. In these stories there are sections or episodes in which animals or spirits appear about whom there is no standardized myth cycle.

For instance, the Mohave Raven song-myth cycle begins from Ha'avulypo, and some of its songs refer to bat, stars, cane, enemies and war, scalps, the masohwaṭ bird, night hawk, curve-billed thrasher, and mockingbird. But the Mohave do not say they dream all this enumeration of spirits; they dream the pattern they call Raven, or some similar pattern: any such pattern includes reference to a selection of birds or other topics like those enumerated; and themes like bat and stars and cane may recur in many or most of the patterns or song cycles.

By contrast among the Maricopa, Spier says, "the cycles did not recite myths, but were confined to the personal experience of the dreamer. Nor did the dreamer project himself back into the mythological period: dreams and songs referred to the contemporaneous scene alone . . . Maricopa dreams were individual experiences of a type more general in North America than the Mohave" (p. 257).

The mountains, to the Mohave, are the sacred culminating spots at which the mythic events happened, or at least began. There is really ordinarily only one

mountain, Avikwame, with significance something like Sinai to the Hebrews, but even more concentratedly; Ha'avulypo is a "house" in a narrow canyon. And I do not recall having ever heard a Mohave allusion to the *spirit* of Avikwame appearing either in a myth or in shamanistic power (except as just below). The contrast is quite sharp with the Maricopa ranging the mountains in opposite camps as tribal protectors.

Visiting mountains in dreams enters into significant experience of Maricopa curing shamans. Spier gives three illustrations on page 247. In two of these, mountain peaks are connected by cobwebs or string in a manner reminiscent of the aerial "roads" between mountains constructed by rattlesnake, spider, and scorpion in a Mohave shaman's account in *Handbook,* pages 776–777. Here too appears the only previous Mohave mention of "spirits of mountains": rattlesnake asks Avihamoka, "Three-mountains" (our Double Mountain south of Tehachapi) for permission to bite and kill a man because he wants to have his soul as a friend. On the other hand, though "a deliberate quest for dreams . . . was rare" (Spier, p. 244) there are two Maricopa instances of mountain caves, "Kukupura's house" and "Bat's house," being visited for supernatural power in the Walapai manner.

Summary

The tšekamitšk clairvoyance of the Mohave is separate from their general pattern of shamanism and belief. Of its four distinctive elements, a hut or covering, sand-eating and trance, bodily possession by spirits that speak, and the contention of spirits of mountains, not one has a close parallel in the general shamanistic and mythological web of the tribe. The clairvoyant practice seems intrusive and unassimilated. The four features, however, recur in a complex among the Walapai and three of them with the Maricopa. Among the Walapai the complex serves the much more general purpose of curing; and not only do spirits of mountains appear in the seance, but sought dreams experienced in caves in certain mountains seem to be the strongest source of Walapai shamanistic power. The Maricopa lack the specification of possession, and the function of the clairvoyance is enemy-locating, as among the Mohave. The Maricopa in fact have a second, parallel performance for this purpose; and they have the most systematized set of beliefs about spirits of mountains, both in contending for victory in war by their client tribes and in bestowing power on curing shamans.

Items of belief originate and disseminate to other tribes, associate or separate at home, in many different ways; they can even be introduced, like the Mohave tšekamitšk clairvoyance, and remain outside the general tribal pattern as a small independent complex. There is no particular order in the discrete units or items that occur in the beliefs or general culture of a tribe. Anything can flow in through proximity and contacts; almost anything can coexist somehow with what is already there, most often of course in the mesh of the established patterns of the culture, but also independent of them. There are relatively amorphous cultures, like that of the Walapai, in which much of the content floats free, or is organized into many loosely related small patterns. There are others, like Mohave, in which patternings are strong and extensive, and several of them are interconnected to embrace most of the culture. But even here there will be items and even small com-

plexes outside the great patterns. On sufficient acquaintance with the culture, these will be sensed as exceptional or even "contradictory" to its prevailing run. Analysis may then modify or extend the pattern they violate, or may sharpen the pattern by excluding the refractory item. It is of course the patterns that make the "style" of the culture—that which makes possible a specific and coherent characterization of it. But no culture can be wholly reduced to a style or set of patterns. There always remain not only bellies to be filled, and such like, but non-concordant "tšekamitškes."

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ABBREVIATIONS

AA	American Anthropologist
AAA-M	American Anthropological Association, Memoirs
BAE-R	Bureau of American Ethnology, Reports
UC-AR	Anthropological Records, University of California
UC-IA	University of California Publications: Ibero-Americana
UC-PAAE	University of California Publications in American Archaeology and Ethnology

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