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THE MEASUREMENT OF MENTAL FUNCTIONS
IN PRIMITIVE GROUPS

By FLORENCE L. GOODENOUGH

It is with some feeling of trepidation that I, a psychologist, come before
an audience of anthropologists to speak on a topic that is, in my opinion,
so generally misunderstood by both groups. Yet the source of the misun-
derstanding is not far to seek. It is to be found in the unwise and undiscrimi-
nating use of the term measurement when what is really meant is the classi-
fication of individuals on the basis of a sampling of their observed character-
istics.

The two things are very different. Just as the physical anthropologist
measures the external bodily dimensions of the members of a given racial
group and compares them with those of another group, so the psychologist
compares the length of time required to perform a given task and the num-
ber of errors made in doing so, or determines the minimal differences in the
size or weight of objects that the average member of each group can dis-
tinguish with accuracy, or counts the number of trials needed to attain a
given degree of skill when learning a new task. All these are measurements
properly speaking, and repeated studies have shown that they can be made
with a degree of precision that compares very favorably with most anatomical
measurements. It is in the inferences to be made from these measured
facts that the difficulties arise.

Near the end of the last century, in connection with the famous Cam-
bridge Anthropological Expedition to Torres Straits, Rivers, Myers, and
McDougall carried out a series of psychological measurements on several
native tribes that might well serve as a model for present-day investigators.
They measured various kinds of sensory acuity, including the visual, audi-
tory, olfactory, cutaneous, and kinesthetic senses; speed of reaction to vis-
ual and to auditory stimuli, using both the so-called "simple" reaction and
the "choice" reaction in which the subject is required to respond to one

1 Address given at the meeting of Section H (Anthropology) of the American Association
for the Advancement of Science, Minneapolis, June 28, 1935.
type of stimulus and to ignore others. They studied the ability of their subjects to discriminate small differences in pitch and gave tests for color-blindness. The latter are of special interest insomuch as color-blindness is believed to be a sex-linked hereditary characteristic; hence its relative frequency or infrequency within a given ethnological group is not only of interest in itself but might be of some value as an indication of racial intermixture. Recent studies of the frequency of color-blindness in different racial groups have in general confirmed the results obtained in the Cambridge Expedition in finding fairly marked racial differences in the number of color-blind individuals in general and in the proportions of various types of color-weakness. Much further work needs to be done both in the way of confirming the results obtained so far and in extending the investigations to other races and to geographically isolated groups.

One of the attractions of the St. Louis Exposition in 1904 was a so-called “Congress of Races” at which members of many of the chief ethnological groups were displayed. Just how representative of their respective tribes the individuals taking part in the exposition may have been cannot be stated with assurance, but at all events there were included among them members of a number of primitive groups whose abilities had never before been subjected to any sort of formal testing. The numbers, however, were small; in most cases fewer than ten. Woodworth and Bruner gave them a number of tests of auditory acuity, and a form-board test. With typical caution, Woodworth states that the latter may afford a “crumb of evidence” as to the relative intelligence of these groups.

It is rather unfortunate that these studies have been so widely quoted, for it would appear that the majority of people have been content to take the results at second hand and thus a number of misleading statements made in the earlier reports have been perpetuated. As a means of acquiring either material wealth or scientific information, the chain-letter method is not to be recommended!

The impression that seems to be current, at least among psychologists, is that the question of racial differences in sensory acuity was settled by these two early studies. As a result, very little further work in this field has been done. Now as a matter of fact, a careful reading of the original reports should convince any competent scientist that the question is not yet answered. Let me run briefly through some of the results. Rivers found the average visual acuity of Murray Islanders, as measured by the Snellen E charts, decidedly superior to the norms at that time accepted for Europeans. He interprets these findings with a good deal of conservatism, however, pointing out that the results of different surveys in Europe have
not always yielded entirely consistent results, hence the norms used could not be regarded as wholly accurate. He also notes that the lighting conditions under which his tests were made may perhaps have been more favorable to clear vision than those under which the European standards were established. All this is quite possible, yet the fact remains that the Murray Islanders, of whom 115 were tested, did better than any of the European groups with which they were compared. The normal five-foot line could be read by the average islander at a distance of eleven feet. Similar results were obtained for other smaller groups from near-by regions.

In his discussion, Rivers refers to the popular notion that primitive races are gifted with extraordinary sensory acuity. He points out that the basis for this belief goes back, for the most part, to traveller's tales of what seemed to them remarkable feats of sight and hearing. These feats, however, were carried out in an environment with which the savages were familiar while the white men who observed them were not. Since much of our seeing in everyday life depends upon the utilization of secondary cues, the meaning of which has been built up through experience, Rivers is inclined to think that the factor of knowing what to look for, where to look for it, and what supplementary cues can be relied on in interpreting what is seen has played a greater part in these apparently marvelous feats of sensory skill than has any difference that may exist in sensory acuity per se. The soundness of this view can hardly be questioned but it is unfortunate that the majority of those who have quoted it have been so impressed by its fundamental logic that they have failed to realize that Rivers did not offer this as an explanation for the results of his tests (which, it may be noted, would offer a much less familiar situation to the islanders than to the whites), but only by way of accounting for the popular reports which, if taken at their face value, would demand a visual sensitivity many times greater than that actually found. Thus the statement often made that Rivers found the islanders no better than, or inferior to civilized man in sensory acuity does not hold for the visual field. The only thing that can in fairness be said about racial differences in vision is that while we do not know with certainty whether or not such differences exist, the only objective evidence thus far available indicates a fairly marked superiority of certain primitive groups in distance vision. Because of the possibility of a difference in testing conditions this evidence is not conclusive. Further tests should be made.

The hearing of primitives was tested very carefully at the St. Louis Exposition by Bruner and Woodworth, and somewhat less thoroughly by Myers at Torres Straits. Both found the general auditory acuity to be somewhat below the average for whites. In the absence of medical examinations
to determine the comparative frequency of diseases of the ear, it is impossible to say whether or not this represents a true racial peculiarity or is merely the result of the conditions under which certain races have lived. Myers also found that the natives were inferior to Europeans in the ability to distinguish small differences in pitch. He found no differences between the islanders and Europeans of the same age in respect to the upper limit of hearing, that is, the highest audible pitch, but Woodworth and Bruner found that the six African pygmies whom they tested at the St. Louis Exposition surpassed the average of the whites in the ability to hear tones of very high pitch. One cannot place much confidence in this finding because of the small number of cases. However, in view of the small size of the pygmies which, in all probability, means that the fibers of the basilar membrane are also somewhat reduced in length, it is not impossible that we have here a true racial difference. At all events, the matter is worth checking.

Certain apparent differences in the cutaneous and kinesthetic senses were found by McDougall in his studies of the native tribes about Torres Straits. Tactile sensitivity, as measured by the two-point threshold, was about twice as great for the natives as for Europeans. Sensitivity to pain appeared to be only about half as great, but since this is a matter for which there is no objective check, it is not impossible that the islanders were more reluctant to admit pain. The natives also surpassed the Europeans in their ability to distinguish small differences in lifted weights.

In the light of this evidence it is somewhat surprising to find practically all modern psychologists holding to the belief that the question of racial differences in the elementary processes of sensation and perception has been adequately settled, and pointing to these very studies as evidence for their contention that no such differences exist. It is quite true that at the time these investigations were made, their authors were chiefly interested in ascertaining whether or not there was any foundation in fact for the widespread popular belief in the extraordinary sensory powers of primitive men. In their reports, therefore, the failure to find any such phenomenal cases as had been described by travellers, together with the fact that there is much overlapping of the races even within those sensory fields in which the difference between the averages is greatest, are the points that are chiefly stressed. Yet the fact remains that if we take the figures at their face value, the differences in central tendency are entirely comparable in magnitude to those that have been so painstakingly computed from the results of intelligence tests.

After the experiments of Woodworth and Bruner, little further quantitative work in the study of racial differences in mental traits appeared until
the second decade of the century, when the rising wave of interest in intelligence tests swept away the banks of scientific caution and flooded the psychological journals with a torrent of figures on racial differences in "general intelligence." Examination of the literature in this field over the past twenty years shows that approximately two-thirds of all the publications dealing with racial differences in mental traits have been concerned with the measurement of intelligence by means of tests designed for use with American or European whites, while a large proportion of the remainder have to do with the even more hazardous problem of comparing races with respect to traits of temperament and personality as indicated by their scores on tests designed to measure these qualities but of very doubtful validity, even for whites. Now the fact can hardly be too strongly emphasized that neither intelligence tests nor the so-called tests of personality and character are measuring devices, properly speaking. They are sampling devices. If the conditions that underlie the valid use of any sampling technique are adequately met, then such tests may not only have much practical usefulness in the guidance of individuals but may become valuable instruments for scientific research. But first of all we must be sure of the sample.

As long as we are dealing with measurements, the question of sampling presents relatively few difficulties. If our measuring instrument is known to be reasonably accurate, and if we apply it to a sufficiently large number of representative members of the group in question, under conditions that insure adequate cooperation, then we need have no hesitation in presenting our results as they stand. If, under these circumstances, one group is found to do less well than another, it is unnecessary to quibble about the fact, though we shall still be interested in ascertaining, if possible, what factors have played a part in producing the difference. But the difference is no less real, whatever its cause.

When, however, we leave the field of direct measurement, and endeavor to classify individuals or races on the basis of some presumably general trait that cannot be measured directly, we are faced with another and much more difficult problem of sampling. Not only must we be sure of the adequacy of our sampling of subjects, but we must also be sure that the test-items from which the total trait is to be judged are representative and valid samples of the ability in question, as it is displayed within the particular culture with which we are concerned. The reason that the ordinary intelligence test works as well as it does for American urban populations is simply because the items of which it is composed are fairly representative samples of the kind of intellectual tasks that American city dwellers are likely to be called upon to perform. The principle involved is essentially the same as
that employed by the thrifty housewife who takes a handful of beans out of the barrel from which she is to purchase a supply and judges the quality of the total on the basis of this sample. Considered as a sample, the intelligence test, with its variety of short tasks selected from out the infinite number that the individual is likely to be called upon to perform in the course of his daily life, differs from the handful of beans in only one important respect. The handful of beans is taken at random; the items comprising the intelligence test have been carefully selected with view to their representativeness for the cultural requirements of the group for which the test was designed. Such a selection is made necessary by reason of the fact that the amount of variation within the field of intellectual requirements is so very great that a random selection of tasks would necessitate so large a sample that the time required to try out all of them would be prohibitive. Nevertheless, the warrant for any judgment regarding the characteristics of the total complex of abilities of which the test is a sample or of the barrel of beans of which the handful is a sample rests upon the same fundamental assumption in both instances.

The wise housewife, engaged in a search for a good value in beans, would not make the mistake of judging the quality of one lot on the basis of a sample taken from another lot. She would not, moreover, make the further error of assuming that the standards applied to her judgment of beans are fully valid for the judgment of potatoes. Nevertheless, errors of both these types and particularly of the latter, are all too common in much of the published work on racial differences. A part of the difficulty, as I have indicated before, seems to be due to the unfortunate use of the term "measurement" in this connection. We may measure certain kinds of mental performance with an encouraging high degree of accuracy, regardless of the group upon which the measurement is taken. But the inferences to be drawn from such measurements will vary with circumstances. In view of the relatively small amount of information concerning the mental characteristics of various cultural and racial groups that is based upon direct measurement of simple functions it is greatly to be regretted that up to the present time, psychological interest has been so closely centered about problems of classification on the basis of broad general assumptions, the validity of which is uncertain.

It is unnecessary for me to review in detail the results of the many hundreds of studies that have had as their object the classification of racial groups on the basis of intelligence, personality, or what-not. Suffice it to say that the great majority have erred both in regard to the accuracy of the so-called "racial" classification and in respect to the representativeness
of the sampling of test-items for the trait that they are presumed to represent. Few people realize the extent to which small differences in customs may change the meaning of an apparently simple task. Let me cite an example of such a change that has occurred within our own culture and within our own times.

The Stanford Revision of the Binet tests has undoubtedly been more widely used than any other intelligence test that has been devised. Its value both as an aid to the practical understanding of individual children and as an instrument for certain kinds of scientific research has been so well established that it is not necessary for me to dwell upon it further. Although this test was standardized upon the basis of data collected more than twenty years ago, most of the items included within it can still be regarded as valid samples of the kind of intellectual tasks required of children in a culture such as our own. Yet changes have occurred within that culture during the twenty years, and as a result of these changes the intellectual demands made upon its members have also been modified in certain very definite ways. Now, one of the tasks that at the time of the original standardization was found suitable for children of six years is the question, “What’s the thing to do if you are going some place and miss your car?” To the average city child of six, in the years 1913–14, this question at once evoked an idea of the familiar street-car and well over half of them gave the conventionally correct answer, that they would wait for the next one. But to the six-year-old of 1935 the word “car” does not, in most cases, suggest a street-car at all, but an automobile. And when asked what is the thing to do when you miss your Ford or Chevrolet or Chrysler as the case may be, the six-year-old usually says he does not know and so he fails the test. A few, prompted either by personal experience or hearsay, offer the very intelligent suggestion that you should tell the police, a reply that is not in the book and that the majority of mental testers to whom I have submitted it with no other comment than to ask how it should be scored have also classified as a failure. Yet automobiles were familiar objects in 1914, and street-cars are still in existence. But the number of middle-class six-year-olds who have never ridden on a street-car is almost certainly far greater today than it was in 1914, and therefore, even though the term be explained, the difficulty of the task is, on the average, considerably increased, while its validity as a sample of the kind of thing the modern six-year-old needs to know is very much lessened.

This little example illustrates how impossible it is to think of a sample of any general function apart from the conditions under which that function is to be exercised. But if the culture-requirements are the same, then a
sampling device may provide a reasonably good measure of the relative ability of individuals or of specified groups to function effectively within that culture, though they may be poor ways of predicting how well the same persons would function if the cultural requirements were different. Looked at in this way, no very great error is introduced by comparing, for example, the children of different immigrant groups within this country as to their scores on so-called tests of intelligence, for insofar as the items making up these tests are valid samples of the intellectual requirements of American culture, a poor rating on the samples implies a strong probability that the total performance will also be poor. Even the possibility that the results may have been unfavorably influenced by imperfect knowledge of English need not concern us too greatly, unless the sample-tasks that comprise the test make a greater demand upon the knowledge of English than do the situations that must be met in everyday American life. Poor command of the language of the group with which one lives and works is a handicap to effective functioning within that group, as all of us who have travelled in foreign countries know from personal experience. So if we look upon an intelligence test, not as a mysterious device for getting at some fixed and unchangeable quality of an individual, but simply as a series of samples of the intellectual requirements of a given culture, then it is perhaps not unfair to apply such a test to all individuals who are forced to live and compete within that culture. The results would then be interpreted merely as present facts, or, more precisely speaking, as estimates with a certain stated probability of error. Of course if we adopt this point of view we must take intelligence testing out of the fortune-telling class. In saying this, I do not mean that we should wholly cease to make predictions of future development on the basis of intelligence-test results, but rather that we should clarify our thinking on the subject. It has been shown experimentally that the great majority of persons do not greatly change their relative standing on these tests during a period of several years. But some do change, at least as far as test-results are concerned. While it has been the fashion among most psychologists to look upon these changes as the results of errors of measurement, apparently because of the rather ostrich-like idea that changes which cannot be foreseen cannot occur, nevertheless the theory of the "constancy of the IQ" is simply a matter of the probability curve. Although most of the cases that have been re-examined cluster fairly closely around the midpoint of zero change, there is no valid reason for assuming that the extreme cases have been selected by purely fortuitous causes. A real change in ability may have taken place.

But if we are to look upon intelligence tests as samples of the intellectual
requirements of a given culture-group, what basis is there left for applying such a sample of tasks to individuals from another group whose cultural patterns differ widely from those of the original group for whom the test was designed? Very little, I think. About all that can be learned from such a procedure is that the cultures are different; in other words, that the tasks chosen are not representative of the abilities of the subjects. In this way we may be able to find out a little about what these people can not do, but it is not likely that we shall learn much about what they can do.

I am not, therefore, very optimistic about the amount of valid scientific information to be gained by comparing different culture-groups on the basis of such broadly defined traits as general intelligence, personality, temperament, and the like, for no one of these traits is absolute in meaning but each derives its system of values from the particular society in which it functions. Nevertheless, I believe that the study of mental functioning within various culture-groups, and particularly among those which afford the greatest contrast to our own, has many possibilities for scientific research if approached from the rational and direct standpoint of measuring different types of performance as they occur, and limiting generalizations to the particular functions studied. I have already indicated the need for further investigations in the field of sensation and perception. The question of dextrality, especially in its relationship to speech defects, is at present the subject of much controversy among certain psychologists. A thorough-going study of hand-and-eye dominance in groups among whom the preferential use of the right hand is more or less strongly established by tribal custom might help to throw light on this problem. Dudley Kidd tells us of certain Kaffir tribes with whom the use of the left hand for many of the everyday affairs of life is so strongly taboo that parents resort to the most violent kinds of corrective measures to overcome any signs of left-handedness that their children may show. He says nothing about the frequency of stuttering in these people, but if the cerebral dominance theory is right, it should be very common.

Except in a descriptive way, little is known about racial differences in motor skills and endurance. Garth has conducted several studies in mental fatigue among American Indians, but as the tasks set were of the schoolroom variety it is uncertain whether or not the more rapid work-decrement that was found for the Indians as compared to the whites can be ascribed to a lesser degree of endurance or to unequal difficulty of the tasks for the two groups of subjects, making it necessary for the Indians to expend a greater degree of effort per unit of time. Comparative studies of the work-decrement in different racial groups for various kinds of tasks selected from
the customary occupations of each of the different groups studied would be a more illuminating way of getting at the problem.

Only a few studies have dealt with racial differences in the ability to learn, and in most of these the tasks set for learning have been of the "White man" variety. A comparison of races as to their ability to acquire certain motor skills, such as throwing at a moving target, walking a tight-rope, or learning to run a stylus maze, or to open a series of puzzle boxes would be interesting. If one wished to extend the learning problems to the verbal level, one might invent a series of nonsense words of which the linguistic elements were selected equally from the languages of the two groups to be compared, apply these as names to a series of common objects, selected to include an equal number from both groups, and see which learns the series more readily.

Another type of investigation that holds much promise for the psychologist as well as for the anthropologist whose interest extends beyond the field of physical anthropology is the study of the behavior of the children of primitive groups by the use of more precise quantitative methods than have been common in the past. In the anthropological literature we have a fair amount of valuable descriptive material of this kind, but very little that has been collected in a systematic manner. Recent work in the field of functional development during infancy and early childhood has perfected a number of devices that might be utilized for comparative studies of the development of children in primitive groups. Examples that may be cited are the motion-picture studies of the development of fine prehension which have been carried out at the Yale Psycho-Clinic; the studies of the manner of solving simple problems of spatial relationship, such as those first developed in the work on anthropoid behavior by Köhler and later adapted for use with young children by Alpert, Kellogg, and others; the investigations of the process of learning to discriminate simple forms which have been used alike in studies of animals and young children. In the field of social psychology we might well consider the desirability of comparing the play of children of different ages and various ethnological groups, or of securing careful time-records of the division of activities over the twenty-four hours of the day for different groups and for individuals of varying ages and of both sexes. A body of carefully selected data of this kind would go far toward helping to interpret the differences in specific abilities that commonly appear in comparisons of racial groups and would also possess a great deal of intrinsic interest.

The foregoing are but a few of the many lines of investigation for which psychology has developed techniques that have a fairly high degree of
accuracy. For all of these, it should be noted that the methods of approach are simple and direct, involving no assumptions other than those of the adequacy and representativeness of the sampling of subjects studied and reasonable control of the conditions of experimentation, including the motivation of the subjects. Provided these conditions are met, the conclusions that can be drawn from such measurements are simple and direct. They have to do with conditions as they exist for a particular group at a particular time and under the conditions specified. Unlike intelligence tests or the so-called tests of “personality” and “temperament” which have been so greatly stressed by students of racial differences in the past, measurements of the kind that I have described are interpreted directly in terms of their actual face value, and are not regarded as indices of something else which is not subject to direct measurement. No assumptions are made as to whether the functions measured are innate or acquired, although it is true that the developmental studies just mentioned may in the course of time, when coupled with sufficient additional evidence, throw some light on this question. These studies, it seems to me, would be of very great value both to the anthropologist and to the sociologist interested in the development of cultural patterns as well to the psychologist. It is much to be hoped that in the years to come, psychological interest will be shifted from the present questionable practice of attempting to classify groups with regard to hypothetical “traits” of uncertain validity and will concern itself with the study of more basic abilities as they actually exist within different culture-groups.

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A STUDY OF A BALINESE FAMILY

INTRODUCTORY

Of a people numbering more than a million, divided into castes, into districts which have felt the varying influences of separate dynasties of rulers, and whose religion and social structure appear in all forms from the ancient Bali-Agā to the orthodox order of the Sivaite and Buddhist Pedandas, one would be bold indeed to attempt a study of the family. There are no statistics, and generalizations are almost certain to be, in some sense, false. But it is true that more than ninety percent of the Balinese are Sudra, the fourth and lowest Hindu caste, called in Bali djabā,¹ or outsiders. This is to distinguish them from the Brahmana, Satrya, and Wesya, who claim descent from the Hindu-Javanese invaders of Madjapahit, conquerors of the island in the fourteenth century. These djabā are, then, more “insiders” than outsiders, the real Balinese. They are organized into village groups, the desā, each with its council to which belong all married men who are householders. The desā has its individual adat (customary law), functioning quite apart from the laws laid down by the rajas of the past or the present Dutch administration. The village has a sub-organization into two or more bandjars, spatial divisions, each with its own meeting place for the men. The bandjars may once have been divisions of family groups, but now they resemble clubs, which rival each other in gamelan and theatrical performances, whose young men nourish a feud, and whose children dare not cross the invisible line which limits their own bandjar, lest they be set upon by “the other gang.” Religion to the djabā means the regular giving of offerings to the spirit of his ancestors, in his own sanggha or household temple; offerings to the gods in the village temples, under the tutelage of the Pemangkoe, a priest also of djabā caste; propitiation of demons and bad spirits, on the roadside before the house,

¹ Throughout this paper I have used the Dutch phonetic spelling for Balinese terms. This is the spelling in current use in Bali, taught in the schools.

dj is pronounced as j in jam
j is pronounced as y in yam
nj is pronounced as ny in canyon
tj is pronounced as ch in chew
oe is pronounced as oo in moon or u in put
e is short unless accented, as in French
i is pronounced as ee in deep
or as i in tick
a is pronounced as in father
final ā is pronounced as the final a in America

(djabā)
(wajan)
(njoman)
(tjenik)
(doesṭā, ketoet)
(ketoet, madė, metēn)
(bibi)
(tjenik)
(djabā)
(djabā)
at the crossroads, the graveyard; and only occasionally a pilgrimage to one of the temples of mountain-gods, sea-, lake-, and sacred spring-gods; proper and suitable celebration, with offerings and performances according to his means, of births, birthdays, tooth-filings, weddings, deaths, cremations, etc., in the family. As a rule it is only when some difficulty arises in the carrying out of the ordinary prescribed rites that the djabā turns to the Hindu high-priest, the Pedanda, for advice, and still more rarely, for his offices.

It has occurred to me that a study of one such ordinary Balinese—one of the 900,000 djabā—in his family relations, might be of value, not to lay down any rules, but simply as an illustration of certain common phenomena in the mores.

I have chosen, therefore, an ordinary family, in an ordinary village, lying halfway between the modern town, where the old customs are perforce breaking down, and the region of the old-style Bali-Agā villages, which in their isolation have preserved an order which is today more of a curiosity in Bali, not characteristic of the country as a whole. And so, although it must not be concluded that what has happened in this family is what necessarily would happen in any Balinese family, still the procedures may be taken as fairly typical—if one remembers that variations are great from village to village, and that the customs of the high-caste Balinese, as well as those of the Bali-Agā villages are not represented in the general picture.

A BALINESE FAMILY

Rendah is the head of his household, and therefore a pengajā, member of the village council. He owns his own pekarangan, a walled courtyard within the village limits, enclosing the shrines of his household gods, the living quarters of his immediate family, and the place of the pigs and fowls. He owns also a sufficient portion of the irrigated rice-fields surrounding the village for the maintenance of his family. The fields are tilled by himself, his brothers, and their sons when they are old enough. All the old people and the dependent women and children get enough to eat. If the crop is good, and there is rice left over, it may be sold in the market. The women cook and weave cloth (kamben) for the family, and carry up fresh water from the spring, night and morning. The small girls of five or six begin to help their mothers and aunts at this task, balancing a coconut-shell full of water on their erect heads. The little boys of the same age take charge of the ducks and the water-buffalo, driving them out into the inundated rice-fields at sunrise, driving them back to their shelters near the house-court, at dusk.
Rendah has one wife, Rich, who has borne him nine children. Three of these died in infancy. His eldest, a girl, he has just given in marriage to her maternal parallel second cousin, the eldest son of Gari, Rendah’s own paternal parallel cousin (fig. 1, I). This girl, Tinggih, has “followed her husband,” and now lives across the street in the house of her father-in-law Gari. Only five children remain in Rendah’s house.

Rendah himself was one of eight children, also borne to his father by one wife (fig. 2, II). The children, in the order of their birth, receive title-names, as follows:

- Wajan—First-born
- Madé—Second-born
- Njoman—Third-born
- Ketoet—Fourth-born.

If there are more than four children the series begins over again, and the fifth child is Wajan, the sixth Madé, etc. Every djabā must have one of these title-names, which precedes his given name. The given name is chosen by a system of divination through fire, from a group of names suitable to a child born on a certain day of the astrological calendar. It is rarely used, except to children and to inferiors in age and in social status. To use the title-name is always more polite, and compulsory for inferiors in age and in social status. Thus Rendah, although he is the eldest son, was the fourth-born of his family. He is called Ketoet by his equals, or I Ketoet Rendah when they are speaking about him, to distinguish him from other Katoets.

After the birth of his first child he is called ‘Pan Tinggih,2 father of Ting-

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2 ‘Pan is the abbreviation of bapan, from bapā, father, with the possessive -n. In certain mountain districts the old word ‘nang, shortened from nanang, father, is used. Similarly bibi for mother or aunt, is sometimes found instead of the usual mêmé. On the island of Noesă Penidâ, belonging to Bali but with a distinctive dialect, bibi and bèbè are in use for mother and aunt as well as mêmé, and maman for paternal or maternal uncle, reserving nanang for the biological father. In Balinese classical literature paman is used for uncle and as a familiar form of address to older men (as a young prince speaking to his father’s minister). In Malay paman may be used in the latter way, though its original meaning is maternal uncle. Iboe, mother, and jajah or adjî, father, occur also in the literature. Iboe and adjî are current high Balinese, and biang, mother, is found among high-caste people used as a title-name. Keramâ is another old form for mother, found in the texts. Kaki, grandfather, appears in the literature more often than the current pekak. Nini and nénê, old forms for grandmother, are rarely found outside the texts. Nini is still in use as the name for the rice-goddess of the harvest ritual, although apparently not universally known.

Koeranan is used for both husband and wife. The words moewani, male, and loch, female, are commonly substituted in low Balinese for husband and wife. In high Balinese they are lanang, male, and istrict, female; istrict has come to have a more definite meaning of wife, although high-caste Balinese have many different words, as rabi, wife, prami swari, consort (of
glih, and this appellation is still more polite and proper to be used by strangers, equals, and inferiors. All his younger brothers and cousins call him bli, elder brother. His own children, his nephews, and the children of his first and second cousins on both the male and the female sides call him bapā, father. In speaking of him, the children of his younger brothers and cousins may call him oē, elder uncle (or aunt). His wife generally calls him bli, elder brother, or Ketoet, his title-name, and sometimes even bapā, as the father of her children. She, as his inferior, may not call him by his given name, Rendah. But he may call her by her given name, although he is more likely to use her title name, or to call her mêmē, mother. When Tingglih’s first child is born, Rendah and Rieh will be known as Pekak — and Dadong —, Grandfather of — and Grandmother of — followed by the given name of the child. Thus a man’s own name tends to die out even during his own lifetime, as he grows older, and there is no one left to “speak down” to him. During this investigation I asked Rendah for the name of his own mother, who lives in the house with him. He could not remember, and was forced to inquire of the old lady herself. Her husband, Rendah’s father, had been dead only six months. He alone of the household had the right to call her by her given name, and if Rendah could not remember it, the old man must not have availed himself of the right for many years.

Rendah’s eldest sister, Wajan Koti, was married, and later “thrown away.” She returned to her father’s house. As Rendah has inherited house and household, she now lives within his courts, sharing in the work of the women.

ew prince), penawing, wife from lower caste, and sang keringan, concubine (“married to the kris” when already pregnant). (Princes may take concubines without marriage ceremonies, but if a concubine is going to bear the prince a child, the ceremonies are carried out with the substitution of the prince’s kris, borne by his high priest, in place of the bridegroom—so that the child may be made legitimate, although the mother is of lower caste.)

Brothers, sisters, and all relatives are called njamā (low) or semotonan (high). Elder brothers and sisters are called kakā. To address an older sister, or in polite usage any female older than one’s self, mbok is used; to address younger brothers, sisters, or relatives, or in speaking of them, adi is used.

Child in the sense of “the child of ——” is pianak. For “a child” or “children,” there is no word. Anak tjenik (polite) or anak tjerik (familiar) are substituted, which mean simply “small man.” Anak in Malay means child in either of the above senses, but in Balinese it means man or person, as anak Bali, a Balinese, anak loeh, a woman.

Grandchildren are called tjoetjeo, great-grandchildren kelab or boejoet. Male and female great-grandparents are called koempi or koempiang.

Finally there are the terms matoē, father-in-law, mantoe, son-in-law, and ipah, used for either brother- or sister-in-law.
The second sister, Madé Njambléh, was given in marriage to the son of her father's male paternal cousin (fig. 1, II).

The third sister died at ten years.

Fig. 1. Genealogies of Balinese families.

After Rendah came the fifth child, a boy, Wajan Mertá. This boy, on his marriage, moved out of his father's courts, and set up a household for himself farther down the road (see map, fig. 3). Thus he left Rendah, as
Fig. 2. Genealogies of Balinese families.
the eldest son, at the paternal house with his wife and children, to succeed the old man at his death. But this does not always happen, and as we shall see in other cases, it is more usually the older brothers who move out one by one and set up new households, leaving the youngest boy to succeed and take over the paternal house. (Daughters do not inherit property.) It may be that the desire for independence, when newly married, prompts the elder brother to depart, leaving his right of succession to the youngest.

Fig. 3. Map of Bandjar Koetoeh, Village of Sajan. 1, House of the great-grandfather, Gentoeh-Njade-Keroena; 2, House of Géwar-Klepoeg-Gari; 3, House of Bedil-Dari; 4, House of Boedal-Rendah; 5, House of Mertá; 6, House of Soewaká; 7, Balé Bandjar of Koetoeh, meeting house of the men; 8, Poerà Dalem, temple of the dead; 9, Poerà Panataran, temple; 10, Poerà Désa and Poerà Poseh, village temple and temple of origin (common to the five bandjars); 11, Peken, village market place; 12, Poeri, “palace” of the Tjokordá; 13, Balé Bandjar of Bandjar Mas; 14, Poerà Nagasari, temple; 15, Poerà Panti, temple; 16, Semá, village graveyard and cremation place.

They say also that the youngest son has had, at the time of his father's death, less time to set himself up, and is therefore less able to fend for himself, than the others who have been married several years. Whatever the reasons, the rule is that the eldest or the youngest, not the intermediate son, inherits the paternal house. (See exception, pp. 23–24.)

The sixth child was another boy, who died in childhood. The seventh, Njoman Dog, another boy, who, although married for several years, has remained in the paternal house. He and his wife Rawi are now a part of Rendah’s household. In Balinese this is called ngerob, “to share a kitchen.” They say it is because Rendah is not strong, and Dog helps with the cultivation of Rendah’s ricefields in return for sharing in the house. Rendah seems to have complete authority over Dog, and orders him about as his
younger brother, putting most of the heavy work upon him. But it is true that Rendah is not strong and that Dog is not very bright.

Dog and his wife Rawi have no children. This is a tragedy to the Balinese. Rendah has recently allowed them to “adopt” one of his little girls, Dajoeh. It would not seem to be a very serious change for the child, as they all live together in the same court, and she, with her brothers and sisters, has always called their aunt and uncle by the same names as their mother and father, mémé and bapâ. However, Dajoeh senses a difference, and follows Rawi about, always at her heels like a young colt.

The eighth child of Rendah’s father was a girl, Ketoet Djinar, who married and went to live in Njéstanan, a different bandjar of the same village. She is in and out of Rendah’s house, helping when there is heavy work with the harvest, rice-pounding, or special preparations for a temple festival. But she neither eats nor sleeps there regularly.

The mother of all these children, whose name, she remembered, was Kintil, is of course also a member of the household. She continues to sleep in the place of honor, the balé sâkâném, which in her husband’s time she shared with him. Although the old man had been for several years in his dotage, unable to fulfill his functions in the village council (where he was represented by Rendah), and rather pushed aside by his grandchildren, Kintil is still a vigorous old woman in full possession of her faculties, respected and esteemed by all the household.

On the plan of Rendah’s house (fig. 4) it will be seen that of the space enclosed by the walls the “highest,” that is, the northeast corner, towards the mountains and the abode of the gods, is reserved for the household shrines. The balé sâkâném is an elevated open pavilion, on a stone foundation, whose thatch roof is supported by six wooden pillars. There is a brick wall at the back, the other three sides are open. The space between four pillars forms a sleeping-place, a raised platform built between the pillars. Here the grandmother sleeps, sometimes with one or two of the children.

The metèn is a house with mud-walls enclosing two sleeping platforms, and outside, on the porch formed by the overhanging roof, are two more sleeping places, with curtains of red print material. Newly married young people, and young unmarried girls sleep in such inner rooms—they have reasons for privacy, or must be protected. Here the two girls Ngetis and Dajoeh usually sleep. Rendah sleeps on the porch, in the “higher” bed, towards the east, often with one of his little sons. His wife occupies the other outer bed. The baby sleeps with her, or in fact any of the younger

2 The term is anak ngidih, “asked-for person.”
children, who have no fixed sleeping places, but are apt to visit about with their elders.

In the balé siang-sangā, which is a sort of family sitting room, also without walls, a place is found for the younger brother Dog and his wife Rawi. There are three platforms, and the children may also sleep here if they please. When guests are spending the night Dog and Rawi give up to them the largest bed, on a slightly higher level, to do them honor.

![Diagram of the house of Rendah](image)

**Fig. 4.** Plan of the house of Rendah. 1, Balé Sākāpat, pavilion with four posts; 2, Metĕn, house with inner room; 3, Sanggah, household temple; 4, Balé Piasan, pavilion for offerings; 5, Balé Siang-sangā, pavilion with nine posts; 6, Balé Sakanem, pavilion with six posts; 7, Paon, kitchen; 8, Djineng, rice-storing place; 9, Aling-aling, magic wall; 10, Kori, gateway; 11, Pigs. A, Koti; B, Dajoeh, Ngetis; C, Rich, Tjeremė; D, Rendah, Adā; E, Dog, Rawi; F, Kintil, Dapet.

Koti, the eldest sister who was married and divorced, sleeps in a little room in the balé sākāpat, which is reserved for her.

The kitchen stands opposite to the metĕn. East of this is the high pointed-roofed djineng, the rice-storing place. Behind are the pigs and fowls. Immediately opposite the entrance gate, blocking the court from the view of passersby and the attacks of evil spirits, is the aling-aling, a single strip of wall. The gate is a sort of stile, covered over by a thatched roof—you go up steps, through a doorway, and down more steps onto the roadway without. Cut into the mud wall on either side of the gateway are small niches used for offerings.
The father of Rendah was called Boedal. He was one of five sons who were, in order of birth, Bedil, Boedal, Klepoeg, Krèbèk, and Siboh (fig. 2, I). All five served during their adolescence as parakans, temporary slaves or attendants, to the local Tjokordâ, a Satrya prince. In return the prince made them grants of land when they married. He was a prince famous for his generosity. “One was ashamed to ask anything of him, for he gave so much.”

The father of these five was called Géwar. No one remembers the name of Géwar’s father, nor of his elder brother, who figures in this study (fig. 2, I). All that we know about the old man, who was Géwar’s father and Rendah’s great-grandfather, is that he lived in a house next to the old “palace” (now disappeared) of the Tjokordâ in Bandjar Koetoeh, the same bandjar which Rendah and his cousins still inhabit. Géwar was born in this house, but he did not inherit it. After his marriage, to Raras, also of Bandjar Koetoeh, he lived in the house marked 2 on the plan (fig. 3). Possibly this was a grant of the Tjokordâ. In those days the region all about the village of Sajan was wilderness, covered with forest. When families grew and the young men wished to set up for themselves, they had to clear the ground for their housecourts and ricefields, level the land into terraces, and extend the canal system to bring water to their fields. This tremendous task must have been carried out by groups. It is known that the parent village of Sajan was Peliatan, a village at a distance of five or six miles, and that in the time of Rendah’s grandfather and great-grandfather offshoots of many Peliatan families, the overflow from a district already fully populated, pushed out to form the new settlement of Sajan.

The five sons of Géwar, then, were born in House 2. The eldest, Bedil, moved out from this house, and built House 3 at the time of or shortly after his marriage. He had five children, the three eldest, girls, the two youngest, boys (fig. 2, II). They are Rendah’s paternal parallel first cousins. It is to the son of one of these cousins, Gari, that Rendah has married his eldest daughter.

The eldest daughter of Bedil, Méweh, became the second wife of a man called Soewakâ, who was a sentanâ to his first wife. A sentanâ is a young man, generally a younger son, who marries the daughter of a man without sons. He then becomes the successor of his father-in-law, lives in the house of the bride’s parents, makes offerings at the household shrine of her ancestors, and foregoes his own family. He inherits from his father-in-law house and land, and his children continue the line not of his parents, but of their maternal grandfather. In this case the man is said to “follow the woman,” and becomes a member of her family, just as in the usual marriage
the girl gives up her own family and her ancestor gods for those of her husband.

Soewakă was the son of a man whose elder brother had no male child, but a daughter. Soewakă married as a sentană this daughter, Metoe, who was his paternal parallel first cousin. Her father was then to him father-in-law, uncle, and adopted father (fig. 1, III). Soewakă lived in the house of his father-in-law and later inherited it. Soewakă had only one child by Metoe, a daughter. He then took a second wife, who was Méweh (eldest daughter of Bedil and paternal parallel cousin of Rendah), and had by her five children, two of whom have married back into the family which concerns us here (fig. 2, IV).

It is customary, and considered more suitable, for a man who takes a second wife to provide her with a house of her own, so that the two wives, at least during the early years, will not have to live in too close proximity. Soewakă built for Méweh what is called a koeboe, an informal sort of house, set out in the ricefields, not within the circumscribed limits of the village. Here Méweh still lives, although her children are grown up and married. With her are her second son, Tèmplok, with his wife and two

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4 A Balinese states the custom for choosing a sentană: "Take the family nearest on the male side, and if there is none [no son], only then can he be taken from the female side" (Ida Bagoes Hadipta, in an article on ancient adat in Bhawanâgara, Nos. 4–5, 1933, p. 73).

5 Household and dependents of Soewakă:

"Town house"
Soewakă
Metoe his first wife
Popol youngest son of Soewakă by his second wife
Te'er wife of Popol
child of Popol and Te'er
Sampring mother of Metoe

Koeboe No 1
Méweh second wife of Soewakă
Tèmplok son of Soewakă and Méweh
Kèbek wife of Tèmplok
2 children of Tèmplok and Kèbek
Poepoel son of Soewakă and Méweh

Koeboe No. 2
Loe'ed daughter of Soewakă by Metoe (first wife)
Gemoeh husband of Loe'ed
6 children of Gemoeh and Loe'ed.

The total is twenty people, eleven adults and nine children, living in the three houses, but receiving their share of the common rice supply, kept in the "town house." Soewakă, and all the young men of the three households, work the ricefields which he inherited from Metoe's father.
children, and her third son, Poepeol, who is unmarried. Her youngest son, Popol, lives with his wife and child in the "town house" with his father and Metoe. Metoe's daughter Loe'ed lives with her husband and her six children in another koeboe also on her father's land, for the husband is poor and landless, and glad to work his father-in-law's fields in return for his share of the crop. (He is not a sentanā, as Méweh has sons enough to carry on the line. Such a working relationship is called silih, which has the meaning of borrowing or repaying a debt, in friendly exchange.) Soewakā says that all these people, that is the two wives with all their children, sons- and daughters-in-law, and grandchildren (with the exception of the daughter Sēmbrod and the son Keroenā), and including also the old mother of Metoe, all these people "get rice at his house." But those who live in the koeboes cook it in their own houses. Here the term which was used for the dependence of Dog in Renah's house, ngerob, does not apply, for although they share the crop, they do not share a kitchen. Nevertheless Metoe's portion, which came to Soewakā as sentanā, has sufficed to maintain his entire second family, as well as Metoe's daughter, with her husband and six children.

Keroenā, the eldest son of Soewakā and Méweh, also became a sentanā. He married Saboh, the only daughter of Njadet and Njamblēh. Njamblēh is his mother's paternal parallel cousin, and Njadet the son of his mother's father's paternal parallel cousin (fig. 1, II). It will be remembered that Njadet and Njamblēh were the children of paternal parallel cousins (p. 16).

The youngest son of Méweh, Popol, has married his maternal crosscousin, Te'er, the daughter of Gari (fig. 1, IV, a). But this description, though biologically true, does not quite fit the case. Gari, born the brother of Méweh, was adopted at the age of three months by a childless uncle, Klepoeg. He became, then, by adoption, Méweh's paternal parallel cousin, instead of her brother. The marriage of Popol and Te'er is to be considered socially as another marriage of the children of paternal parallel cousins (fig. 1, IV, b).

There is one more child of Méweh who concerns us here, the daughter Sēmbrod, who married the son of her father's male paternal cross-cousin (fig. 1, III). Her case differs from that of her brother Popol, in that her relation to her husband sprang from an original cross-cousinship, carried through a parallel phase in the second generation; and Popol's relation to his wife (considered socially) sprang from an original parallel cousinship, carried through a cross phase in the second generation. Note that the description of Sēmbrod's case holds true whether she is considered as the
child of the line of Gongang, Soewakâ’s own father, or of the line of Mentik, father of Metoe, who became Soewakâ’s social and religious father when Soewakâ married Metoe as a sentanã. It is impossible for a Balinese to state definitely to which line Sembrod belongs, as she calls both Mentik and Gongang pekak, grandfather, just as she calls all her uncles bapâ, father. Even Rendah, who is her mother’s cousin, she calls bapâ. That is why the matter of inheritance is significant in tracing the emphasis of kinship, for it is only after the fact of succession to property that one can be sure where the emphasis lies. In this particular family, of Metoe-Soewakâ-Méwéh, complicated by the sentanã marriage on one side and the ordinary marriage on the other, we shall have to await Soewakâ’s death to see what transpires. One can only note probabilities from the present distribution of the family in the three houses, the “town house,” and the two koeboes in the fields (cf. fn. 5 and fig. 2, IV). Metoe’s only daughter Loe’ed did not marry a sentanã, as might have been expected, although her husband is a poor man. They do not live in the town house, but in a koeboe of their own on Soewakâ’s land. There is no chance of their inheriting the town house, since succession does not go through daughters. Probably they will inherit a tenancy right to a portion of the ricefields. Méwéh’s eldest son, Keroenã, married as a sentanã into the family of Njadet, and therefore has no further claim on his own family, but will inherit from Njadet. He is already pengajã, member of the village council, for that household, because Njadet has become too deaf to fulfill his functions. Méwéh’s second son, Têmplok, lives with his mother in her koeboe, and he is pengajã for that household. Poeopoel, the third son, also lives there. Probably either Têmplok or Poeopoel will inherit that house, perhaps share it. In any case they have no chance for the town house, as they are intermediate sons. The daughter Sembrod is now a member of her husband’s family and can expect nothing from her father’s estate. This leaves Popol, Méwéh’s youngest son, who would seem to be the logical heir to the town house. As we should expect, we find him living there with his father and his co-mother Metoe, ready to take over the rights on the death of his father.

Let us go back now two generations to the family of Géwar, where we find another curious case of inheritance, not conjecture this time, but accomplished fact (fig. 2, 1). It will be remembered that Géwar had five sons who served as parakans of the local prince. He lived in the house marked 2 on the map. His eldest son, Bedil, after his marriage, lived in house 3, and the second son, Boedal, in house 4 (which is now Rendah’s). It was the third son, Klepoeg, who inherited the paternal house, No. 2.
It came about in this way: the Tjokordā, prince of the district, was waging war on the region to the south of Sajan. He was victorious, and the inhabitants fled, leaving the country deserted. Krēbēk, the fourth son of Géwar, was sent away by the Tjokordā to Bandjar Toenon, in the conquered district, as a settler. He was given a grant of land, and therefore did not succeed to any of his father's lands in Sajan.

The fifth son, Siboh, who might have been expected to inherit the paternal house, was married as a sentanā, and moved away to the house of his father-in-law in Bandjar Mas, thereby forfeiting his claim. And so it was that the third son, Klepoeg, was the only one left to inherit the house of Géwar. His two elder brothers already had houses of their own, and his two younger brothers had departed from the bandjar before their father's death.

Now Klepoeg had the house, and he took two wives, but he had no children. This was an important matter not only to him, but to the entire family. For although each pekarangan must contain its individual household temple, the sanggah of the paternal house is larger, and all the children and grandchildren feel a responsibility for keeping up this parent-temple, whence their own house-temples spring. Surely the spirit of the forefathers is more powerful there! And so an heir must be found for Klepoeg, who has inherited the house of Géwar and become guardian of his sanggah. The eldest brother, Bedil, who has already three daughters, hands over to Klepoeg his first-born son, at the age of three months. This child, who is Gari, still lives in the house of Géwar. He has inherited his grandfather's house, not as the child of the eldest son, but as the adopted child of the youngest son who remained at home.

The house in which Bedil lived has passed on to his younger son Dariā, who lives there now with his wife and four children (House 3).

It is difficult to trace the inheritance of property other than the dwelling house. For the ricefields are loaned, mortgaged, paid for in labor, or held by a system of tenancyry with a sharing of the crops, all arrangements made between individuals, rarely with a written agreement. Within the family land is loosely held, and a man will speak of as his all property belonging to himself and his brothers, even of his cousins. Princes claim as theirs land which may have been given as a grant by their father or grandfather in return for services already rendered, or else let out on a crop-sharing basis, or even land originally owned by the commoner, but which the prince has planted with lines of his own coconut trees between the fields. The Balinese have an extraordinary memory for their relative indebtedness to every individual of their acquaintance, and the balance of favors given and
received swings back and forth through the generations. But all the same, when quarrels arise, the present government has difficulty in settling the ownership of property to which one man may hold the deed, whereas another has been farming it and paying the taxes on it for ten or fifteen years.

In the case of Rendah and his two surviving brothers, the ricefields of their father are supposed to have been divided between them. Rendah, as the eldest, claims to have got a larger share than Mertā and Dog. Now Mertā maintains a separate household, whereas Dog lives in the house of Rendah, and has no child (except the daughter of Rendah whom he has adopted). Rendah says that two-thirds of the land belongs to him, but that as the land is all lumped together, and worked chiefly by Mertā and Dog, they divide the crops between the two households, that of Mertā and that of Rendah, which includes Dog. Rendah's household consists of six adults and five children, Mertā's of two adults and seven children. "If he has not got enough rice, I give it to him, whereas if I am lacking, he gives to me," says Rendah. Hence we assume that the division of the crop is done on a friendly basis according to necessity, and if Rendah's household should diminish through the death of the old mother and sister, or the marriage of his daughters, and Mertā's increase with the birth of more children, the ratio could be proportionately altered. But it is hard to tell, when Rendah says he owns two-thirds of the land, whether this is really all his, or whether he is counting with his own Dog's share, since Dog is in the position of a dependent in his household. In this case it is of no particular importance, as Dog has no son to inherit from him.

It will be noted that of the eleven marriages recorded in Rendah's generation, only two were polygynous. Njadet married Dartā, and having no children by her, took his paternal parallel second cousin Njamblēch, who gave him one daughter. When she grew up, a sentanā marriage was arranged with her cousin Keroenā, who is related to her both on her mother's and on her father's side (fig. 1, II). In the son of this marriage, Njadet has at last got his heir to carry on the line. The other marriage of two wives was when Soewakā, who, as sentanā husband to Metoe, had only a daughter, wedded his second cousin Méweh as a second wife, and by her got four sons.

In the preceding generation Klepoeg and Krèbèk had each two wives.

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6 The deed, called pipil, is in writing, engraved upon a leaf of the tal palm. It is tucked away in a safe place, and forgotten. In the case of land which I rented, the search for the pipil lasted six weeks. It was finally discovered in a roof-bamboo of the house of the paternal second-cousin of the "owner."
Klepoeg's first wife seems to have been unsatisfactory, and was divorced. She had no children. Neither did her successor, Gijoer. Klepoeg obtained an heir by adopting the son of his brother. On Krébek, who moved away to another district, we have no information beyond the fact of his marrying twice.

It is of course impossible to draw any conclusions on polygyny from such a limited study. The cases mentioned only illustrate how the taking of a second wife is more strongly motivated by the need for sons than by an affectional urge towards the woman. It would be expected that the young men for whom initial marriages have been arranged would later wish to exercise an independent choice. But, as we have seen, even the second marriage may be an arranged one, between cousins. The Balinese say that a poor man who has one good wife can ill afford to take another. As a rule any wife who is not unfaithful, and who has borne a son, is considered "good." It is the son, in Balinese culture, who is of most importance—a social and religious necessity.

ADDITIONAL REMARKS

It is generally said of Bali that marriage is by abduction, the husband subsequently making a payment to the bride's father, varying from 10,000 to 50,000 kepangs (value approximately 12 to 65 Dutch guilders). It is true that this custom is widespread, and prevalent in the districts around Den Pasar and Boeleleng, the big townships. It may be found surprising that no such marriage is mentioned in the study of Rendah's family. There is much evidence to show that the form of arranged marriage practiced in Rendah's family, planned to link the households of relatives and friends, and without monetary compensation, is the older and more characteristic procedure. In fact, the old men of Sajan say that in their generation (before the Dutch occupation) girls were often stolen from one village by the young men of an enemy village, but that, instead of the husband's making a payment to the bride's father, the father would make a payment to the young man so that the girl should be returned to her village and the marriage dissolved. To have one's daughter marry in a hostile village was to lose sight of her completely. They say that if the match is suitable the father asks no payment, and only if he were angry would he require compensation of the bridegroom. Cynical and commercial-minded young men of the towns say, "the father always manages to be angry, so that he will get the money." But for the old-fashioned people of Sajan this is not true.

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7 Certainly it is considered decorous for the father to at least pretend anger at the ravishing of his daughter, just as the girl herself must weep and struggle to escape at the time of her capture, even though she has previously agreed, and helped to plot that capture.
Rendah said to me, on the occasion of his daughter’s marriage to her cousin, whose house is across the street, “I would rather roegi [take a financial loss] than to have my child marry away from home. If she married in another village, I should have the money, but my heart would be sick.”

Such a remark, reflecting a sentiment of paternal devotion, must not be taken only at its face value. The Balinese, like other races, often say things which “sound well,” but do not render their complete thought process, and are therefore misleading. It is not to be assumed that a man like Rendah, in giving away his daughter to her cousin, is actually casting to the winds a profit of ten or twenty thousand kepangs, having weighed them as nothing beside the possibility of losing sight of his child. Certainly he would prefer to have her near home. But to have her marry into a family with which he already has an established relation of favors given and received is more satisfactory than to set up a new link with a family of comparative strangers. And if he should accept compensation, he would be sanctioning a system which would not profit him in the end, for might not his own sons then have to pay for their brides? It would seem that these indebtedness relations are best carried on within the family group.

In connection with this it must be mentioned that sister-exchange between parallel cousins, maternal or paternal, is not permitted. This statement was made to me by various informants. None mentioned the prohibition of sister-exchange between cross-cousins, nor between unrelated families. I did not find any reference to sister-exchange in Balinese texts. The term used for the forbidden sister-exchange of parallel cousins is malik-terbalik, meaning a reversal, a going and coming. Malik may be a Balinese form of the root balik, although balik or walik are those commonly used in other connections; terbalik is definitely a Malay form, foreign to the Balinese language. But as many informants speak Malay fluently, and consider it a more polite usage to foreigners, the form of the term is not a proof of the late introduction of the taboo.

The difference in the two marriage schemes, that of the arranged marriage as against the marriage by abduction, reflects on the divorce. A woman may be “thrown away” by her husband for various reasons: she is barren, quarrelsome, or unfaithful. She may either return to her father’s house, or, if she has a lover, marry him. Whether or not a payment has been made to the father, the father does not have to make any restitution to the husband in case the marriage fails. But the lover or second husband must in either case pay compensation money to the wronged husband. This is rather more expensive than the cost of an unmarried girl, for besides the cost of the woman, a fine must be paid to the community.
This is part of the old village law, now taken over by the courts—a sort of punishment and compensation for disturbing the “balance” of the social group. I have known a case of an unusually fickle woman, who passed from one husband to another in such rapid succession that none had had time to pay for her before being deserted. Husband No. 4 found himself sued for a sum in which each of her previous husbands had a share: No. 4 was to pay No. 3, who owed No. 2, who still had not paid No. 1. The father also had a claim, but with faint hopes of recovery.

A wife who is maltreated by her husband has the option of returning to her family, where she will be graciously received if they do not consider her in the wrong. At the present time, if she can prove her husband’s delinquency in the courts, she may obtain some right over the children. However, even under the old regime, a husband has been known to allow the wife to bring up the children in a separate establishment, although they are considered chiefly his, and the descendants of his ancestral line.

There was the case of a girl for whom a marriage was arranged with her cousin, the son of her father’s elder brother. Her father had died, leaving his wife with four girls and a baby boy, and no one to provide for them. The cousin’s branch owned house and land, whereas the girl’s family had nothing, not even the space on which their house stood. Her father had occupied the land as tenant-farmer to the Tjokordā, and after his death, as there was no man left to till it, the land was forfeit. Obviously the marriage of the eldest daughter to her cousin would have been advantageous, and all her relations urged her to agree to it. The wealthy branch could have advanced the money needed to buy the land on which her house stood, and thus saved the house for her mother and sisters, and eventually a heritage for the small brother. But the girl resisted all persuasion to the match. Finally the grandmother resorted to a ruse. She came to the girl, asking her to prepare medicine for her cousin, lying ill with a fever. The girl collected the necessary herbs and spices, pounded them together, and brought them to her cousin’s house. Although she hated him, she could not refuse to help a relative in illness.

“He lies within,” said the grandmother, indicating the metèn, the room enclosed with walls. “Take it in to him.”

The girl entered the room. At once the old woman pulled the door to, and bolted it on the outside. The young man leapt up from the couch to embrace his unwilling bride.

For several days the girl was kept a prisoner. When they began to relax their vigilance, she ran away, but her cousins always pursued her, and brought her back. They believed that once used to the young man, she
would give in. After a time she did relent, and when a month had passed, agreed to the purification ceremony, mesakapana, which solemnizes a marriage. But within three days of the ceremony she could stand no more, and again escaped. It was too late to return to her home, especially as her mother and sisters had been in favor of the marriage from the beginning. She ran away to the town, where she lives at the present time, kept by an Arab merchant.

This case suggests that the women can, and sometimes do, rebel against marriages arranged with their cousins. Certainly with the peaceful times under the Dutch administration, when it is safe to visit other villages without fearing assault, and the increased freedom of communication by motor-bus and bicycle, the young people are more apt to resist an unwelcome marriage. There is infinitely more opportunity for a free choice, based on mutual attraction, than there was. In the old days, living in the enforced isolation of the village surrounded by hostile camps, if a girl refused her cousin or neighbor she might get no husband at all. Also, she was the more likely to be pleased with him, so few were the youths of her acquaintance with whom she might compare him.

We have seen that marriages of first and second cousins, both maternal and paternal, are frequent, and that no special distinction is made between parallel and cross-cousins. In Balinese the name for them is the same: misan, first cousin, mindon, second cousin, whether paternal or maternal, whether cross or parallel. Marriages of third cousins are more difficult to trace, as there is no term for them other than the general term for brother, relative, njama (low Balinese), semotonan (high Balinese).

The ordinary incest taboos are as follows: a man may not have sexual connection with his mother, his sister, his half-sister, or the mother of his half-sister or half-brother (that is, his mother’s co-wife). The penalty for this first degree incest, according to records of ancient adat, includes immediate banishment from the formal limits of the village, a great purification ceremony with animal offering to the bad spirits, and sometimes a rite whereby the offending pair are dressed in the yokes worn by pigs, and made to approach on all fours and drink out of a pig’s drinking trough. Subsequently the two are to be banished from the village for life and their lands confiscated. As such banishment in the old days left them no recourse but to wander off in the jungle, there to be hunted by wild beasts or to face death from starvation, the punishment amounted in fact to a death sentence. No other village would take in people banished from their own

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8 Recorded as part of the adat of the village Boeoengan, district of Bangli (Ida Bagoes Made Hadipta in Bhawanagara, Nos. 4–5, 1933, p. 71).
village, lest they bring ill luck and disaster. It can be imagined that the infringement of this incest law has not been frequent.

Less serious punishment falls upon the offenders in second degree incest, which is (1) sexual connection of a man with his aunt, either his father's or his mother's sister; or (2) connection with his first cousin once removed, that is his father's or his mother's first cousin. The first is called reramā dimisan, "mother at (or of) the first cousin," the second reramā dimindon, "mother at (or of) the second cousin." Offenders in these cases are banished from the village for a period of a year (420 days), but are allowed to return to the village after the completion of the necessary purification rites. They spend the intervening year in a hut put up for them on some unholy ground, the graveyard or the crossroads, and are not allowed during that time even to enter the village limits. It will be noted that both relations have the mother element reflected in the terminology, and that the man belongs to the generation younger than the woman, and therefore stands with her as a child to its mother. Similar relations but reversed, as of a man to his niece, or to his first cousin's daughter, are not forbidden, and entail no banishment, according to the old adat. There is here a feeling that the male should be older than the female, as he is her superior, and that he should not marry, or have connection with, a woman belonging to an older generation, even though he might be her senior in actual age. The law also may be based on a desire of the older males to maintain their authority within the household, and to prevent younger males from threatening their authority by marrying into a generation equal to their own. And perhaps it may have originated as a safeguard to the morality of the household during the absence, at work or at war, of the father, the head of the family.

In connection with the incest laws an exception may be mentioned, that of boy and girl twins, who may marry each other. Among the djabā the birth of such twins is considered a great wrong (manak salah), for the brother and sister are thought to have had a too intimate contact in the womb of their mother. The event will bring famine and disaster upon the entire village, unless averted by the temporary banishment of the children and their parents, followed by great offerings for purification and the propitiation of the evil spirits. When these rites are completed, and the twins have grown up, they may marry, for their incestuous connection is considered to be already atoned for. Since Balinese legendary history contains many examples of boy and girl twins who married and brought forth a tribe, or a line of kings, there is a certain feeling that boy and girl twins

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8 See the author's Rites and Customs Connected with the Birth of Twins in Bali (Tijdschrift voor Indische Taal-, Land- en Volkenkunde, Batavia, September, 1935 [in press]).
are intended to be man and wife, even though it is wrong for them to be so connected. At the present time it is rare for such twins to marry, although the banishment and propitiatory ceremonies are still widely practiced.

Curiously enough, it is not considered a wrong for very high-caste people to have such twins. But there are many divergences in the customs of the high-caste Balinese from those of the djabā. In passing it may be mentioned that men of the three higher castes may marry women who are djabā, whereas it is a crime for high-caste women to marry or to have relations with a man of lower caste than their own. Offenders used to be either drowned in the sea or banished to another island. Within the past five years the government was forced to deport a Brahmana woman and a djabā man, who, banished from their own village, were refused admittance to all other villages and towns.

The Balinese lontars, sacred texts written on leaves of the tal palm, describe in full the penalties and atonements for the unusual case in family matters: incestuous unions, birth of twins, seductions, suicides, etc. These are the things that are "not done." But for the things that are done, the customary, everyday occurrences, there are no written rules. That is why I have presented this study of a single family in its intimate relations, with the hope that it will give some picture of the family life running its ordinary course.

New York City
THE PASSING OF THE MIDEWIWIN IN THE LAKE WINNIPEG REGION  By A. IRVING HALLOWELL

This study is a post mortem record of a ceremony which once was of major importance in the native culture of the Saulteaux Indians of the Lake Winnipeg country. So far as I have been able to discover, through direct personal inquiry where possible and likewise by hearsay, the Midewiwin is no longer being held at any of the localities discussed below and which are indicated on the map. Undoubtedly a great deal of valuable information could still be obtained, however, by making inquiries on the spot, at places which I have never visited. Most of the information recorded below was obtained more or less incidentally to an ethnological study of the Berens River Saulteaux: the detailed account of the Midewiwin of the Indians of this river will be published elsewhere. But Chief William Berens, my interpreter and mentor during the course of my inquiries, proved to be such a spontaneous and reliable source of information in regard to all events and personalities during his lifetime, and even before, that I felt impelled to record as much as I was able, of what he told me. Owing to family connections which will be brought out later, to an active life of varied occupations, which brought him into contact with Indians from one end of Lake Winnipeg to the other, as well as with the inland peoples to the east, and owing above all to an exceedingly alert mind and a fine memory, I consider the facts obtained from him to be as reliable as any such information can humanly be. I have attempted to correlate the oral information obtained from him, as well as from a few other informants, with the data extant in documentary sources in order to place on record (a) the places where the Midewiwin was formerly held, (b) the approximate dates (whenever possible) when it was discontinued, (c) the names of the headmen and such anecdotes about them as seem pertinent to the subject, and (d) facts concerning the connection between different localities, especially where these concern personnel in leadership.

So far as our information goes, the Midewiwin or Grand Medicine

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1 The local term for Ojibwa speaking Indians which has gained a place in historical and ethnographical literature and, except for pedantic reasons, can scarcely be discontinued at this late date.

2 The numbers placed at the beginning of certain paragraphs later in the text correspond to those on the map.

3 I have only had direct contact with the Indians of Norway House, Island Lake, Grand Rapids (mouth of the Saskatchewan River), Poplar River, Wanipigow River, and, on the Berens River, from the mouth to Lake Pekangikum (Ontario).

4 He was born not later than 1865.
Lodge, as it is sometimes known, was confined to certain Algonkian and Siouan-speaking peoples: the central Algonkians in particular, Siouan tribes immediately in contact with these, and other Siouan tribes in contact with the latter. There are some indications that the ceremony was, in origin, an Algonkian rather than a Siouan institution⁸ and the suggestion has been advanced more than once that among Algonkian peoples the Ojibwa may have represented the hearth from which it was originally spread.⁹ If this be taken as an historical hypothesis, and to it be added the idea that it is not a ceremony of great antiquity, the provenience of the Midewiwin in the nineteenth century, when we first have real knowledge of it, is fairly intelligible. From both earlier and later records we know that the extreme northeastern Algonkians (Montagnais-Naskapi) did not have it, nor the Wabanaki peoples of northern New England nor the Algonkians of the middle Atlantic States. Where it has been reported for the Cree⁷ it is most likely a late acquisition, presumably from some Ojibwa group, but Skinner’s assertion⁸ that “the rite extended northward almost to the shores of


⁷ Ibid., p. 9. The author says it was carried nearly to the foothills of the Rockies by the Plains Cree and Ojibwa. Rev Henry Budd, e.g., who took up his residence at the Nepowewin mission on the north bank of the Saskatchewan River opposite Fort à la Corne in 1852, records in his Journal the following spring, that the Midewiwin is to be omitted. He adds that “it is the first spring for a long time that that ceremony is not to be kept up here.” It appears from a previous entry that he is speaking of the Thickwood Cree (cf. H. Y. Hind, Narrative of the Canadian Red River Exploring Expedition of 1857 and the Assiniboine and Saskatchewan Expedition of 1858 [2 vols., London, 1860] Vol. 1, pp. 399, 402-403). But as indicated below there is good evidence that Ojibwa speaking groups had penetrated the Saskatchewan country by the end of the eighteenth century. Even farther west, in fact, but still later in the nineteenth century than the previous citation, Father Petiot, in a letter dated December 30, 1873, from Lac la Biche (Saskatchewan) and addressed to his Superior (cf. Missions de la Congregation des Missionnaires Oblats de Marie Immaculée [Paris, 1874], Vol. 12, p. 462), says that between Lake Vert and Carlton he met many bands of Cree on their way to the Midewiwin which was being held on the shores of Pelican Lake. Whether the leaders of this ceremony were Cree or Saulteaux (Ojibwa) is not stated. Today there is a small band of the latter on Indian Reserve 159 north of North Battleford. I am indebted to Dr J. M. Cooper for calling my attention to this latter reference which also contains a description of the ceremony narrated to Petiot, it may be emphasized, by a half-breed Saulteaux (p. 464). I have not been able to consult the text but Dr Cooper provided me with an extensive resumé.

Hudson bay” is not borne out either by the intrinsic evidence offered in that author’s “Eastern Cree and Northern Saulteaux” or the later ethnographic work of J. M. Cooper in this region. Likewise, the absence of the Midewiwin among the plains Algonkian (Blackfoot, Arapaho, Cheyenne) would make it appear that the ceremony must have had its rise and spread after these peoples had become detached from the main stock. Unfortunately, the relative contemporaneity of all our records offers little if any opportunity for establishing chronological developments or even directions of spread in any detail. All that we can do is to infer for the most part that the Midewiwin must have been spread by the movements of the peoples with whom in recent times we know it to have been associated most intimately, or to have been borrowed, in certain cases, by peoples who came in contact with those who had it.

In the case of the Ojibwa speaking peoples it would appear that their northwestward expansion from the region of the Great Lakes carried the Midewiwin into regions where it had been previously unknown. Its maximum distribution subsequently, if we could plot it, would be a direct reflection of this migratory movement. It will be unnecessary, of course, to review here the evidence for this expansion of the Ojibwa. That they were intruders in the Red River valley and farther west is referred to again and again by various classes of writers. Begg, for example, with the former region in mind, says that the Cree considered the Saulteaux as having encroached upon their territory.

an instance of which was given when Lord Selkirk, in making his treaty with the Indians, committed the mistake of placing the Saulteaux first on the list. As will be remembered, the Creees were bitterly indignant at this and threatened not only to break the treaty, but also to demand back their lands, thus causing the Scotch settlers much anxiety, lest their farms should be taken from them by the savages.

Since the date of Lord Selkirk’s treaty was 1817 the movement from the east must have taken place much earlier. Both the diary of the younger Henry and Tanner’s account of his early life offer substantial testimony, at the very beginning of the nineteenth century, for the presence of Saulteaux in the Red River valley and in certain portions of the country west of it.

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9 Anthropological Papers, American Museum of Natural History, Vol. 9, Pt. 1, p. 60 et seq.
10 “A branch of the Chipewyans” (i.e., Chippewa).
12 To Henry we owe an explicit statement in regard to the triple synonomy for the Ojibwa of the old “Northwest” which allays any possible confusion. In 1808 he wrote, “the Ogeebois
But just how early in the preceding century this westward movement of the Ojibwa began it is hard to say, since this country was totally unknown to white men before the explorations of La Verendrye, which did not begin until the fourth decade of the eighteenth century, and documentary sources are almost nonexistent until much later in the same century. We do know positively, however, that Ojibwa speaking peoples had spread far beyond Lake Winnipeg by the end of the eighteenth century and that by 1794 some of them were to be found near the Pas and a considerable distance up the Saskatchewan River.

Today there are a number of Saulteaux bands in the province of Saskatchewan which are often called "Plains Ojibwa," although the districts they inhabit are part of the "bluff" country intermediate between the wooded areas and the true prairie. The Sun Dance has become their major ceremony, but it is not unlikely that the Midewiwin was formerly held by some, if not all, of these groups before they absorbed so much Plains culture. It is a problem which deserves inquiry and I have a single scrap of positive evidence which is perhaps worth recording here.

In the summer of 1931 when visiting the Fishing Lake Reserve (Indian Reserve 89, Saskatchewan) I was told that the Midewiwin had actually been performed there in the past. The last leader was mekwégi'zik, "amidst the sky," who died about 1919. He was the father-in-law of my informant. I also had an amusing personal experience on this Reserve which may be offered as evidence supporting the contention that the Midewiwin was probably known, although I made no systematic inquiries.

Earlier in the summer I had obtained a few medicine bags and some other articles at Wanipigow River on Lake Winnipeg. I had these in my luggage and one day, while sitting in a tipi, some mention was made of the Midewiwin and I casually remarked that I would bring one of the bags along with me the next day to show them. The words had no sooner left my lips than one of the older women present rose instantly to her feet and with considerable agitation pointed her finger at me and said, "Don't you bring that thing in here." And with that she left the tent. Later I did

are commonly called by the English Algonquins, by the Canadians [i.e., the voyageurs and others from Quebec] Salteurs and by the Hudson Bay Co. servants Bungees" (Elliott Coues, New Light on the Early History of the Greater Northwest [3 vols., New York, 1897], Vol. 1, p. 533).

13 He first reached the present site of the city of Winnipeg in 1738.
14 The Journal of Duncan McGillivray of the Northwest Company at Fort George on the Saskatchewan, 1794-5 (Introduction by A. S. Norton, 1929). See Introduction, p. xxxvi et seq., and pp. 13, 18, 20, all of which contain references to Saulteaux met by the author en route to his post. The location of Nipawin (p. 20) is approximately 53° 4' N. Lat., 104° W. Long.
15 By Ambroise Wolf of the Miscowequan Reserve (I. R. 85) near Lestock, who was visiting Fishing Lake at the time. His wife belonged to this band.
show a couple of the old men the bags, but at the farm house where I was staying. The old woman's behavior, however, seemed to me a sufficiently objective demonstration that the magical function of those articles was understood, even though the ceremony itself was extinct. I might as well have remarked that I planned to bring a loaded gun with a hair trigger into her tipi.

Returning now to the province of Manitoba and the Lake Winnipeg region, I will review the data I have been able to collect concerning the former occurrence of the Midewiwin at various points, starting with the district west of Lake Winnipeg, then moving south of it, next proceeding up the eastern shore, and finally ascending the Berens River to Lake Pekangikum, which is in Ontario.

1. In Manitoba, at some point west of the lake of the same name, there is casual mention by Tanner that, in the spring, the Indians with whom he
was staying at the time were "collected for the solemn ceremony of the meta or medicine dance, in which Net-no-kwa always bore a very conspicuous part."17 From Tanner's subsequent reference18 to descending the "Little Saskawjewun" as he started off for the Red River, as well as his previous peregrinations, it is at least plain that he had been hunting somewhere west of Lake Manitoba and probably south of the Riding Mountains. After returning from Red River he speaks of the "Naowawgunwudju, the hill of the buffaloe chase near the Saskawjewun" which can hardly refer to other than the Riding Mountains19 and he further states that he did not follow the Indians to "Clear Water Lake," which is probably the present day Clear Lake. Broadly speaking then, Tanner had been living somewhere in the upper reaches of what is now called the Minnedosa River,20 and it was here that the Indians must have held the Midewiwin referred to. The period was the early nineteenth century (1801–1806) as we know from the contemporary events and persons referred to by both Henry and Tanner.21 Furthermore, there was a small band of Riding Mountain Saulteaux which were included in Treaty No. 2 (1871). The name of their representative who signed it was "Mekis, the Eagle, or Giroux,"22 (mígazi—bald-headed eagle).

2. In respect to what must have been formerly the territory of the Waterhen23 and Crane River Saulteaux, who also were signers of the same treaty through their representative Broken Fingers or François,24 I received oral information25 that the Midewiwin was formerly held on Garden Island26 in

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17 Net-no-kwa was the Indian woman who had adopted him. Interesting accounts of the praying, singing, and prognostic dreams of this old woman (pp. 51–53, 66–67, 72) whenever food was scarce is undoubtedly a corollary of her reputation as a mi'dékwe.
18 Page 107.
19 Cf. Coues, op. cit., p. 305 n. The same identification is suggested in Place-Names of Manitoba (Geographic Board of Canada, Ottawa, 1933). The meaning of the native term appears to be "backbone mountain" (nawáwi-gonewadji'wan) which may be reconciled to Tanner's "hill of the buffaloe chase" if the contour referred to suggested a bison's back to the Indians.
20 The name of the river had not been changed in Coues' time (op. cit.). Cf. Place-Names of Manitoba.
23 The grebe, cingabis.
24 Ibid.
northern Lake Manitoba. The last head man there was àgásgogat, arrow
legs,27 who died before the treaty was signed. Years before, when a middle-
aged man, he had said, "As long as I can lift my drum stick I shall not die."
He revealed the fact that he had been told this by wísakedjak28 who was one
of his strongest guardian spirits.29 Like wísakedjak, Arrow Legs constantly
wore a woodchuck (aká'kotci's) skin bound around his head,30 undoubt-
edly a command of this spiritual helper. People testified that they had seen
Arrow Legs walk upon the water. On one occasion when the old man could
be plainly seen approaching the shore, his son sneaked down to the point
where he believed his father would reach the land. Arrow Legs, however,
observed the young man, and at once began to sink into the water, but he
managed to reach the shore. He chided his son for his impetuous curiosity.
All the other observers were said to have remained at a discreet distance.
In addition to being a great mide, Arrow Legs had obtained medical knowl-
dge through a dream blessing.31 He was máníáo.

3. South of Lake Manitoba, at the Long Plains Reserve (I. R. 6),32
Skinner gathered some meager information on the Midewiwin in 1913, at
which time it was apparently a going concern. He also obtained some of
the bark records used.33 In 1925 Cadzow visited the same reserve and also
Swan Lake to the southwest. He obtained more information and bark
records and stated that the Dominion Government had forbidden the
ceremony.34

4. South of the Forks35 or Grand Fork of early writers, the site of the
present city of Winnipeg, we know from Henry’s diary that the Midewiwin
was being regularly held by the Saulteaux of this region at the beginning of
the nineteenth century. In May, 1801, Henry was in process of establishing
a fur trading post near the junction of the Pembina and Red Rivers.36 On

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26 From Chief Berens of the Berens River band. When subsequently I speak of "my in-
formant" without further specification, the information may be attributed to the same source.
27 K’tigani-minis, Lat. 51° N., Long. 99° W.
28 From Cree àgös, arrow.
29 The equivalent of nánábójo, ná’nábúč.
30 Pasi’kwébl’on kaká’kotci’sweyan.
31 Usually from memí’sgweći’, a semi-human creature living in the rocks.
32 I. R. stands for Indian Reserve and the number in preceding and succeeding citations
is the official numerical designation used on government maps, etc.
33 Skinner, Medicine Ceremony, p. 309.
34 Donald A. Cadzow, Bark Records of the Bungi Midéwin Society (Indian Notes, Vol. 3,
No. 2, pp. 124-34, 1926). Mr Cadzow writes me that there seems to be no immediate prospect
of publishing the remainder of his notes.
35 The junction of the Red and Assiniboine Rivers.
one of his trips to the "Forks" he camped at the mouth of Roseau River,\textsuperscript{37} where on the 18th of May, he "found the Indians busy making the grand medicine—a ceremony performed every spring, when they meet and there is some novice to be admitted into the mysteries of this solemn affair."\textsuperscript{38} The following spring (May 20th) Henry reports that the Indians were "performing their grand medicine, as usual..."\textsuperscript{39} and a similar entry is of May 23, 1803.\textsuperscript{40} Since Henry is established by this time at Pembina one might suppose that the Midewiwin was being held there, but, on the other hand, he may simply be referring to the ceremony without detailed reference to locale since he was trading with the Indians within a considerable radius and since the point referred to above was about fifteen miles to the north. At any rate, the information provided by Henry demonstrates the annual periodicity of the ceremony and the season of the year at which it was customarily held, even though more intimate details are lacking. Of course, it cannot be denied that it may have been held near Pembina too, at one time or another, but it seems unlikely that there would have been two of these ceremonies performed annually at points relatively so close to one another.

At a later date there is a bit of evidence that the Midewiwin was being held some twenty miles up the Roseau River. Hind's party\textsuperscript{41} made an attempt to attain the upper reaches of this river, but gave it up, because they were improperly equipped. For some twenty miles upstream the river is said to wind a tortuous course through prairie country. Then at a point

\textsuperscript{36} Trading posts in this locality were built successively by the Northwest Company (Peter Grant House, 1793; Chaboillez House, 1797, and Fort Pembina, 1801 by Henry), the XY Company, 1801, and the Hudson's Bay Company, 1793, 1801, 1821 (Fort Daer). See Coues, \textit{op. cit.}, Vol. 1, p. 79 n., and E. Voorhis, Historic Forts and Trading Posts (Department of the Interior, Ottawa, 1930).

\textsuperscript{37} Called by him the Reed River, which joins the Red River from the east at about thirteen miles north of 49\textdegree. Coues (\textit{op. cit.}, Vol. 1, p. 69) provides an excellent historical note. The Indian term for the river given by W. H. Keating (Narrative of an Expedition to the Source of St. Peter's River, Lake Winnipeeek, Lake of the Woods, etc. [2 vols., London, 1825], Vol. 2, p. 76) is Pekwionusk, Reed-grass River, which corresponds with the contemporary Saulteaux term, pebi'gwéwÔnëskoś'bi', hollow grass river. Near its mouth this river flows through I. R. 2 occupied today by the remnants of the Pembina band who remained within Canadian territory after the boundary line was established. They were brought into treaty in 1871.

\textsuperscript{38} Coues, \textit{op. cit.}, Vol. 1, p. 182.

\textsuperscript{39} \textit{Ibid.}, p. 197.

\textsuperscript{40} \textit{Ibid.}, p. 212. In his entry of October 31st of this year Henry (p. 229) refers to an Indian of the name of Rats Liver camped at his post. Coues identifies this man with Tanner's "O-zhush-koo-koon (the muskrat's liver), a chief of the Metai" (p. 115 n.) who offered his granddaughter to Tanner as a wife.

\textsuperscript{41} Hind, \textit{op. cit.}, Vol. 1, pp. 157, 163.
called by Hind the "crossing place" and beyond for some distance, the banks rise fifty to fifty-five feet above the level of the river, and farther on the country changes to reedy flooded swamps. The point at which this change in topography occurs is identified as the rim of an old lake (Agassiz). Coming back they did not return to the mouth, but evidently cut across the prairie to the "Forks," diverging from the river at the "crossing place." Here it was that they observed the "skeletons" of dwellings, sweat lodges, and the "framework of a large medicine wigwam [which] measured 25 feet in length by 15 feet in breadth." I suspect that this must have been a structure built the previous spring for the Midewiwin. If it were, this identifies another point at which the ceremony was once held. A portion of the reserve belonging to the Roseau River Saulteaux of the present day is located at approximately this point. From still later sources we know that the Indians of the Roseau River Reserve remained outright pagans until the beginning of the present century and even in 1934 only 91 individuals out of a total of 206 in the band were reported Christians. Whether the Midewiwin has been held here within recent years I do not know.

5. On the lower Red River, after Lord Selkirk had established his colony (1811–12) the pure-blooded Indians, both Cree and Saulteaux, tended to congregate between St. Andrews Rapids and Lake Winnipeg, although even prior to this period there is evidence that this point was an old rendezvous of Indians and traders. It was in this district that the missionaries first began to labor when they actually went beyond the borders of the colony proper. It was chiefly in the neighborhood of Netley Creek.

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42 See Annual Report, Department of Indian Affairs, 1902, p. 89.
43 Ibid., 1934, p. 37.
44 That is between the "Forks" and Lake Winnipeg.
46 Henry, e.g., remarks, "The Creees and Assiniboines formerly assembled here in large camps to await the arrival of the traders." This, of course, was prior to the days of the Red River colony and it seems that Joseph Frobisher in 1774 had built a fort a short distance below the rapids (cf. McDonnell in L. R. Masson, Les Bourgeois de la Compagnie du Nord-Ouest [2 vols., Quebec, 1890], Vol. 1, p. 268), which Coues (op. cit.) thinks may have been the earliest on the Red River after the French establishments of the early eighteenth century. The last fort to be built in this district was the famous Lower Fort Garry in 1831.
47 See, e.g., Ross, The Red River Settlement (London, 1856), p. 278 et seq., on Archdeacon Cochran's unofficial attempts to Christianize the Cree and to teach them farming. The Saulteaux have always had the reputation of being more intractable.
48 Which joins Red River from the west at a point nine miles from the lake. It was formerly known as Dead River, oni·bōwisi·bi', a term still perpetuated among the modern Saulteaux, and which refers to the slaughter by the Sioux of some Cree, Saulteaux, and perhaps
that the Saulteaux of lower Red River periodically gathered in the spring and remained until autumn when they dispersed to their hunting grounds. It was here that the famous Pegwis made his headquarters, and in May, 1832 when the Rev Cochran paid a visit to the Indians encamped here he was shocked to find that they were performing a heathen ceremony. Even from the meager description evidently paraphrased from Cochran’s journal by Sarah Tucker, it can hardly have been other than a Midewiwin.

He found a large tent had been pitched, and was directed to the east end, where the chief was sitting, fanning himself with the skin of a muskrat. Pieces of riband and cloth were hanging all round the tent, the offerings of these poor people to the conjurer. . . . There were as many as 150 men, women and children crowded together in the tent, the top of which was open, and admitted the rays of a cloudless sun; and here the whole party was engaged in dancing, shouting, singing and drumming, shaking their rattles, and running round and round the tent.

That this ceremony was likewise in full swing three decades later is evidenced by the remarks of Hargrave, who did not come out to the colony until 1861 and wrote of contemporary events subsequent to this date. He refers to the “dog feasts” held each autumn (sic) near Lower Fort Garry in “an enclosure about 40 feet long by 25 feet broad, fenced in with the branches of trees.” This structure was oriented east and west and had two doors facing these directions.

The ceremony occupies two or three days, during which the ground in the interior of the enclosure is crowded with the savages, who sit along side each other drawn up close inside the fence. In a line running lengthways through the center are erected perpendicular poles with large stones at their bases, both stones and poles coloured red over different portions of their surfaces with the blood of the dog sacrifice. The animals are selected and killed, and after lying exposed on the stones beside the posts during the performance of certain ceremonies by the “medicine man,” whose “medicine bags” composed of the entire skins of wild animals, form an important feature of the ceremony, are cooked and eaten by the company. The dog meat when prepared presents a very uncouth and repulsive appearance, as it

Assiniboine families camped here in the late eighteenth century. See Coues, op. cit., Vol. 1, p. 21 n.; McDonnell in Masson, op. cit., Vol. 1, p. 268; Keating (op. cit., Vol. 2, p. 78) speaks of one Indian village at the mouth of Red River and the other at Death River (1823). At the junction of Red River and Netley Creek the Northwest Company established a post in 1803. See Voorhis, op. cit. The still earlier Fort aux Roseaux was near the mouth of Red River.

49 The Rainbow in the North; a Short Account of the First Settlement of Christians in Rupert’s Land by the Church Missionary Society (New York, 1852), p. 128. In the same volume verbatim quotations from Cochran’s journal are given, but I have been unable to discover whether it was ever published.

40 J. J. Hargrave, Red River (Montreal, 1871), p. 197.
is borne from man to man, in shapeless tin trenchers, that each may select the portion he means to devour.

Hargrave comments that it appears that the object of the affair is to eat dogs, but then he adds that a resident informed him that the natives were "assembled for what in their eyes is the celebration of a solemn act of communion with spirits."

Despite the inadequacy of Hargrave’s account and the probable inaccuracy of certain statements, the ceremony he refers to must be the Midewiwin.

In 1871 Red Eagle, miskógi·niːu,81 son of Pegwis (piˈɡwaiˈs) was one of the signers of Treaty No. 1 or the "Stone Fort"82 Treaty, and these Saulteaux (along with some Cree) of lower Red River became known as the St. Peters band83 and were domiciled upon a reserve seven miles square, located twenty-two miles north of Winnipeg, adjoining the town of Selkirk. Even as early as 1875 they were characterized in the report84 of the Deputy Superintendent of Indian affairs as "the best settled and most progressive of all the Bands which have been a party to Treaty No. 1." In 1907 they surrendered their lands on Red River and were transferred to a reserve on Fisher River which was named after their famous chief.85 No Midewiwin has ever been held on the latter reserve and the last one held at St. Peters was probably not held much after the '70's. Two brothers were probably the last leaders, āsiniˈbón and djakógvaiyo, short neck. The former was the father of sāgātcfˈweās who later became the chief of the "Island bands" and the leader of the Midewiwin given at Dog Head. This man is no longer alive but his sister’s daughter is the oldest woman of the Berens River band today.

6. In contrast with the "progressive" character of the St. Peters band, the Saulteaux formerly known as the "heathen" or Fort Garry band86 became domiciled after Treaty No. 1 on Broken Head River87 (I. R. 4), a

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81 His English name was Henry Prince.
82 Referring to Lower Fort Garry, where the treaty negotiations were conducted.
83 From the name of the parish which was included within the reserve. The parish was an organized settlement prior to the "transfer" of these western lands to Canada or the date of the Indian treaties. See Annual Report, Department of Indian Affairs, 1908, pp. 79–80.
84 See Report for 1875, p. 38. It is also stated that more than half the band were of mixed blood.
85 But in 1932 there was a schism and under the leadership of Grey Eyes some of these Indians returned to the St. Peters Reserve and, despite the efforts of the government, refuse to be ousted (1934).
86 Annual Report, Department of Indian Affairs, 1878, p. 51.
87 The Saulteaux term is paskandabéwiˈsiˈbiː; raw head (scalped?) river, from which the English form is no doubt derived.
tributary of Lake Winnipeg east of Red River. They remained pagans for many years. In respect to the performance of the Midewiwin here we are dependent upon oral information of relatively recent data secured by Skinner and the writer. The former\(^{28}\) records the fact that a Long Plains Saulteaux (ogimauwinini) interviewed in 1913 was “taught the secrets of the four degrees in order that he might establish an accepted lodge at Long Plains,”\(^{29}\) by an old mide at Broken Head, many years before. The name of this man was nénagi’s,\(^{30}\) quivering, who was also remembered as a mide by my informant at Berens River. Although I could obtain no information in regard to the probable date of the last ceremony given on Broken Head River,\(^{41}\) it can be stated that it was within the lifetime of my informant. Whether the Midewiwin of the Long Plains group did actually stem from this man, as Skinner’s statement implies, I do not know. But it may well have been the case and I suspect that if so, it also implies that nénagi’s may have been the best known mide south of Lake Winnipeg at the time, and that possibly the ceremonies on the St. Peters and Roseau River Reserves were already extinct. On the other hand, of course, there may have been other reasons which guided the Long Plains Indian in his selection of a capable tutor.

7. Following the eastern shore of Lake Winnipeg, we soon reach Traverse Bay and the mouth of the Winnipeg River.\(^{62}\) There is some dispute whether Fort Maurepas, the earliest post established in the Lake Winnipeg country by La Verendrye, was built here or on the Red River below Selkirk.\(^{63}\) The present Fort Alexander was built by the Hudson’s Bay Company in 1792 on the site of an old Northwest Company fort.\(^{64}\) In the journal\(^{65}\) of Peter Jacobs, a converted Ojibwa, who passed down the Winnipeg River in 1852, is found an exceedingly brief, but sufficiently explicit statement that establishes the fact that in the middle of June of this year a Midewiwin was being held at Manitou Rapids, a short distance east of Fort Alexander, and the first rapids encountered going up the river. That this was an established locale of the ceremony we infer from the author’s statement that this

\(^{28}\) Medicine Ceremony, pp. 316–17.
\(^{29}\) Another stronghold of paganism.
\(^{30}\) Written as nánigis by Skinner and translated as “someone shaking.” Descendants of the brother of this man are members of the Berens River band.
\(^{41}\) At present this band has almost completely disintegrated.
\(^{42}\) Saulteaux, sagíwi’sí-bí, the initial element sagí—simply means the mouth of a river, or where it opens into a lake.
\(^{44}\) See Voorhis, *op. cit.*
is the "place where they generally hold their manito feasts." About fifty years ago or more, it was given up, the last headman being ni jotés, little twin, also known as Two Hearts. Chief Berens recalls having seen him. The old man still retained the fashion of wearing his hair in two braids, a custom that at the time was not common among the other Indians. It may be remarked in passing that the acculturation of the Fort Alexander band proceeded very rapidly.

8. The next point to the north which was an old established locale of the Midewiwin was the east end of what today is called Black Island, the Saulteaux name for the island being kaplplkwewi'k, a term referring to a curious reverberation or thumping sound, said to occur when walking near Drumming Point, its eastern extremity. The latter place name, apparently, is derived either from the Saulteaux name for the island or it has reference, perhaps, to the custom of holding the Midewiwin there. The mysterious reverberations which are said to occur may have been a factor which led the Indians to choose this spot as one especially suited for the performance of the Midewiwin. Sekanakwégabau, the one who reaches the sky when he stands up, was the earliest leader about whom information was obtained. He must have been born very early in the nineteenth century, for one of his sons was about the same age as Jacob Berens who died in 1917 when he was almost ninety. The latter, who was occasionally a visitor in the camp of the old mide, often commented upon the pet bear which the latter kept tied up in his wigwam. The animal was perhaps three years of age. It was fed with pounded fish and berries, and its special feeding dish was a shallow wooden platter, stained or painted black. In those days the young men often used to play lacrosse in the early summer evenings. As soon as the bear saw them starting to play, he would begin to cry, and to strain at his tether. Then the old mide would say to the animal, "I'll let you go if you behave yourself. Now remember, don't hurt any of those young men." Then he would unloosen the bear which would run off and caper playfully about among those watching the game. As soon as the game was over, however, the bear would straightway return to the wigwam of the mide. It was believed by the Indians that the bear was controlled by the magic power which the mide possessed.

After the death of Sekanakwégabau, leadership of the Midewiwin was assumed by nődage, old woman (Cree). He was also mänáo. This man was

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68 Which is the literal meaning of twin.
67 Cf. Coues, op. cit., Vol. 2, p. 453 n., who says it was also sometimes called Grand Island. It has always been famous for the abundance and quality of the blueberries which grow there.
66 Father of the present chief, William Berens.
living at the time Treaty No. 5 was made, but refused to have any dealings with the government, even relinquishing his per capita share of the annual payments. His successor was Kagi-wébit, one who turns. Since the latter's English name was Black, the contemporary name of the island where the Midewiwin was held may have been derived from his connection with it. By this time the Saulteaux of the adjoining mainland were organized into the Hollow Water River (Wanapi-gáuwiisi'ibi') band which was given a reserve (I. R. 10) at the mouth of the river. Kagi-wébit was of the Moose sib and called my informant "younger brother." He died about 1919 and was said to have reached the age of ninety years. From this date until 1925, or perhaps later, the Midewiwin was conducted by Morning Star (Wabanaang), who died in 1932. So far as I know, he was the last leader of the Midewiwin to survive, not only among the Saulteaux whose territory borders directly on Lake Winnipeg, but inland to the east for a distance of three hundred miles.

9. Another contemporary place name, Dog Head, on the eastern side of the Narrows (wapáng), perpetuates a memory of an ancient locale of the Midewiwin on Lake Winnipeg. On Bloodvein River, nearby, is the reserve (I. R. 12) of a contemporary Saulteaux band. An Indian called sagatcí-wéás, one who comes up over the mountain, also known as Peter Stoney, was elected chief of the so-called "Island bands" as a result of the treaty negotiations (1876). These bands comprised: (1) a few scattered families on what was formerly called the White Earth or White Mud River, since re-named Icelandic; (2) the Indians on Big Island (k'tcí'minis), now Hecla, and

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69 His maternal grandfather was Isini-bon, a leader of the Midewiwin on Red River.
70 Locally known as "Hole" River. I do not understand the precise meaning of the name but it is reputed to refer to some peculiar characteristic of the local topography.
71 Which on the maps is spelled Wanipigow.
72 A photograph of his grave, made in the old-fashioned way, was obtained by the writer in 1931.
73 Chief Berens was making a trip south by sailboat in June, 1923, and put in at the Hollow Water Reserve. The Midewiwin was in progress at the time.
74 The contemporary Saulteaux maintain that this place name was applied by the whites. It can be traced back to at least the beginning of the nineteenth century, as it is mentioned by the younger Henry.
75 Miskóosi-bí', a name which is said by the Indians to have reference to personal decoration with red paint and not to the "red veins in the granite of its bed." See Place Names of Manitoba, p. 16.
76 Said to imply reference to the Thunder Bird (Pinésî').
77 Morris, p. 155.
78 The Sandy Bar band. See Morris, p. 129.
79 As Coues says (op. cit., Vol. 2, p. 451 n.) its name is warranted by the fact that it is the largest island in the southern portion of the lake. He believes it to be the Isle de Fer of La Verendrye.
on Wanipigow River, previously mentioned; (3) a group on Bloodvein River and at Dog Head; (4) another band on the west side of the lake at the mouth of Jackfish River (kinozéwi'si'mi'bi'). It is interesting to note that

the Indians gathered at Dog Head to receive their treaty money at this period, a fact which is probably connected with the annual performance of the Midewiwin there. Sagatcī-weēs was the headman of the Midewiwin as well as chief. He wore his hair in long braids and was never converted to Christianity. He was the son of ḥsini'bōn, one of the former leaders of the

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80 The term, jackfish, is the local name of the pike, *Esox lucius*. 
Midewiwin at St. Peters Reserve on Red River, and his English surname, Stoney, as still happens among these Indians, was derived from his father’s personal name. Sagatcf’weäs was assisted in the Midewiwin by two other leading men, wabosékwan, rabbit robe, and wagíbékwan, crooked back, both of whom died before him. Evidently sagatcf’weäs sometimes participated in the Midewiwin held at Jack Head, across the lake, because a Berens River man (François Felix) told me about a miracle (mämandawi-zí-) which he had seen sagatcf’weäs perform during a Midewiwin held there. He sharpened a stick into a point and then walked up to one of the other mide. The latter stuck out his tongue as far as he could. Sagatcf’weäs then spitted the man’s tongue with the stick and cut it off with his knife. He then circled the Midewiwin lodge, exhibiting the spitted tongue, after which he returned to the man whose tongue had been cut off, and replaced it. Another miracle witnessed by my informant was equally astonishing. A mide named makatcf’wewe, black (lesser) snow goose, took a gun in one hand and a cup of water in the other. He poured the water down the barrel of the gun (a muzzle loader), pulled the trigger and the gun went off.

10. At a previous period Yellow Legs, uzaúaskogat, the paternal great-grandfather of Chief W. Berens, had conducted the Midewiwin on the east side of the lake near the mouth of what is now called Pigeon River and at Jack Head on the western side. After the death of sagatcf’weäs the ceremony was no longer held at Dog Head, but the people of the Bloodvein band used to go across the lake to Jack Head each year where wawásan, lightning, had assumed leadership. This man had gone through the various degrees under Yellow Legs and was assisted by his three sons, one of whom, manzi’napkingéwiní’, the man who is painting the rock, may still be alive. But the Midewiwin itself has not been given at Jack Head for perhaps forty years. Chief Berens remembers Lightning well, and was at Jack Head once when the ceremony was in progress. The sons of Lightning assisted in the Midewiwin given at Hollow Water River after their father’s death and its irregular performance at Jack Head.

11. The mide previously mentioned, called Yellow Legs, although born on the western side of Lake Winnipeg, and who was the head man at Jack Head early in the nineteenth century, later took up his residence on Berens

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81 He was married to one of the granddaughters of Pegwis, a daughter of Henry Prince.
82 The native name for this river is pim’étígweasí-bí’, obliquely running river. The next river to the north, known as Berens River, is the pigeon river, omi’m’si-bí’, in Saulteaux parlance. The now extinct passenger pigeon is the bird referred to.
83 Who, like several of the other leaders, retained the old-fashioned style of hair dressing.
84 He probably died not later than 1830.
River, where his descendants are numerous and constitute the contemporary members of the Moose sib at the mouth of the river. From the standpoint of the history of the Midewiwin in this region Yellow Legs represents an important link between the Saulteaux on the eastern and western sides of Lake Winnipeg at this period. He appears to have been the most famous mide of his time whose reputation has survived. There is no one now living, of course, who ever saw him, but his widow was still alive when the oldest man of the Berens River band was a boy. This man, who is one of Yellow Legs' great-grandsons, told me that on one occasion Yellow Legs was seen walking on the water, on a calm day, over to Jack Head Island in order to secure medicine (mácki‘ki‘). He was brought back by memengwéci‘wak, semi-human creatures who live in the rocks and travel in canoes. All this happened in broad daylight while people were watching. He also brought back some gulls' eggs from the island, "in order to make the people believe in his power," my informant added.

On another occasion Yellow Legs dreamed of a large round stone on what is now called Egg Island, but which the Indians call wigwásiminis, birch island. He sent two men to fetch this stone for him: they were told to follow a bear's tracks to be found on the shore, which would lead them directly to it. But to make sure that they had found the right stone, a few branches would be broken directly above it. The men found the stone by following the directions given them by Yellow Legs, and it was brought to Berens River. It is now in the possession of Chief Berens. It was used in the Midewiwin for many years, and exhibited certain animate properties, externally represented by what appear to be a mouth and eyes. In the course of the Midewiwin Yellow Legs use to tap the stone with a knife, whereupon the mouth would open and he would extract a deerskin packet of medicine. The latter would be made into a concoction, which was then shared by all present.

Another miraculous demonstration by Yellow Legs took place under the following circumstances. One day when a group of young men were idling near his wigwam, a golden eagle (ki'ni'u) was sighted, far up in the sky. As it was observed sailing aloft, the young men again and again kept expressing the wish that they might obtain some of the bird's feathers for their arrows. Finally Yellow Legs got tired of hearing them repeat their desires. From his medicine bag he took a small iron spearhead, with a hole

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80 Jim MacDonald.
86 The implication is that these were among the old man's guardian spirits. They are noted for the knowledge of medicines which they transmit to those human beings whom they bless.
87 It is the larger of the pair of islands in the middle of the lake, east of Jack Head, of which Jack Head Island is the other member.
in the butt. Placing the spearhead in the palm of one hand, he slapped it with the other. No sooner had he done this than the eagle began to fall towards the earth, turning over and over as it dropped. It fell to the ground not far from where they were sitting. "Now, go and get your feathers," the old man said to the young men. With shouts of pleasure, they did so. Then Yellow Legs said to his wife, "You go now and open the heart." Inside of it was the spear point.

After the death of Yellow Legs, one of his sons, maskwa, bear (Cree), assisted by his brother, caúwanās, the one who travels with the south wind, became leaders of the Midewiwin. After the former died caúwanās became converted and joined the Protestant church. This was during the initial period of Christianization, when Rev E. R. Young was conducting the missionary work.88 The last Midewiwin must have taken place over fifty years ago. My informant pointed out the spot where the lodge had been erected.

Fortunately we have an extremely interesting though superficial account of the ceremony as given at the mouth of this river. Although not published until 1905, the observer, James Stewart,89 has unwittingly provided us with several clues which indicate the approximate period at which the ceremony described must have taken place. There is reference, for example, to the fact that a Mr Cummings acted as Stewart's interpreter. This man was in charge of the Hudson's Bay Company post when Peter Jacobs and Rev John Ryerson stopped there in the middle of the last century.90 Stewart also mentions "the Indian called Bear," who is said to have explained the general meaning of the ceremony to him. This man can be no other than the grandfather of Chief Berens, referred to above.

Berens River, it may be mentioned in passing, not only represents the farthest point north on Lake Winnipeg at which the Midewiwin was held, but it also marks the northern boundary of the ceremony to the east of the lake for at least three hundred miles. It was never given at Poplar River, at Island Lake,91 Deer Lake, or Sandy Lake, according to unanimous testimony of my informants, although Saulteaux speaking people are found today in all of these localities.92 In view of the western spread of the institution, even as far, apparently, as the Saskatchewan bluff country, its ab-

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88 Started in 1873.
90 Peter Jacobs stopped at Berens River in 1852 (see Journal, op. cit., pp. 42, 43). Cummings is referred to in 1854 by Rev John Ryerson, Hudson's Bay or a Missionary Tour in the Territory of the Hon. Hudson's Bay Company (Toronto, 1855), p. 81.
91 My own inquiries yielded only negative statements.
92 Needless to say, perhaps, it also failed to spread to the Cree of Norway House, Cross Lake, Oxford House, God's Lake.
sence among these Saulteaux speaking bands of the wooded country north of 52° N. Lat. presents a striking contrast.

12. Proceeding eastward up Berens River we find the Little Grand Rapids band domiciled a little over one hundred miles from the mouth. This was a less important center of the Midewiwin than farther up the river or at the mouth. From the information I have been able to secure, it appears that a lodge was only opened up for a few years under the leadership of Flatstone (nabagábek), assisted by Bluffhead (pi’kwákwasti’gan). The last ceremony was held about 1918. Flatstone’s son told me that the last few years the Midewiwin was given, his father did not actually participate. Bluffhead acted as head man. The midewigamik was built on a little rise of ground near the place where John Duck’s wabano pavilion now stands.

13. The chief reason for the subsidiary character of the Midewiwin at Little Grand Rapids was the dominance of tetabaiyábin, daylight all around the sky, one hundred miles farther east at Poplar Narrows. This man and his father (pazagwí’gabau) before him were indisputably the most influential and famous mide up the river. The son (otci’tcák, crane) of the former is the individual from whom the major portion of my information in respect to the ritual and significance of the ceremony has been obtained and although he has never conducted a ceremony in the capacity of leader, he is the only man on the river who, at present, would be capable of doing so. Sagáski’, hiding by bending down, was the associate of tetabaiyábin in the conduct of the ceremony. The lodge was erected on the north side of the river, on a level piece of ground near where I found some of the Poplar Narrows Indians34 camped late in the summer of 1932. The last Midewiwin was held here not later than 1922 and tetabaiyábin died a couple of years after this. He was failing rapidly and probably was not physically able to conduct another ceremony. Flatstone was trained in Midewiwin affairs by tetabaiyábin’s father and before opening a lodge at Little Grand Rapids, he was an active participant at Poplar Narrows, where other Grand Rapids Indians also went, either to see the ceremony or to go through it.

The relationship of all these men to each other is of interest. Everyone of them, including those at Little Grand Rapids, belonged to the Sturgeon sib. Bluffhead and Flatstone were parallel cousins and the former was a half-brother of tetabaiyábin. The father of pazagwí’gabau was likewise a head man of the Midewiwin. Although unformalized, it is obvious that

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33 Chief Berens, who was the representative of one of the opposition fur traders at Grand Rapids in 1919 or 1920, was told at the time that the Midewiwin had been held only a couple of years previously.

34 Descendants, in fact, of tetabaiyábin. I have in my possession the leader’s “bark” (k’tc’wilgwas) belonging to him as well as a number of “barks” obtained from the descendants of other deceased leaders, and a large number of other articles connected with the Midewiwin.
effective Midewiwin leadership was here confined to a single family line for three generations.

14. At a still earlier period it is said that pazagwi'gabau used to hold the Midewiwin farther up the river, where it enters Lake Pekangikum. His chief assistant was pindâ'ndakwan, stuffing something with brush, a son-in-law who conducted the ceremony at Pekangikum after the death of pazagwi'gabau. It was discontinued here about 1920 when pindâ'ndakwan went to live at Lac Seul.66 As late, therefore, as the second decade of the present century, the Midewiwin was being given at three places on Berens River. Before the next decade had passed, every one of the leaders had died or, as in the case just cited, moved away. No one seemed prepared to assume the leadership and since missionary effort, governmental supervision of the Indians, and other acculturative influences are becoming stronger each year, it is not likely it will be given again unless revived under the leadership of otcîtcâk. Pazagwi'gabau himself was in close touch with the head man at Lac Seul and used to make trips there occasionally to obtain new medicine. The man who was his acknowledged master was sagatî'weəs,67 but as we have previously pointed out, his father was also a well known leader.

This link with the Lac Seul band makes it clear that the Berens River Saulteaux, so far as the Midewiwin was concerned, were open to influences emanating from a point fairly close to the Ojibwa proper only a few generations ago. At the mouth of the river, on the other hand, there were connections across the lake mediated through Yellow Legs, and to the south there were also connections regarding which we have less specific information. These and other links, already mentioned, it is interesting to note, connect points which it would be impossible to infer were connected without specific reference to individuals, Long Plains and Brokenhead, St. Peter's Reserve and Bloodvein River Reserve, the latter with Berens and Jack Head, and Jack Head in turn with Hollow Water River and Berens River, to say nothing of Lake Pekangikum and Lac Seul. Furthermore, I was told that pazagwi'gabau sometimes came to the mouth of the Berens River and participated in the Midewiwin there, so that in this case, through the mediation of a single individual information could have been disseminated all the way from the Lac Seul Ojibwa to the Saulteaux on the western shore of Lake Winnipeg and perhaps beyond.

66 He belonged to the Pelican sib.
67 Who was chief of the Lac Seul band at the time Treaty No. 3 (Northwest Angle), 1873, was negotiated. See Morris, op. cit., p. 321. His name is spelled sak-katch-eway in this document.
INDIAN RUINS IN NORTHERN CHILE  By RICARDO E. LATCHAM

It has generally been supposed and widely stated that Chile possessed no architectural monuments dating from before the Spanish conquest. Such few stone constructions as had been mentioned by the chroniclers and historians were considered to be of no importance and, in any case, only to be referred to the time of the Inca occupation of the country, at the beginning of the sixteenth century.

All this is erroneous, however. In northern Chile there is a series of ruins of ancient cities, not only prehispanic, but also pre-Incaic. We employ the term cities advisedly, as the importance and extent of some of these groups of buildings fully justify its use.

Naturally all these ruins are not in the same state of preservation, nor are they all alike. In some respects they are similar to those found in the Southwest of the United States, but in others widely different. Like them they are almost entirely built of stone, but their general appearance varies with the locality and with the kind of stone employed.

During our last archaeological trip to the interior of the province of Antofagasta, we had an opportunity of visiting and studying several of these ruins which the present article briefly describes.

At about twenty-five miles east of Calama and more or less the same distance from the mines of Chuquicamata lies the townlet of Chiu-Chiu, on the banks of the river Loa, just before its junction with the Salado.

Half a mile to the north are the ruins of the old Indian town called the pucará or fortress by the neighboring inhabitants. It is evident that the ancient town occupied a considerable extent of ground, but today it is very much destroyed, not so much by time, as by the modern villagers, who for some reason, deciding to move the site of the town, pulled down most of the old buildings to utilize the material in the construction of new ones. Among the ruins left some walls remain partially standing and the foundations of many others are still visible. As far as we could judge the original town occupied a space of about 400 by 300 metres. The plan of the houses was rectangular: about six metres by four. They were built of stone cemented by mud mortar. As the soil is a combination of clay and sand, this, mixed with the saltish water of the Loa, becomes in a short time as hard as cement.

The rows of houses were separated by narrow streets or alleys, not more than five or six feet wide, on which opened the only doors of the buildings. It is difficult to say if these houses had windows, as none of the
walls were well enough preserved for us to judge, but taking into account similar buildings elsewhere, it is probable that they had.

At the southern end of the ruins, one of the walls of the old fortress was in better condition and in some places reached nine or ten feet in height. It seems to have been quite a considerable building, its outer walls enclosing a space of about sixty feet by thirty, and with an interior division. The standing wall is perforated by loop-holes twelve inches square, more or less, probably to enable the defenders to shoot their arrows.

This town was built on the plain and we discovered no signs of a circumvallating wall such as we found in other ruins. The quarry from which the stone had been obtained, was later used as a burial place, caves being excavated for the purpose.

Six miles farther up the river there are ruins of another ancient city in a far better state of preservation. It is called Lasana, and in its general aspects may be taken as typical of the last pre-Incaic stage of the Atacama culture.

Lasana is situated in a narrow valley, bordered on either side by high vertical cliffs which rise three hundred feet from the valley level to the plateau above. In the bed of the valley there is a knoll of about a hundred feet in height, which on one side drops perpendicularly to the river and on the other slopes gradually to the valley level. It is on this slope that the city was built in a series of irregular terraces.

Many of the buildings are almost intact, others partially fallen, but none of them are wholly destroyed, so that it is possible to obtain a good general impression of what the place looked like when occupied. All the houses were built of stone slabs, cemented by mud mortar, as in the pucará. The walls had an average height of about ten feet and an average thickness of fifteen inches. Most of the buildings were rectangular, but some were of irregular shape, as they follow the inequalities of the hill. Their general dimensions were from fifteen to twenty feet in length and from twelve to fifteen in width, but of course there is considerable variation.

All the houses had doors and windows, which in many of the buildings are still in perfect condition. It is, however, not easy to say how they were closed; probably by rush or cane mats, as there is no sign of woodwork.

These are undoubtedly the earliest windows hitherto recorded in American architecture, and are at least two centuries antecedent to those found in the Inca region of Cuzco and Macchu-picchu. In fact, the latter ruins seem to be derived from Atacameño influences, as it is now known that these desert Indians overran the Peruvian highlands before the Incas left the valley of Cuzco to conquer the surrounding country, that is to say, in the twelfth century.
The portals were well aligned; the stones forming the jambs being frequently squared to fit. The doorways were from six to seven feet in height, with an average width of about three feet. The cross pieces were formed of a single stone slab, well fitted to the jambs and giving the doorways a neat and even appearance. The window-openings were formed of thin stone slabs and were square, with a width of from eighteen to twenty inches. Nearly all the houses had one window; a few had two, opening on the narrow streets, or when they were on a higher level, overlooking the valley.

All the buildings had a granary or store-room, sometimes two; almost always inside the larger room, but occasionally outside adjoining one of the walls. They were built of stone, about four feet high, with no doors, but near the ground a square opening of the same size, shape, and construction as the windows. It is probable that these granaries were roofed, as we frequently found remains of sticks and rushes in the interiors.

In a good many of the houses, especially in the upper part, near the southern end of the city, we also found underground granaries, sometimes lined with stone slabs, and the entrance covered with a larger one. Many of these had been used as sepulchres and contained one, two, or more skeletons.

In the steeper part of the hill, the soft cliffs had been cut away in the form of terraces to make room for the houses, with the vertical cut at the back utilized as a wall. In this wall small caves, used as burial places, had been excavated. Lower down there was a series of natural caves that had been used for the same purpose. In both cases the front had been walled up. We found no regular cemetery in the neighborhood and it seemed to be the custom to bury the dead in the precincts of the town, generally in the houses themselves.

Communication between different parts of the city was effected by a number of narrow, crooked streets, many of which ended in small open spaces that served the purpose of squares. These narrow lanes rarely ran more than twenty or thirty feet in any given direction. Like the houses themselves their position depended on the lie of the ground and they were frequently interrupted by abrupt rises of level.

At the foot of the hill the city was surrounded on its southern, western, and northern sides by a massive wall, about four feet high, which served as a defense. A short time ago, this wall was torn down to utilize the material in the construction of a bridge over the Loa, in the immediate vicinity of the ruins. At present, only a few stretches remain standing, but these serve to give an idea of its importance. Houses fronting on this wall had a
series of loop-holes about twelve inches square instead of windows; these were probably used by the archers in case of an attack. Between the wall and the houses there was a roadway about ten feet wide which gave room for the defense of the barrier.

The cultivated land lay to the west of the city, between the outer wall and the high cliffs that shut in the valley. It is situated on a small level plain that runs little more than the length of the city and has a width of about five hundred feet. The system of irrigation is very interesting and remains intact. A canal, brought from higher up the river, follows the slope at the foot of the cliffs to the entrance of the little valley. On reaching the plain it crosses this by means of an ingenious aqueduct built of large blocks of stone, about five feet in length, four in height, and two and a half in thickness, set end to end and cemented. The upper surface of these blocks had been gouged out to form a concave channel of about eighteen inches in width by ten in depth. The aqueduct is connected with another channel cut in the rock which runs the entire length of the city, just outside the encircling wall. From this latter watercourse, feeders for irrigating the plantations run perpendicularly to its course, till they reach the foot of the cliffs. These feeders are distributed regularly every five metres, which is the width of the small plots parcelled out among the inhabitants. They are formed in the same way as the aqueduct, but the stones used are much smaller and rise only a few inches above the ground. At the foot of the cliffs there are remains of another small canal, used to drain off the surplus water.

Thirty miles to the northeast of Lasana, at Turi, are the ruins of another Indian city, built on three small hills which form part of the outer edge of a long slope of lava flow, which runs down from the Echado volcano.

The ruins of Turi belong to three distinct periods, ending with that of the Incas. In the lower part of the city, the walls, where left standing, are not more than four feet high, and, like most of the older Atacameño ruins, are divided into a number of small square or rectangular rooms, without either doors or windows. The walls must have been used as pathways and entrance made by the roof. These walls, as also all the others of the town excepting some Inca buildings of which we will speak later, are built of dark colored lava blocks of irregular shape, and no mortar was used in their construction.

In the upper part of the town the houses are of another type, that already described as prevailing in Lasana. Here walls are higher, frequently reaching ten feet, and have both doors and windows. Most of them have interior granaries, but in some cases the store-rooms are built outside. The
streets are narrow and crooked, and most of them run up the hill till they meet the Inca roadway, which runs due north and south, just on the eastern border of the city. This is the most level part of the town, and it is here that we find the series of buildings belonging to the time of the Inca occupation. These buildings are constructed of sun-dried bricks or adobes and have gable roofs. The largest of them, situated to one side of a small square, must have been quite a pretentious building for those times. It measures seventy-five feet in length, thirty-one feet in width, and the height of the gable points is about twenty feet. The side walls rise to a height of nine feet; the one fronting on the square having three doorways, equally spaced and about forty-five inches wide. Each of the gable ends has three windows; two just above the level of the side walls and the other higher up near the point of the gable. The upper windows have wooden sills.

The adobes of which this building is constructed are fourteen inches long, eight wide, and four thick. They are made of a slightly clayey soil, stiffened with a tough dry grass. The structure is still called “Casa del Inca” by the Indians of the neighborhood, although it is generally known as the “Church.” There are a few other adobe buildings in the immediate vicinity of the square, but such are not found in any other part of the town.

On its eastern side, the city is enclosed by a high stone wall, which, although in a ruinous condition, still rises in some parts to ten feet. Outside runs an Inca road, three metres wide, and quite cleared of stones which are piled on either side. It is marked at intervals by pyramids of stone, rectangular in form, with bases of five by seven feet and a height of about eight feet.

Farther up the slope there are several stone walls built to form three sides of a rectangle, with the fourth side open. Around these walls, on the outside, numerous skeletons were seated, covered with piles of stones.

In the lower and oldest part of the town the dead were buried in a sitting posture in shallow graves, which were also covered with piles of stones.

The city of Turi was larger than Lasana. From north to south it extends for about four hundred yards and it is a little more than half that distance from east to west. A rough calculation would give it some 450 houses and a probable population of nearly two thousand.

About sixty miles to the southeast of Chiu-Chiu, lies San Pedro de Atacama, once the most important town of the whole Atacameño region. Three or four miles from the present town are the ruins of an ancient fortified city, built on the steep slopes of a small hill that dominates the
entrance to the valley, where it narrows down to a gorge with perpendicular walls. The other three sides of the hill are inaccessible and fall precipitously to the valley beneath.

The foot of the slope, to a vertical height of about twenty-five feet, is also very steep and difficult to climb, except in certain narrow stretches where roadways have been cleared. At its upper edge this steep incline was defended by a massive stone wall, about four feet high and three in thickness, built of large boulders set on end. This wall is now in a ruinous condition and most of the boulders have fallen and are scattered over the lower slope, but in its time it must have been a good defense. Inside the wall there is a causeway about ten feet wide which separates it from the city itself. On the other side of this causeway there are three long buildings, separated one from the other and having a series of loop-holes overlooking the wall. They were probably barracks. They were long and narrow, with entrances at one end. The entrance was defended interiorly by a stone curtain, which impeded the entrance of arrows shot from outside and at the same time allowed entry to only one person at a time.

At the back of this first row of barracks, adjoining them, but at a slightly higher level, is a second row, of the same form and dimensions. Here again loop-holes overlook the wall, forming a second line of defense.

Behind these defenses the city runs up the hill to its summit. In general construction the city is built on the same lines as Lasana, with doors, windows, and interior granaries as in the latter. Here also there are many underground store-rooms, some of which were later used as burial places. Some differences may, however, be noted. For instance, here not all the houses are rectangular; some have rounded or oval ends, especially when they were built on outstanding cliffs which overlook the valley.

On the very summit of the hill there is a small platform on which is a rectangular enclosure with walls about four feet high. This, evidently used as a lookout station, overlooks the whole valley.

The streets are narrow and crooked, not more than five feet in width. One of these, which zigzags up the hill winding among the houses, allowed us to reach the summit on horseback.

In its lower part, the city has a frontage on the valley of about 170 yards, but diminishes as it recedes up the hill. The distance from the lower wall to the summit is about the same as the frontage. The city contains at least three hundred houses and should have held a population of not less than 1,200.

In the immediate neighborhood of these ruins, but in the valley, is a small hamlet called Quito, where the irrigated lands cultivated by the
present inhabitants are probably the same as those tilled by the ancient people of the ruined city.

In various parts of the region there are other ruined cities, not yet studied, as important and perhaps more so than those described. All this tends to show that the ancient Atacameño culture was much more developed and advanced than has been generally supposed. In a future article we hope to publish the principal archaeological finds from the cities already explored and from their vicinity.

Museo Nacional de Historia Natural
Santiago de Chile
Ruins at Lasana, northern Chile. Upper, General view. Lower Left, A granary. Lower Right, Doorways.
Ruins at Lasana, northern Chile. Left, general views. Upper Right, Gabled building and window in the form of a cross. Lower Right, Windows.
Ruins at Turi, northern Chile. Upper, Lower section of the ruins. Center, Inca building of adobes ("Casa del Inca"), interior. Lower, Exterior of the same.
Ruins at San Pedro de Atacama, northern Chile. **Upper**, Oval building. **Lower**, One of the best preserved segments.
TEKNONYMY AS A FORMATIVE FACTOR
IN THE CHINESE KINSHIP SYSTEM

By H. Y. FÉNG

The Chinese kinship system is primarily built upon the foundation of the old patronymic sib organization and the sharp differentiation of generations. All relatives, both lineal and collateral of the same patronym are classed into one "sib relation" group and all relatives by marriage, including women of the same patronym married out, are classed into the "outside relation" group. The generation principle cuts horizontally through these two groups of relatives and divides them into successive generation strata. These two factors—sib and generation—not only pervade the whole system, but also regulate marriage. A Chinese can marry any one outside his or her patronymic sib; if they are related they must be of the same generation irrespective of age. If the kinship system regulates marriage at all, it is only in the derivative sense.

Since generation is an important factor in the Chinese system, we should expect it to be consistently carried through. But there are some notable exceptions in contemporary usage, such as the fact that mother's brothers and wife's brothers are designated by the same term chiu, mother's sisters and wife's sisters by the same term yi, father's older brothers and husband's older brothers by the same term po, father's younger brothers and husband's younger brothers by the same term shu, father's sisters and husband's sisters by the same term ku (as hsiao ku). These peculiarities are of significance because the Chinese system is not inherently an incon-

1 The author desires to express his deep gratitude to Prof A. I. Hallowell for his many painstaking corrections and criticisms.


3 There is practically no literature on this important subject in English. For a very generalized conception, the reader may consult L. K. Tao, Some Chinese Characteristics in the Light of the Chinese Family (in Essays Presented to C. G. Seligman, 1934).

4 The Chinese characters are not given in this paper. Nearly all the terms used here can be found in the tables in Chen and Shryock's paper cited above, where the Chinese characters are given in full.
sistent one, but, as Morgan has remarked, it "embodies a well considered plan, which works out its results in a coherent and harmonious manner." It is still more significant, as we shall show later, that originally the generations of these relatives were clearly differentiated by distinct terms but in the course of time they gradually merged into each other. There must be at work some powerful disruptive force which threw the generation of these relatives into confusion.

There is one advantage in dealing with the Chinese kinship system: the terms are amenable to historical treatment. The changes of every term can be traced from period to period, and the causes of these changes can be, in most cases, ascertained. First we may take the connotations of the term chiu and the terms for the wife's brother during the various periods and arrange them in a table.\(^6\)

<table>
<thead>
<tr>
<th>Period</th>
<th>Connotations of chiu</th>
<th>Terms for wife's brother</th>
</tr>
</thead>
<tbody>
<tr>
<td>I. Before 3rd century B.C.</td>
<td>mother's brother husband's father wife's father</td>
<td>sheng</td>
</tr>
<tr>
<td>II. 2nd century B.C. to 9th century A.D.</td>
<td>mother's brother</td>
<td>chi hsiung ti</td>
</tr>
<tr>
<td>III. 10th century A.D. to present</td>
<td>mother's brother wife's brother</td>
<td>chiu</td>
</tr>
</tbody>
</table>

The different connotations of the term chiu in Period I are perfectly intelligible in view of the fact that cross-cousin marriage was undoubtedly in vogue at this time.\(^7\) In such a marriage the mother's brother and husband's father would be the same person; so also would be mother's brother and wife's father. In Period II cross-cousin marriage was dropped, so correspondingly the meaning of chiu became confined to mother's brother.\(^8\)

The terms for the wife's brother were different during each of the three periods. In Period I wife's brother was called sheng. Sheng also meant at this period father's sister's sons, mother's brother's sons, and sisters' hus-

\(^{6}\) L. H. Morgan, *op. cit.*, p. 421.

\(^{7}\) The chief reference is Liang Chang-chu's *Ch'êng-wei-lu* (Book of Addresses) in which the first eight books are about kinship terms. This work is a laborious and comprehensive collection of terms from all periods.

\(^{7}\) For cross-cousin marriage in ancient China, see Chen and Shryock, *op. cit.*, p. 630.

\(^{8}\) The new terms in the modern system for husband's father is kung and for wife's father yo fu.
bands (man speaking). This is also explicable by cross-cousin marriage of the bilateral type together perhaps with sister exchange. In Period II, because of the disappearance of this type of marriage, sheng was no longer applicable to any of these relatives and new terms were introduced to take its place. Chi hsiung ti was the term used for wife’s brothers.

In Period III the term chiu (mother’s brother) was extended to include wife’s brothers. The first use of chiu in this new meaning is to be found in the Hsin T’ang Shu. In the Biography of Chu Yen-shou, it says: “Yang Hsing-mi’s wife is the older sister of Chu Yen-shou. Hsing-mi (in ordering Chu Yen-shou to take up an important position) says: ‘I am so sick and my sons are too young. Having chiu take my place, I shall have no worry.’” This is certainly a curious extension of the use of chiu. Through all the vicissitudes of the term during the previous periods the generation element was always preserved. This blending of generations certainly warrants explanation.

In a strict sociological interpretation the conclusion would be a marriage with the wife’s brother’s daughter as an extension of the sororate, because in such a case the wife’s brother would be a potential father-in-law. We see in Period I chiu also meant father-in-law. Since the wife’s brother is a potential father-in-law, so the extension of the term chiu to him is perfectly logical. However there are several very serious difficulties to this interpretation. In the first place there is absolutely no evidence, either historical or contemporary, to support this hypothesis. In the second place it is contrary to the generation principle. Wife’s brother’s daughter will be

9 Chen and Shryock, op. cit., pp. 630, 657.
10 It is purely a descriptive term. Chi means wife, hsiung ti means brothers, older and younger. Sometimes fu hsiung ti and nei hsiung ti were used. Both fu and nei mean wife. The new term for father’s sister’s sons and mother’s brother’s sons is piao hsiung ti; tzu fu is the term for older sister’s husband, and mei fu for younger sister’s husband.
11 New Annals of the T’ang Dynasty (A.D. 618–905), Book 189, p. 10 (Tung-wén edition of the Twenty-four Histories). In this article only the authentic reference of the first occurrence of a new term or the new use of an older term is given. The numerous later references are omitted for the sake of brevity.
12 Indeed the Chinese system does not allow such complete departure from the generation principle, for in modern colloquial usage special modifiers are used to differentiate the generations: such as mother’s brothers are called chiu fu; fu indicates they belong to the “father” generation. Wife’s brothers are called chiu hsiung and chiu ti; hsiung and ti indicate they belong to the “brother” generation.
13 The question of the sororate during the feudal period was discussed in full by Marcel Granet in La polygynie sororale et la sororate dans la Chine féodale (1920). There are many exaggerations and twistings of evidence in this work; however, the discussion is very lively.
one generation lower than ego; so in the Chinese system she is within the incest group. The third is a temporal difficulty. Chiu ceased to mean wife’s father at least a thousand years before it was extended to mean wife’s brother. In the face of these objections this interpretation is not tenable.

It is significant that Chinese scholars had been employing teknonymy to explain this anomaly long before its introduction into ethnological discussion by E. B. Tylor.\(^{14}\) Ch’ien Ta-hsin (1727–1804), one of the most exacting classical scholars of his time, attributed this extension of the meaning of chiu to the gradual and imperceptible effect of the practice of teknonymy.\(^{15}\) Wife’s brothers are chiu to one’s own children. The father adopts the language of his children, so he also calls his wife’s brothers chiu. This can be clearly seen from the instance of Chu Yen-shou. Yang Hsing-mi called Chu Yen-shou chiu together with the mentioning of his own sons. It is inferable that after long teknonymous usage the term chiu established itself and displaced the older term.

Whether this hypothesis can be sustained or not depends upon the additional evidences which we can adduce for its support. At this point we may turn to the examination of the terms which the wife uses to address her husband’s father’s brothers and her husband’s brothers. Curiously, a similar mixing of generations occurs.

Po means father’s older brothers (both man and woman speaking)

\[\text{husband’s father’s older brothers (wife adopting husband’s term)\}^{16}\]

husband’s older brothers

Shu means father’s younger brothers (both man and woman speaking)

\[\text{husband’s father’s younger brothers (wife adopting husband’s term)}\]

husband’s younger brothers

So far as I am aware, there is no social or marital usage in China, nor is there any comparable usage that ethnographic data suggest, which could produce such a terminology. From the historical point of view the terms for these relations were different at different periods. In the \(E r h \ Y a\)^{17} the

\(^{14}\) On a Method of Investigating the Development of Institutions, etc. (Journal, [Royal] Anthropological Institute, Vol. 18, pp. 245–69, 1889).

\(^{15}\) \(H\)éng-yen-\(l\)u (Books of Ordinary Sayings), Chüan 3.

\(^{16}\) A man or woman calls his or her father’s older brothers po and father’s younger brothers shu. The category of the sex of the speaker is usually not distinguished by terms in most cases in the Chinese system. When a woman marries, she adopts her husband’s terms in addressing her father-in-law’s brothers, e.g., as po and shu. There is no special term used by the wife for her father-in-law’s brothers. See Chen and Shryock, \(o p. c i t.,\) p. 640.

\(^{17}\) The \(E r h \ Y a\) is the earliest Chinese dictionary; variously attributed to Chou Kung (B.C. 7–1105) and to the disciples of Confucius (B.C. 551–479). Probably it is not a work by
father's older brothers were called shih fu. From the second century B.C. to the third century A.D. po fu was generally used. From the fourth century A.D. and onward only po was sometimes used. Husband's older brothers were called hsiung kung in the Erh Ya. During the succeeding centuries hsiung chang was commonly employed. About the tenth century A.D. po was extended to mean husband's older brothers. As has been already stated, no possible explanations can be found in marriage forms for this blending of generations: the only possible alternative is teknonymy. Husband's older brothers will be po to the wife's own children. The mother adopts the terminology of her children, so she also calls them po. The term shu can be similarly explained.

Ku in the modern system means:
- father's sisters (both man and woman speaking)
- [husband's father's sisters (wife adopting husband's term)]
- husband's sisters

In Period I ku was used to mean father's sisters, husband's mother, and wife's mother (as wai ku) due to cross-cousin marriage. When cross-cousin marriage declined, ku was employed only for father's sisters. Husband's older sisters were called nü kung in the Erh Ya. Nü shu or shu mei were also used a little later for the younger sisters of the husband. During the fourth century A.D. the term ku began to be extended to husband's sisters. What was the cause of this extension cannot be exactly ascertained although the social history of the period concerned is fairly well known. It cannot be due to marriage with the wife's brother's daughter, in which case the husband's sister would be elevated to the position of the husband's father's sister: the objections to the interpretation of chiu by

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one hand but gradually augmented through many centuries. Its date cannot be much later than the fifth century B.C. The section on relationship terms has been translated by Chen and Shryock, op. cit., pp. 654–60.

19 Erh Ya, Chen and Shryock translation, p. 655. Both man and woman speaking, wife's term for them being the same.

21 Ibid., p. 659.

22 Ch'eng-wei-lu, Chüan 7, p. 6. Most of the chronologies in this paper are based on this work.

23 Chen and Shryock, op. cit., p. 630.

24 The modern term for husband's mother is p'o or p'o p'o, literally "old lady." The term for wife's mother is yo mu.


26 The first occurrence of hsiao ku (husband's younger sisters) is in the famous poem Kung chiao tung nan fei. The exact date of this poem is disputed but all scholars agree it cannot be later than the fourth century A.D. Ta ku is used for husband's older sisters. Ta means big, senior; hsiao means small, junior.
this usage also apply here. Furthermore, other features do not follow either terminologically or conceptually. Teknonymy remains the best explanation, because husband's sisters will be ku of the wife's children.

Correspondingly we find the same peculiarity of blending of generations of mother's sisters with wife's sisters. Both are called yi. Originally yi was used, as in the Erh Ya, for wife's sisters. Mother's sisters were called tsung mu. The first use of yi to mean mother's sisters is found in the Tso Chüan. In the twenty-third year (B.C. 550) of Duke Hsiang there is a passage: "Yi's daughter of Mu-chiang." By checking the marriages among the feudal lords of this time, it is clear that the term yi here does not mean wife's sister, as it ought, but mother's sister. As a matter of fact it ought to say "tsung mu's daughter of Mu-chiang" not "yi's daughter." This passage has perplexed the classical commentators for centuries and it still baffles the modern social anthropologist. Theoretically, a sororate together with a marriage with the father's widows would adequately explain it. In such a marriage, mother's sisters would be equated with wife's sisters. This explanation has certain plausibility, as a man's secondary wives are also called yi. That is mother's sisters, wife's sisters, and secondary wives (concubines) are all grouped into one class; a usage usually attributed to the sororate. It is well known that the sororate was practiced among the feudal lords, but as to the inheriting of father's widows there is no evidence. Indeed such a marriage would be abhorrent to the ancient Chinese. We learn from the old writers how they compared the Hsiung-nu, pastoral nomads of the northern steppes, to dogs as they married their father's widows.

The consensus of opinion among the classical commentators about the

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25 As among the Miwok where marriage with the wife's brother's daughter is reflected by twelve terms (E. W. Gifford, Miwok Moieties, University of California Publications in American Archaeology and Ethnology, Vol. 12, 1916, p. 186), but they are all lacking in the Chinese system.

26 Among the Omaha marriage with the wife's brother's daughter is reflected by the conceptual identification of the father's sister, the female ego, and the brother's daughter (A. Lesser, Kinship Origins in the Light of Some Distributions, American Anthropologist, Vol. 31, 1929, pp. 711-12) although not indicated by the terminology. In the Chinese system the generations of the father's sisters, husband's father's sister, and the husband's sisters are clearly distinguished conceptually although the terminology fails to differentiate them.

27 Erh Ya, op. cit., p. 657.

28 Ibid., p. 656.

29 James Legge (The Chinese Classics, etc., Vol. 5, Part 2, p. 503) translated this passage: "A daughter of the younger sister of Muh-Kêang (the mother of duke Ch'ing) ..." This is certainly a mistranslation. Legge not only did not check up the marriages among the feudal lords, but he did not even read the commentaries carefully.
discarding of tsung mu and the extension of yi to mean mother’s sister is the psychological similarity between these relatives. Mother's sisters are yi to one’s father just as wife’s sisters are yi to oneself. The son imitates the language of his father, so he calls his father's yi also yi. In short this case seems to demand a psychological explanation together with a reverse teknonymy.

The foregoing cases are the only instances in the Chinese system where the generation principle is openly violated. In every case we have tried to explain these exceptions by facts and hypotheses which have proved illuminating in the discussion of analogous phenomena elsewhere. But we found none of them applicable to the Chinese material. Instead, we found teknonymy the only satisfactory explanation. There is no doubt that teknonymy is the determining factor in all these cases, but we may ask, is teknonymy universal in China and of sufficient antiquity that it may have been involved in producing such effects in the kinship terminology? There is no question about the universality of the practice in China; only the frequency of its use might have varied from time to time and from place to place. It is usually of the type that omits the child’s name, just as in America a man may call his wife simply “mother.”

As to its antiquity, we have to depend upon historical evidence. Skipping the numerous comparatively late references, the earliest instance that can be interpreted as teknonymy is recorded in Kung-Yang’s Commentary of the Spring and Autumn Annals of Confucius. In the sixth year (B.C. 489) of Duke Ai is recorded the instance of Ch’én Ch’i. Ch’én Chi in referring to his wife says “Mother of Ch’ang...”. Ch’ang was known to be Ch’én Ch’i’s son. The teknonymous usage here is indubitably clear. The fifth century B.C. is more than a millenium earlier than most of the cases we have just discussed, except the case of yi (B.C. 550) which is more than half a century anterior. On the other hand, if we make allowance for the conservative spirit of the classical writers in recording colloquial language, it is reasonable to infer that teknonymy is much older than this documentary evidence shows.

In the very limited literature on teknonymy, various theories have been put forth to account for its origin, but no author attempts to use it to explain other social phenomena. Teknomy as a usage is based on

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30 A. L. Kroeber, Classificatory Systems of Relationship (Journal, Royal Anthropological Institute, Vol. 39, 1909, pp. 77–84). Kroeber’s views on linguo-psychological causation of kinship nomenclature have been much attacked by students. For an equitable comment see A. Lesser, op. cit., p. 711.

kinship and kinship nomenclature—a circumlocutory way of expressing embarrassing relationships. Through its long and intensive use, why should it not have produced certain peculiarities in kinship terminologies as other social usages are reputed to have done? The Chinese cases are especially illuminating. It would require a series of marital or other special practices to explain the peculiarities of chiu, po, shu, ku, and yi, whereas they can be uniformly explained by a single principle—teknonymy.

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THE KOLASKIN CULT: A PROPHET MOVEMENT OF
1870 IN NORTHEASTERN WASHINGTON

By VERNE F. RAY

A CULT of the general Prophet Dance type, but lacking any actual dance content, flourished among the Sanpoil, Spokane, and Southern Okanogan between 1870 and 1880 and exerted a profound influence, during its lifetime, on native religious concepts and social organization.

The organizer and leader of the cult was a young man named q'olá'skin. The movement has come to be known by his name, rendered variously in English as Kolaskin, Skolaskin, etc. He was born at snuké'ilt, a small native village on the Columbia River, in a long mat house of the old type, occupied by four or five families, mostly relatives of Kolaskin's father (sxwi'lakan) The latter had but one wife, qatsi'pitsa. Of the union were born three sons of whom Kolaskin was the youngest. Informants characterized the family as rather colorless, its members exhibiting no outstanding accomplishments nor yet any signs of physical or psychical abnormality. Kolaskin had gained a guardian spirit but not a powerful one. He participated normally in group activities such as fishing, hunting, and games. He was of an affable temperament and was well liked by his fellows.

This active and normal existence was terminated abruptly at about the age of twenty. Kolaskin was taken ill and confined to bed. All parts of his body swelled in size and sores covered his skin. His legs gradually became flexed and he was powerless to extend them. This condition lasted for about two years. During that time he stayed with a relative named

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The Sanpoil, to which group Kolaskin belonged, are a Salishan people who formerly occupied the territory on both sides of the Columbia River at the great bend in northeastern Washington. An ethnographical description will be found in the author's Sanpoil and Nespelem (University of Washington Publications in Anthropology, Vol. 5, 1932). A brief description of some of the informants furnishing the present material (William Burke, John Tom, Bob Covington) is contained in the preface to that work. Other informants included Julia Garry, an intelligent, elderly Lower Spokane woman who was in close contact with the movement, and kwip'lakan, a Sanpoil whose home was near Kolaskin's and who observed the movement throughout its duration.


3 The Southern Okanogan are known also as the Sinkaietk (snaq'aie'tk̕w̓).

4 The others were qwebmla'klak (eldest) and sqalslu'xpú's.

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qa·qe’ltsē, who lived in Spokane territory at Dentillion. Both of his parents had died previously. Household cures, such as poultices of herbs, were tried in an attempt to cure him but to no avail. Shamans were called. Prominent among these was a Sanpoil doctor of enviable reputation, named k’e·u’se·’ laxan, who treated Kolaskin at great length but was unable to restore him to health. Other lesser shamans likewise failed.

Some natives account for the onset of the illness in the following manner: Kolaskin was gambling at cards with a crippled old man. The old man lost a blanket to Kolaskin but refused to give it up. Kolaskin took it from him by force, then struck him with it several times. About a month later Kolaskin became ill, his symptoms being the same as those of the old man he had maltreated.

Terminating the two years of illness, Kolaskin suddenly lost consciousness. Prior to this time he had been mentally normal, though suffering violent physical pain. Informants disagree as to the time of this loss of consciousness, or “death,” as the natives call it. An old Sanpoil named kwap’lakan, who was present at the time, stated that it was midwinter. Julia Garry, a Spokane, recalled that it was late summer, perhaps August. She was not present but nearby. The family with whom Kolaskin stayed was living at the time at aʔasiʔkam, a camp on a small lake near Dentillion, which was a settlement occupied the year round. All agree that the time of day was late afternoon.

Upon discovery of Kolaskin’s apparently lifeless condition, his relatives called the occupants of the two or three other houses of the village and began to make preparations for burial. Kolaskin’s best clothing was made ready and the morrow was set for the actual interment. However about nightfall he regained consciousness and all present began to rejoice because their friend had “come to life” again. Kolaskin began to sing. The song he sang was a new one; no one had ever heard it before. He spoke to the assemblage, declaring that his pains were gone and that he was well once more. He told them that he had experienced a great revelation in a dream while he had been dead. He had been given a message by qwīlan-ts’u’tan (qwīlan, helper, creator, powerful one; ts’u’tan, possessive suffix)6 to deliver to the people. All of the Indians, he had been told, must change their ways; they must no longer drink, steal, or commit adultery. But it was most important that they pray to their new god. Upon arising in the morning, before going to bed, and before each meal a prayer must be ad-
dressed to qwâlantsutân. Moreover, every seventh day must be devoted to prayer and singing exclusively. On that day all work was to be prohibited and expressions of vanity were to be suppressed. No one was to paint his face, or so much as look at his image in the water. Rather, all were to gather together to pray and sing, and to listen to Kolaskin, prophet of their god. Prayers must precede berry picking or hunting expeditions in order that such ventures be successful. Indulgence in dancing and gambling was not to be permitted on Sunday. Friendliness and kindliness to others were to be considered virtues of the highest order.

Kolaskin’s audience was spellbound. One after another of those present began to pray, at Kolaskin’s request, and after his example. They agreed, almost as a whole, to give their allegiance to the newly revealed god and to follow Kolaskin as prophet and leader.

Actually, the recovery of Kolaskin from his prolonged illness was very gradual. It was not until the following spring that he began to hold regular meetings and sought actively to promulgate the doctrines of the new cult. Even at that time he still was unable to walk, but he had recovered largely from his other afflictions. He failed to gain many converts among the Spokane; most of those present at the time of his “return to life” were enthusiastic disciples, but the mass of the people were skeptical. Curiosity, however, brought large crowds to his meetings.

At this time some missionary work had been done among the Spokane by the Presbyterian branch of the Protestant church. The chief of the Lower Spokane (sinâkâ’lt), o’yâpaxân, had become a convert and had been appointed native representative of the Presbyterian church by the missionary who periodically visited the people. The chief did not actively resist Kolaskin in his efforts to organize the new cult, but his passive opposition and the influence of the missionary undoubtedly were factors in the failure of Kolaskin among the Spokane.

A short time later Kolaskin returned to the Sanpoil and made his home at Whitestone. Gradually he had regained the ability to walk, but his knees were permanently flexed. For the rest of his life he walked in a stooped position with a hand on each knee.

Once back among his own people, Kolaskin’s success was phenomenal. The story of his remarkable recovery and the essence of his new teachings had preceded him. When he arrived in person he was hailed as a great messiah. A large percentage of the total Sanpoil population became ad-

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6 Possibly on other days of the week as well. Informants disagreed on this point but I am inclined to feel that the restriction applied to the one day only.
herents of the new faith. Kolaskin was raised to the rank of chief at Whitestone.7

Kolaskin had not been married previous to his recovery, but at that time he had taken two wives, enthusiastically offered to him by parents who had been impressed by the miracle of his "return to life." One of the women was a Spokane; the other was a Sanpoil named si·pi'tsa. Besides these two wives he married others after returning to Whitestone. Informants declare that he had a total of at least five wives. One informant stated that "all of the women were crazy about Kolaskin and it went to his head."

Meetings were held more or less regularly, usually once or twice each Sunday. Kolaskin taught his followers prayers to repeat and songs to sing. Upon entering the meeting place all kneeled and one began a prayer, whereupon the others joined in. Little specific information as to the content of the prayers is available at this date, but it seems that they followed closely the pattern of the traditional appeals and supplications for help addressed to Sweat Lodge.8 The songs were newly composed for use in connection with the services. The implication is that they were inspired during periods of emotional stress at the meetings as were the guardian spirit songs at the winter dances.9 The cult songs are said to have resembled the native love songs in lyric character more than any other preexisting type of song.10 In addition to the singing and praying in which all present took part, Kolaskin spoke at each meeting. Here again, adequate information is not available to establish the exact nature of his addresses. He exhorted his audience to lead exemplary lives, to refrain from work on Sunday, to attend the meetings and to pray to wəlantsutən. He repeated often the story of his illness and miraculous recovery, and his dream and the revela-

7 Informants say that Kolaskin was given the chieftainship by action of the village assembly, but are uncertain whether the existing chief abdicated or was deposed, or continued to serve as co-chief. The last alternative seems most reasonable, although in such a case the previous officeholder undoubtedly retained only nominal power due to the great influence that Kolaskin possessed with the people.

8 Sweat Lodge was the tribal deity. See Ray, Sanpoil and Nespelem, pp. 177, 179.


10 This may have been due to the oft repeated references by Protestant missionaries to the "love of God" and such exhortations as "love one's fellow man." A priori reasoning as to the nature of the cult songs undoubtedly would have led one astray here, for the conclusion that these songs should resemble the sweat house songs would logically follow since Sweat Lodge was the nearest approach to an aboriginal deity and the most highly formulated songs were addressed to him.

The most strenuous objections of the Protestant missionaries to Kolaskin's cult were directed at the "sensuous" nature of the songs.
tion. As time passed he came to speak of his recovery as having occurred instantaneously and completely at the time of his vision, although his older followers knew that this was not true.

Dancing was never a part of the procedure at the meetings. Bells were not used, as in the Pompom and Shaker Indian cults. The audience sat quietly on the floor for the duration of the service; violent gesticulations such as those indulged in by the Shakers were not a characteristic.

In time Kolaskin felt that a separate building was needed to serve as a meeting place. Accordingly, he enlisted the help of a number of his followers, and logs were cut and assembled in sufficient quantity to provide a large building. The preparation of the logs and construction of the building required a long time but eventually it was finished. The building was erected at Whitestone and thereafter all meetings were held in it. In celebration of its completion, meetings were held practically every evening for several weeks.

During this time Kolaskin experienced a second revelation and announced to his people that at the end of ten years' time the world would be enveloped in a great flood. To avoid destruction, he continued, they were to build a sawmill near the church and saw the lumber for a great boat. Before the end of ten years the boat would be completed and all followers would gather inside at the appointed time. Also, a male and female of every animal and bird would be included. Then the rain would come and flood the earth but all those in the boat would be saved.

So strong was the control of Kolaskin over his followers that they actually contributed the funds and labor to construct the sawmill and prepare the lumber for the boat. Over a period of years this was done, but though the lumber was cut, the boat was never assembled, because of the sudden curtailment of Kolaskin's activities by agents of the federal government, as explained below.\textsuperscript{11}

In the meantime Kolaskin had again played the rôle of prophet. Early in the winter of 1873 he suddenly announced that he was departing for Kartaro, a native village in the Southern Okanogan territory. He took four of his followers with him. They were unable to reach Kartaro the same day. In camp that evening he divulged the secret of his mission to his companions.

"Something disastrous is going to happen to the world. I don't know what it will be. Perhaps we shall reach home again before it happens, but it may come before we reach Kartaro."

\textsuperscript{11} Remnants of the lumber and the old mill are still to be seen at Whitestone.
The party arrived at Kartaro on the following day. Kolaskin immediately led them to the home of swipki'n, a Southern Okanogan prophet. Kolaskin addressed the prophet: "We have come to learn what you may know of the catastrophe which is to befall the world. What terrible thing is it that is coming? When will it come?" But the Okanogan man had had no vision or premonition. Then Kolaskin continued, "Well, I can tell you what will happen. The land is going to shake. Buildings will fall down. People will go out of their heads. You had better tell your people. Warn them as to what is going to happen."

Kolaskin and his party departed for home. All along the river they warned the people of the impending tragedy. They were laughed at by some, but many more took the prophecy seriously. Those who had become followers of Kolaskin began to pray to qwí:lantsu' tan to deliver them from destruction.

At the end of the first day of the return journey the party had reached the mouth of the Nespelem River and made camp there. A slight earth tremor was felt that night. They arrived at Whitestone on the following day and Kolaskin gathered his followers together in the church to pray. Severe quakes occurred that night and throughout the following day. Further tremors were felt at intervals from that time until spring.12

Julia Garry, a Spokane informant, told the following incident concerning one of the lesser quakes: "I was camping at Whitestone, not far from Kolaskin's camp, a little while after the big earthquake. Suddenly one day Kolaskin rushed out of his lodge and called to the people to begin praying and to look out for what was to happen. A little while later another earthquake came, just a small one. More people believed in him after that."

Not only was Kolaskin's influence at home greatly enhanced, with the result that many who had been skeptical now wholeheartedly joined his band, but swipki'n and all of his followers among the Southern Okanogan became supporters of the Sanpoil prophet as well.

The fight waged by the Protestant missionaries against Kolaskin was decidedly a losing one.13 Not only were they helpless to curtail the rapid spread of his influence, but their own converts were joining the native cult.

No pressure was brought to bear by Kolaskin in gaining adherents to his cult, but as chief of Whitestone he required all of the villagers, whether

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12 The exact date of the major quake, which dates the movement as well, was November 22, 1873. The date is positive, since there is record of but one major earthquake in this region. The quake was classed by Edward S. Holden (A Catalogue of Earthquakes on the Pacific Coast, 1769 to 1897, Smithsonian Miscellaneous Collections, Vol. 80, 1928, 7, No. 2955) as "extremely severe" and was rated VIII on the Rossi-Forèl scale.

13 The Catholic missionaries do not seem to have opposed Kolaskin.
they were devotees of his cult or not, to regulate their conduct by the same standards. These standards were the ones set by the cult, not by Sanpoil tradition. In pursuance of this object, Kolaskin had a jail built, or rather, dug. A pit was excavated and covered with boards sawed at the mill. By this time Kolaskin was demanding that labor required for operating the mill, and accomplishing such ventures as the construction of the jail, be furnished equally by all men of the community. Indeed, women as well as men were forced to contribute their time working at the same tasks. A certain amount of ill feeling was aroused by such procedure, but it was increased when Kolaskin began to imprison persons for minor offenses and subject them to a starvation diet. Adultery, philandering, stealing, and intoxication were some of the charges on which he confined persons to his jail. He appointed men from among his followers to act as his agents in apprehending persons accused of crime, and in carrying out his orders in general. No hearing was given an accused person; the word of Kolaskin was final.

Officials of the Indian agency, located at Fort Spokane, were at a loss to know what action to take to curtail the arbitrary authority that Kolaskin had taken unto himself. The agent wrote to the Department of Indian Affairs at Washington, D.C., asking for advice, after several complaints had been made to him by natives. In the meantime an incident occurred which precipitated the situation.

Two of Kolaskin’s prisoners had managed to break out and escape. One of the men, sqwhelu’mqon, returned to his home at snuke’ilt, while the other, q’otálaka’iáš, fled southward. Kolaskin discovered that they had escaped and started out to assemble his agents for a search. One agent, sqpxapqé’n, joined him at Whitestone and accompanied him to snuke’ilt where they expected to enlist the services of another agent, sanúmti’tsa. After locating the latter, the three rode to the camp of the escaped prisoner, who came out and faced the posse. Kolaskin ordered one of his deputies to tie up the fugitive so that he might be taken back to jail. An old man, usałxa·qu’sam, uncle of the prisoner, heard the commotion and came out of the house. Upon seeing that his nephew was again in the custody of Kolaskin, the old man railed bitterly at the prophet: “You are an evildoer; you cause only harm! You are always making trouble for your friends. You preached and made all of the people believe in you. Now you put them in jail for no reason at all!”

Kolaskin’s only reply to the accusation was to order his agent to bind the old man so that he might be taken along also. All returned to White-
stone.
The following day was Sunday. Many people were congregated at Whitestone and others were camped a short distance up the river. In the latter group was a cousin of the fugitive named kənəm'swi'ktsa. He was a strong man of large stature. Some one informed him that his cousin had been returned to prison and that his uncle had been placed there also. Immediately he took his gun and rode off in search of sənəmti'tsa, the deputy who had been most active in executing Kolaskin's orders. He was joined by a friend, qwily'sulklak. They found the deputy cutting hay in a field above snuks'ilkt. The nephew accosted him and criticized him harshly for his part in imprisoning the old man. The accused agent then took the offensive and said that they might as well settle their grievances then and there. At this, the nephew struck the deputy across the face with his quirt and the latter retaliated with a swing of his scythe, but only hit the horse. The nephew then departed for Whitestone to see his imprisond uncle. But the deputy hurried to his lodge, got his gun and his horse, and followed in pursuit. He overtook the nephew a short distance from the jail. Both dismounted at once, but the agent shot first, killing the nephew.

He hastily buried the body, then remounted and rode to Whitestone. There he sought out Kolaskin and told him to procure a rope, explaining, "I have killed my friend." But one of the men present, kəmalakə'n, took the murderer's gun from him and sent him away. Kolaskin did nothing.

Two friends of the dead man, squul'm and sənəmxw'ilkim, disinterred the body and took it to Whitestone to be buried properly. Then they broke down the jail door and released the old man and his nephew, telling them of the fate of their relative who had championed them. Those present decided to burn the jail. A woman, sən'seit, overheard the remark and told Kolaskin and others, but no one interfered. However, the jail was not burned. No one present had any matches, so they satisfied themselves by wrecking the cover of the pit.

As a result of this episode both Kolaskin and the actual murderer were taken to Fort Spokane and held in custody. The murderer was released after a hearing, but Kolaskin was sent to the federal penitentiary on McNeil Island for a period of three years.14

During his absence many of Kolaskin's followers continued to live according to his teachings, and meetings still were held regularly in the church. After serving his term, Kolaskin returned to his people and at-

14 In an attempt to learn something of Kolaskin's record during his imprisonment I wrote to the Warden of the McNeil Island Penitentiary and to the Department of Justice at Washington, D. C., but to no avail. The records of the early years of the prison, I was informed, have been moved so often that it is now virtually futile to make a search.
tempted to disband the organization that he had built up. He declared that all that he had taught them had been false, and the whole scheme had been a hoax to gain power. But those who had remained faithful during his absence would not listen to him. The cult continued to live, and the last adherent, saγi'mpt, only recently (1930) ceased to strive to keep the cult from dying.

Kolaskin retained his chieftainship after his return from prison. He was distinctly unfriendly toward the whites and held meetings among his people to warn them that the white man would take all of their lands away unless they were wary. He advised them to accept nothing from the white people or the government agents. He succeeded upon two occasions in raising sufficient funds from the people to go to the capitol at Washington, D.C., to represent the Sanpoil and protect their interests. He later claimed to have been responsible for the first payment of monies made to each Indian by the federal government.

In this connection his attitude toward the wars with the whites is illuminating. During the Spokane war he was responsible for his people remaining peaceful at a time when he might well have joined his friend, o'γpaxon, chief of the Lower Spokane. Nor did he join the Moses-Columbia people in their war on the whites, but rather encouraged his followers in no case to resort to arms in opposing the newcomers. In maintaining this attitude, of course, he was merely following the strong Sanpoil tradition of pacifism. Thus, though inaccordant, Kolaskin was not actively antagonistic to the whites.

In marked contrast to his career prior to his incarceration, Kolaskin practiced as a shaman in the traditional manner subsequent to his return. At this, as in his former venture, he was successful and sought after. During the later years of his life he was less active, however, and about 1920 he died alone in his house at the village of his birth. About a month later a great celebration was held by the Sanpoil in honor of his memory.

Kolaskin was described by informant Burke as a powerful orator and a clear and quick thinker. He was jovial and pleasant, and always a talented raconteur. He was of sober temperament, never drinking or smoking. Without question he was the most powerful figure the Sanpoil had known for generations.
AUSTRALIAN THROWING-STICKS, THROWING-CLUBS, AND BOOMERANGS

By D. S. DAVIDSON

THE weapons of the Australian aborigines, as is well known, are characterized by simplicity, both in form and in the manner of their propulsion. Aside from the use of the spearchower, which is wide-spread but not continental in distribution, all weapons are either thrown by hand or held in the hand for striking. The bow and arrow, the sling, and the throwing-cord, present in nearby Melanesia, are lacking.2 Aside from the most important weapons, spears, the uses and varieties of which have been considered elsewhere (II), there are a number of other weapons such as throwing-sticks, throwing-clubs, and boomerangs, and their larger counterparts, the heavy striking clubs. In this paper these weapons of secondary import will be considered.

THROWING-STICKS

The most simple form of throwing-stick is shown in Figure 1a. This specimen is from Tasmania where these weapons were typical, but there are others like it from eastern Australia. It is no more than a slightly curved, pointed, peeled stick, two to two and a half feet long and a little more than an inch in diameter. In most cases, it is of the same diameter throughout its length but occasionally it tapers slightly. Generally there are a few scratches to ensure a grip. Many Australian throwing-sticks, principally those in Western Australia, differ only in that they have fine longitudinal flutings (fig. 1b). Throwing-sticks are present throughout most of Western Australia and the western portions of South Australia and Central Australia (fig. 2). In eastern South Australia, Victoria,3 and New South Wales sporadic appearances also occur.

Throwing-sticks with stump ends. A modified variety of throwing-stick which is somewhat shorter and characterized by stump ends is also found

1 This paper represents one of the results of a study conducted in the Australian museums with the aid of a fellowship grant by the Social Science Research Council of New York. The museums visited and the abbreviations for them used in the references include the Australian Museum, Sydney (AM), the National Museum of Victoria, Melbourne (NMV), the South Australian Museum, Adelaide (SAM), the Western Australian Museum, Perth (WAM), the Queensland Museum, Brisbane (QM), the Tasmanian Museum (TM), and the Museum of the University of Pennsylvania (UP).

2 The bow and arrow have reached Cape York but the extent of their use is not known. See 4I, Vol. 4, p. 173. [Numerals in italics refer to the works cited at the end of this article.]

3 In addition to a few appearances of the typical throwing-stick, a similar weapon with more tapering points, almost a throwing-club, was fairly common in Victoria (48, Vol. 1, p. 302).
in the western Kimberley region. It may be related historically to the blunt throwing-stick used at Melville Island (fig. 1c), but there is no evidence at present to confirm such a theory.

Kandri. In the Coopers Creek district of South Australia, and extending into the Diamantina region of Queensland, a large and heavy curved weapon, known as a kandri, is used. In shape it is similar to the ordinary

Fig. 1. Throwing-sticks and adzes. a, Tasmania; b, Western Australia; c, Melville Island; d, Western Australia (typical); e, Wardaman tribe, North Australia; f, g, eastern Queensland (after Roth).

throwing-stick, although the curve is more pronounced. Since the length is often as much as three feet six inches, this weapon may serve in part as a club. It is generally fluted.

We thus find that both main areas for throwing-sticks, Western Australia and Tasmania, are peripheral to, and are separated by several sporadic appearances in, a region where throwing-clubs were in general use in historic times. On the basis of such a distribution, it is suggested
that all the appearances of throwing-sticks are historically related and that their use in the southeastern parts of Australia has been supplanted by the employment of other weapons.

We have no evidence to show whether the throwing-stick was developed in Australia, presumably by the Tasmanians, or brought in by migration or diffusion from the outside. Throwing-sticks are so simple in form that they may have been developed independently in Australia. On the other hand, their simplicity suggests that they may have been in use for great periods of time and may have been a possession of the early invaders of the continent.

**GUM HANDLES AND THROWING-STICK ADZES**

The developments associated with throwing-sticks in Australia, subsequent to the time Tasmanian culture presumably became isolated, seem to have been not numerous, although interesting and important. We have already mentioned the presence of fine flutings on a large number of the Australian specimens, particularly those from Western Australia. Of greater interest are the application of a gum handle and the development of the combined adze-throwing-stick with single and double blades.

The distribution of the gum handle (fig. 1d) is almost as widespread on the continent as that of throwing-sticks. It is a common feature on some specimens in most regions in Western Australia, and along the southern coast, west of Port Lincoln. Gum handles seem to be lacking, however, on both throwing-sticks and throwing-clubs in most of eastern Australia.

*Throwing-stick-adzes.* In most of Western Australia, apparently throughout Central Australia, southern North Australia, and along the western coast of South Australia throwing-sticks are equipped with a stone adze blade inserted in the gum handle (figs. 1d, 2). We do not know whether the Australian adze developed originally in association with the throwing-stick or with the spearthrower, for a large proportion of the latter in the same regions are similarly equipped. Whatever may have been the history of development, the combination throwing-stick-adze and spearthrower-adze are now important and useful weapon-tools in a large part of Australia and can be regarded as good examples of the economies practiced by the Australians who in several instances have combined the principles of two, three, or four tools or weapons in one object.

From the technological point of view the use of the plain gum handle presumably preceded the development of the adze. However we do not know whether the principle of hafting stones in gum handles is indigenous to the continent or derived from New Guinea. Wherever the point of origin
may be, the Australians have discovered several kinds of resinous gums for this purpose.

*Proper adzes.* A specialized type of adze (figs. 1e, 2) is found in a more restricted distribution than the adze-throwing-stick, from which, it seems likely, it has been developed. The main point of difference is in the handle, which is shorter, often somewhat stouter, and usually more curved than the throwing-stick. This proper adze, if we may call it such, for obviously it was designed as such a tool, also serves as a throwing-stick for striking or throwing when occasion demands.

*The double adze.* The double adze is more complex than either of the above types in that it has a blade attached at each extremity (figs. 1g, 2). In many specimens, the two stones are of different forms; one being a point for incising or perforating, the other a broad scraper for cutting, scraping, chopping, or for other like uses. Double adzes appear with both throwing-sticks and the shorter, more curved adze handles.

**THROWING-CLUBS**

Throwing-clubs are functionally closely related to throwing-sticks but differ in form in that they have knobs, bulges, or other protuberances carved in the solid.\(^4\) They appear to be lacking in Tasmania,\(^5\) and in most of the western and central regions of Australia (see map, fig. 2). Their use is concentrated in the east from South Australia to Cape York. Other appearances are found in northwestern North Australia, Melville and Bathurst Islands, and possibly also in northeastern Western Australia.\(^6\) There seems to be no uniformity among these northern appearances, or between them as a group and those found in the east. Individually, however, there are some weapons in North Australia which in a general way can be classified

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\(^4\) In most regions these weapons are secondary but among some tribes, such as the Wailpi in northeastern South Australia, they are preferred to others (25, p. 48).

\(^5\) All early and original reports indicate that the Tasmanians were equipped with only throwing-sticks having equal diameter throughout or slightly tapering from one end to the other. Bonwick (5, pp. 42–43 [1870]), without mentioning his authorities speaks of them as being "knobbed at each end," and as often having a "mushroom top." Roth (42, p. 71), citing Lyne, describes a specimen thirty inches long, about one and three-quarters inches thick at the heavy end, and one and a quarter inches at the handle end, and adds "The heavy end was sometimes knobbed." These statements are obviously ambiguous and may indicate only slight variations from the ordinary form. It is also possible that Australian aborigines, brought in by the colonists, may have been responsible for the introduction of some throwing-clubs, as they were for shields, barbed spears, and other Australian traits. Noetling (39, p. 64 *et seq*.), who seems to have made the most critical study, makes no mention of other than the typical forms as defined in this paper.

\(^6\) A short, tapering, heavy stick from Mulla Bulla, 1920 (UP).
Fig. 2. Distribution of throwing-sticks and throwing-clubs in Australia.


*Smaller with blunt ends.* Western Australia—Broome, Dampierland, Derby, Fitzroy, King Leopold Range (WAM); Roebuck Bay, Cygnet Bay (SAM); Wororra tribe (35, pp. 23-24, 30). North Australia—Melville Island.

*With gum handle.* Western Australia—Kimberley, Roebourne, Northampton, Greenough, Laverton, Murchison, Kanowna, Kookynie, Eucla (WAM). South Australia—Fowlers Bay, west coast (SAM).

*Throwing-stick-adzes.* Western Australia—Isdell Range, Laverton, Mt. Magnet, Kookynie, western Kimberley, Bunbury, Esperance, Geraldton hinterland, Hammersley Range, Lyons River (WAM); Gascoyne (SAM); Roebourne (NMV); Mt. Hahn (UP); Sherlock (10, Pl. 5); Murchison (9, p. 346; 26, p. 8; 27, p. 272). South Australia—Fowlers Bay, Tempe Downs (SAM); Mt. Remarkable, Dieri (55, pp. 65, 74); Everard Range, Warrina (27, pp. 272, 280); [Legend continued on next page.]
with certain examples in the east. A historical relationship must not be implied necessarily as the result of our classification, for what appear to be historical similarities actually may represent what are only accidental resemblances. Generally speaking throwing-clubs seem to be present in those areas where throwing-sticks are lacking, and lacking in those regions where throwing-sticks are present.

A classification of throwing-clubs into types is a most difficult matter as a result of the many and great differences in their shapes and features. It is possible to place the majority into two general groups but it must be realized that each of these is characterized by great variation. In addition, there are many specimens in the museum collections which do not fit into any major grouping. Wood lends itself very readily to the development of variants and the use of this commodity may be responsible in part for the occasional manufacture of weapons with unusual shapes and features.

Throwing-clubs with body flares. The most common class of throwing-club is shown in Figure 3. These specimens differ from throwing-sticks in that each is characterized by a diameter which increases gradually from the handle end and more or less abruptly from the head to form a flare or bulge in the forepart of the body of the weapon. Both extremities are generally pointed. This type, with numerous variations, is widely distributed from eastern South Australia to Torres Straits.

Specimens, differing in details, but coming under the same general classification are also found on Melville and Bathurst Islands. A sub-type having a bifurcated head is reported from the Mackay district, Queensland, and is present in a somewhat different form on Melville Island (fig. 3g, h).

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Specialized adze handle. Western Australia—Gascoyne (SAM); Geraldton (UP). South Australia—Everard Range, Coopers Creek (SAM). Central and North Australia—Macdonnell Range (SAM); Katherine River—Victoria River (UP); Charlotte Waters (55, p. 91). Queensland—Rockhampton, Palmer River, hinterland of Princess Charlotte Bay, eastern and gulf coasts, Wellesley Island, Endeavour, Bloomfield and Mitchell Rs. (all with iron blade) (44, Vol. 7, pp. 20–21).

Double adzes. Western Australia—Geraldton hinterland (WAM); Gascoyne, Nannine, Lyons River (SAM). South Australia—Coopers Creek, Frew River (SAM). Central and North Australia—Macdonnell Range, Barrow Creek, Tennant Creek, Katherine River (SAM); common in Central Australia (52, p. 640). Queensland—Glenormiston (QM); Pitta Pitta, Mitakoodi, Boulia, Lake Nash (43, p. 101); western Queensland (44, Vol. 7, 20–21).

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Throwing-clubs with bulbous heads. Another general class of throwing-club is that with a symmetrical bulbous head carved in the solid. In most cases, the shaft or body of the weapon, like that of a throwing-stick, is more or less of the same diameter throughout its length, and is set off rather distinctly from the head part. As the illustrations in Figure 4 demonstrate, the heads vary greatly in size as well as in shape. Some are spherical, some conical, some nondescript. Others show combined forms which include many varied arrangements of proportions. This class of throwing-club is commonly distributed from the central coast of Queensland to Victoria and southern South Australia. Cruder examples, in which the head is not so sharply set off from the body of the weapon, or in which the head is composed of more or less unrefined roots, are found in a number of
regions which, for the most part, appear to be peripheral to the other appearances (fig. 4j).

Marpungy. Marpungy is the name given to a type of weapon widely distributed in southeastern Australia. The larger specimens are clubs, the smaller ones, throwing-clubs. They seem to be lacking in northern Queensland and the Cape York Peninsula.

Pineapple-head throwing-clubs. A highly specialized type of weapon is the so-called pineapple-head throwing-club (fig. 8f). This type, apparently restricted to the area extending from northeastern South Australia to the Cape York Peninsula (see map, fig. 2) is made entirely of wood, although, it is interesting to note, nails have been used occasionally in recent times to give, or to extend, the studded appearance.

Lil-lil. The lil-lil is a very flat curved missile weapon with a boomerang-like body and a wide flat head which projects on the concave side (fig. 8g). It could be classified as either a variant boomerang or a variant throwing-club. It appears to be restricted in distribution to New South Wales and the adjacent parts of Victoria and Queensland.

The lack of throwing-clubs in Tasmania and most of Western Australia and their concentration in eastern and northeastern Australia introduces an interesting problem in origins. Except for the appearances in North Australia and the unsatisfactory indications for the adjacent northeastern

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8 Attention should be called to the Kul-luk, in Victoria, a somewhat similar but less curved weapon (see 48, Vol. 1, p. 308). In slightly varying forms, the marpungy is often met with under such names as burrong, langeel, leonile, and bendi, according to district (see also 16, p. 317 et seq.). Mannum, Robe, Murray R., Paroo R., Point McLeay, Adelaide Plains (SAM); Paroo, Clarence, Richmond and Bellinger Rs., north coast New South Wales (AM); Mackay (NMV); Rockhampton, upper Fitzroy R., Brisbane, Yaamba, Broad Sound (large) (44, Vol. 13, p. 209); lower McLeay R., Lake Albert, Milmendura, Encounter Bay, lower Coorong, Port Macquarie, Tweed and Nambucca Rs. (18); Coomoorooloo (36, p. 334). See also 48, Vol. 1, p. 302.


10 For specific description, see 18. Size varies up to two feet in length and up to five inches in width of head. Thickness generally is not more than one-half inch. Many have incised designs. Murrumbidgee, n. and w. Riverina, Swan Hill, Macquarie R., Cape Western, Coolabah, Oberley Holding, Bogan R., Lachlan-Darling Rs., Echuca, Yandilla, western New South Wales (AM); Gippsland, Kulkyny, Angeldool, Port Macquarie, Swan Hill, Barwon, Murray, upper Clarence, Balonne Rs. (18).

11 The derivation of the throwing-clubs in northern Australia, apparently isolated from
part of Western Australia, we find the general distribution of throwing-clubs concentrated in one major continuous distribution which stretches from Cape York to the Great Australian Bight.

From the technological point of view there can be no objection to believing that throwing-clubs may have developed from throwing-sticks. Certainly it is possible in any large museum collection to find scores of examples which appear to be transitional from the throwing-stick to the various kinds of throwing-club. At the same time we must not overlook the fact that there is a continuity in the distribution of throwing-clubs as far as Torres Straits, the point of introduction of many alien traits, and this suggests that throwing-clubs as a class may not be indigenous to Australia but derived from a foreign source.

In respect to specific types of throwing-clubs, however, we cannot be at all certain as to the points of origin of most of them. Some types may have diffused into the continent, others may have been invented as a result of foreign influence, whereas still others may have been derived from throwing-sticks prior to the intrusion of foreign influences. At the present time, it is impossible to determine what the actual circumstances of origin may have been, and all these possibilities must be retained until further evidence, perhaps to be furnished by archaeology, may be forthcoming.

The only throwing-club types the derivations of which seem fairly certain are the lil-lil and the pineapple-head form. The former by virtue of its specialized form and restricted distribution appears to have developed in southeastern Australia. The pineapple-head probably has a foreign origin. It seems permissible to infer that either the wooden form represents the application in Australia of the features of the New Guinean stone-headed form to a different material, or that the idea of an older and now extinct wooden form in New Guinea diffused across Torres Straits before the stone variety developed.\(^{12}\)

Of the other Australian throwing-clubs, none shows more than general resemblance with New Guinean and Melanesian weapons, with the possible exception of the marpuny, and all are distributed in regions sufficiently far from Cape York as not to suggest immediately a foreign deriv-
tion, unless we are to assume that they have been replaced in northern Queensland and the Cape York Peninsula by later arrivals such as the pineapple-head type.

It appears, therefore, that until we have more data we can say that the evidence suggests that Australian throwing-clubs, as a class only, may owe their origin to New Guinea but that there is no specific evidence to indicate that the places of origin of definite types, with the notable exception of the wooden pineapple-head form, lie outside the Australian area in which they are now found. Whether Australian throwing-clubs as a class are or are not historically related to Australian throwing-sticks, it seems likely that in many areas the two have influenced each other to produce new varieties in form.

*Handles carved in the solid.* In contrast to the pointed or rounded ends of throwing-sticks or their knob handles of gum, many throwing-clubs of Victoria, New South Wales, eastern South Australia, and southern Queensland are equipped with handles carved in the solid. Apparently these do not appear in northern Queensland and may represent an indigenous Australian development. Some are round, some elongated, and others appear as a tapering series of rings (figs. 4f, i; 5e).

**CLUBS**

Ordinary clubs—round, peeled, stout sticks with or without incised or painted designs—are lacking in Tasmania but apparently are quite widespread, and possibly almost universal in Australia. Relatively few localities, however, are represented in the museum collections and, as a result, we cannot be specific regarding distribution.

The simplest and most common type of club in North Australia, Central Australia, and central and northern Western Australia varies in length from three and a half to four feet, is round in cross-section, plain or longitudinally fluted, of equal diameter throughout or slightly tapering toward the roundly pointed ends (fig. 5a).12 A few scratches for a grip are often

12 These clubs are reported for all the central tribes, but are made particularly by the Kaitish and Warramunga, and traded to the Arunta, Loritja, etc. (51, pp. 602–604). See also 49, pp. 366–68, for the Tjingilli and for northern Australia, where they are said to be common. This statement may apply only to the western districts, for Tindale (57, p. 99) reports clubs lacking for Groote Island. A similar condition may be true for the adjacent mainland. The Nungubuyu are said to secure clubs from the Mara to the south. Whether they are bartered to other tribes of Arnhem Land we are not told, but most of these tribes apply variations of Nungubuyu terms to them. Only throwing-clubs are listed for Bathurst Island (2, pp. 300–301). No mention of heavy clubs for Melville Island has been found in the literature consulted. See 26, p. 11, for Kimberley. WAM—Wiluna, Broome, Roebourne, Dampierland and Isdell Ra. (10, Pl. 2), Mallina tribe.
found at the handle end. In northern Queensland longer clubs are used. Many have sharpened ends, apparently for digging.¹⁴

Fig. 5. Clubs. a, b, Wardaman tribe, North Australia (UP); c, Digging-stick, Broome (UP); d, e, Kakadu tribe, North Australia (SAM); f, New South Wales (SAM). (Scale: f, 3 feet 6 inches.)

Digging-sticks. Digging-sticks, four to four and a half feet in length, are probably used as clubs in all regions where known when other weapons are not available. It cannot be said how widespread their use may be, for these implements are not abundantly represented in the collections.¹⁵ In

¹⁴ See 43, p. 150, for Boulia, Cloncurry, Flinders, Leichhardt-Selwyn and Mitakoodi; 44, Vol. 13, p. 209, for middle Palmer R., Princess Charlotte Bay and Rockhampton. See 28, p. 21, for the Wonkonguru of South Australia.

¹⁵ Digging-sticks are known from all the states but may be lacking in local areas.
many regions they are neatly finished products; in others they seem to be no more than crude pointed sticks. They are definitely lacking in Tasmania.\(^{16}\)

In the Port Essington-Darwin region and the nearby coast to the south a pole-like club, having the same diameter throughout or tapering slightly and with squared off extremities, is present (fig. 5d).\(^{17}\) It is also in this general region that we find clubs with wide, flaring spatulate-shaped heads which taper gradually to either a rounded or a peculiar concave butt (fig. 5e). They seem to vary considerably in width, thickness, and in cross-section of the head part.\(^{18}\) A not dissimilar weapon with a rounded butt also is present in the Kimberley region.\(^{19}\) Some apparently should be classified as sword-clubs. Many in North Australia have handle grips carved in the solid (fig. 5b), but these are distinctly different from the handles of the throwing-clubs of southeastern Australia.

A type of club with a slight bulge in the middle of its shaft is found in New South Wales and the nearby portions of Queensland and northeastern South Australia (fig. 5f).\(^{20}\) It is often equipped with a handle cut in the solid and similar to those on the throwing-clubs of the same region. Many specimens are decorated with incised geometrical designs.

We have already considered the marpunyng, the larger examples of which are clubs, the smaller ones throwing-clubs. Roth reports a similar functional difference for most of the weapons of northeastern Queensland; those having a light weight being missiles, the heavy ones striking-clubs.\(^{21}\)

**Boomerang-clubs.** An interesting weapon is the so-called boomerang-club which embodies the shape of a simple boomerang, bi-convex in cross-section, and the size of a club (fig. 7j). Specimens have been collected from a number of localities in the eastern parts of the continent, principally South Australia, southern Central Australia (apparently traded from the east or south),\(^{22}\) western New South Wales, and Queensland.\(^{23}\) In Victoria

\(^{16}\) See 39, p. 73. Apparently the ordinary throwing-stick was used for digging roots.

\(^{17}\) Larrikiyas, Wogaits, Sheraits, Berringin (1, p. 36); generalized for the adjacent inland tribes (49, p. 366 et seq.). SAM—Darwin, Port Essington.

\(^{18}\) See 1, p. 36; 49, p. 366, Pl. 16; 38, Vol. 1, p. 308.

\(^{19}\) WAM—Broome, Dampierland, Derby. See 3, pp. 169–70.


\(^{21}\) 44, Vol. 13, p. 207.

\(^{22}\) See 51, pp. 596–99. It is believed that these weapons, varying up to four feet in length found among the Arunta and Ilparra, have come from Queensland. They are said to be more common among the southern Arunta. Some with different designs are said to be manufactured by the Urabunna and occasionally by the southern Arunta and Loritja. Boomerang clubs show great variability in length. In northern Queensland most of the specimens appear to be not
a somewhat heavier and thicker weapon was used.  The distribution, however, may be much greater than that indicated, for a typical specimen has been collected from Yundamindra, Western Australia. The quirriang-an-wun of the Murray River tribes is similar functionally but differs somewhat in shape.

Sword-clubs. These weapons are similar to boomerang-clubs in their bi-convex cross-section but differ in that they are straight or nearly so and in the east often have a carved knob handle (fig. 7l). The specimens available indicate a distribution extending from Eucla to Victoria and the central coast of Queensland. Most examples are not over four feet in length. Weapons with the same general characteristics but with numerous minor points of difference are also found at Melville Island. These are intricately carved and painted and possess carved handles or bifurcated butts. With only very general similarities and with numerous minor differences between the sword-clubs of North Australia and those of the east and southeast, a historical relationship does not readily suggest itself. However, there is an unusual type of sword-club, found in both eastern Queensland and in North Australia (fig. 7k). It would seem that there must be some historical relationship between these two appearances.

BOOMERANGS

Boomerangs, as a class, are widely distributed in Australia but are not continental. It is important to note that they are lacking in Tasmania and

less than three and a half feet nor more than four feet long. In South Australia many examples are five feet in length. The longest weapons are found in the Coopers Creek-Diamantina district where specimens eight feet in length have been collected (SAM).


24 Kul-luk (45, Vol. 1, p. 308). This weapon is suggestive of the marpunyng.

25 UP. Found in a cave in 1891.

26 The average length is given as thirty-six inches (48, Vol. 1, p. 316).


28 49, Pl. 18. They do not seem to be used on Bathurst Island, at least no mention is made of them by Basedow (2).

in all the northern peninsulas of Australia, the Kimberley coastal country,\textsuperscript{30} Groote Island, and North Australia approximately north of a line drawn from the Katherine River to the Roper River,\textsuperscript{31} and in the Cape York

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{boomerang_distribution.png}
\caption{Distribution of boomerangs in Australia.}
\end{figure}

\textsuperscript{30} Reported lacking for the northern Kimberley coastal tribes by Brockman (6, p. 17), Port George IV by Love (35, p. 30), and for the inland tribes of the west Kimberley coast by Froggatt (23, p. 651). The last, however, mentions two types for the western coastal tribes and Stokes (54, Vol. 1, p. 91) found the boomerang at Beagle Bay. Boomerangs are now reaching Port George IV.

\textsuperscript{31} Fieldnotes, 1930. Basedow (1, p. 37) found them lacking among the western coastal tribes, to some of whom they are now being traded (53, Vol. 4, Pl. 5). Foelsche (22, p. 12)
peninsula, north of the Mitchell and Palmer Rivers (see map, fig. 6). There are also a few minor districts where they seem to be unknown.

Generally speaking, boomerangs as a class can be regarded as a special form of throwing-stick. They differ from the latter only in their somewhat greater curvature and their bi-convex or semi-oval cross-section. They are used like throwing-sticks for both throwing and striking in hunting and in fighting. These remarks apply in a general way to all varieties of boomerangs except that specialized type which has attracted so much attention, the returning boomerang. The latter, which numerically represents an extremely small proportion of the boomerangs of the continent, is usually regarded by the natives as a toy which, with a few exceptions, seems to be used for neither hunting nor fighting.

Any present attempt to classify boomerangs into types and varieties meets with so many difficulties that the effort is futile. When all the extremely variable features are taken into consideration it is obvious that any inclusive classification would be so unwieldy that it would not serve the purposes for which it was intended. A rough grouping on the basis of general similarities, however, may be of some value. Accordingly, insofar as we have information, the distributions of some of the more prominent forms are given below.

The most common type of pointed boomerang in Western Australia is a light, thin blade, symmetrical or asymmetrical in shape, having one flat

reported them lacking along the northern coast. Tindale (57, p. 99) for the Gulf coast, gives the Allawa on the Roper River as the most northern tribe to make them. See also 21, p. 355; 62, p. 224, for northern Arnhem Land.


They have been denied for the Everard, Fraser and Blythe Ranges in South Australia, but present on Hampton Plains (see 61, p. 728; 27, p. 277; 47, p. 86). King did not see them at King George Sound in 1821 (31, Vol. 2, p. 137), but this does not necessarily mean that they were unknown there at that time. Helms reported them made inland and traded to the coast (27, p. 289). Roth (44, Vol. 13, p. 202) got no information at Keppel Island, a place where many mainland traits are lacking. Sarg (45, pp. 7-8) and Eymann (21, p. 363) delimit a large region along the southern coast as lacking boomerangs. Their information may be correct for some small districts, but the museum collections show an abundance of specimens for this region as a whole.

For discussion see 13.

It must be understood that this list is not an inclusive one, and that many other forms, represented as a rule by very few specimens, have been purposely excluded. It must also be realized that in the majority of cases the distributions are based upon specimens or other data which show only that a particular type is present in an area, but is not necessarily manufactured there. It must not be assumed that the distribution of use of any type is coextensive with the distribution of its manufacture. There are very few cases in which we have information concerning trade and diffusion routes.
and one convex surface, or two slightly convex surfaces (fig. 7a). It is found in the Kimberley district, where it is often decorated with incised designs and painted bands. The distribution of undecorated specimens ex-

![Boomerangs diagram](image)

Fig. 7. Boomerangs: a, b, Kimberley region; c, d, southern Western Australia; e, f, i, Victoria; g, h, Queensland. Boomerang-clubs: j, Coopers Creek. Sword: k, Cardwell. Sword-club: l, South Australia.

Acknowledgement must be made to Mr L. Glauert (Curator, WAM) for his help and suggestions in grouping the specimens from Western Australia. WAM—Kimberley, Wiluna, Beagle Bay, Kookynie, Laverton, Ashburton. SAM—Broome, King Leopold Range. UP—Mulla Bulla, Halls Creek, Maninga Marley, Turkey Creek, Leopold Range, Sunday Island, La Grange Bay. 26, p. 9, Murchison. 10, Pl. 2, Roebourne. All fairly symmetrical. Areas in which one arm is definitely longer than the other include (WAM) Kimberley, Wyndham, Pender Bay, Derby, Isdell Range, Broome, and Point Cloates.
tends southwestward apparently to the Ashburton, Laverton, and Kookynie districts.

A specialized variety similar in many respects, but with certain features accentuated, is shown in Figure 7b. One arm is considerably longer than the other and the greatest width is at the angle, from which it gradually decreases toward each extremity. This variety seems to be concentrated in the Kimberley-Roebourne region, although specimens have been collected farther south.27

Another pointed boomerang of Western Australia is a light, thin, short, narrow undecorated weapon, distributed in the southern part of the state from Geraldton southward to the ocean and eastward to Eucla.28 The arms of this weapon tend to be approximately equal in length but differ in that one has a slight concavity in its outer edge, whereas the other has a convex outer edge (fig. 7c).

The most common type in Western Australia is a thin, plain blade with round ends, symmetrically shaped or with one arm slightly longer than the other (fig. 7d).29 One surface is convex, the other flat. The general distribution extends from the Roebourne area to the southern ocean and to the South Australian border. It seems to be lacking in the Kimberley district and perhaps in the eastern desert regions.

In South Australia somewhat different boomerangs are encountered. One general and fairly symmetrical type is long and narrow but heavy as a result of its thickness. The ends are round in a medium degree. Many of these specimens are decorated with incised semicircular designs, divided often into two quarters. In other cases, there are series of zigzags in various arrangements. This type seems to be concentrated in the south.30 Not dissimilar boomerangs, thirty to thirty-six inches in length, with very fine longitudinal flutings, are found in the Coopers Creek-western New South Wales-western Queensland region.31

27 WAM—Kimberley, Lombadina, Gascoyne, Kookynie, Murchison. SAM—Cygnet Bay, Derby.
30 SAM—Eucla, Eyre Pen., Gawler Range, west coast of South Australia, Penong, William Creek.
31 SAM—Mt. Burrell, 200 miles north of Coopers Creek, Diamantina R., Dieri tribe.
A shorter, broader type, with round ends, and symmetrical or asymmetrical shape also appears in the southern part of the state.\(^42\) It has, as usual, one convex surface, whereas the other varies from flat to slightly convex. Slightly longer boomerangs with a medium width and an oval cross-section are also found in the same general region.\(^43\) A cruder and smaller, but otherwise similar type appears in New South Wales.

In Victoria and southern New South Wales there seems to have been only two important types of boomerang, the wongui or returning boomerang, and the barngeet or war boomerang, both of which are found in variant forms (fig. 7e, f, i). The two are difficult to distinguish, for the latter often exhibits a slight twist, although it cannot be made to return.\(^44\) For the remainder of New South Wales we have very little information and only a few specimens upon which to base an opinion. In the western regions there appears to have been similarity with forms in South Australia, and in the north Queensland types appear.

The most common form of boomerang in a large part of Queensland seems to have been similar to that shown in Figure 7g, found with minor variations in form and decorations from northern New South Wales to northwest central Queensland and eastward to Herbert Vale. It seems to be lacking in the upper Georgina area, the extreme northern regions, and in many coastal areas in the east.\(^45\) These boomerangs are fairly long (thirty inches), and show either one flat and one greatly convex surface, or two convex surfaces. The apices may be round, pointed, or emarginate in form with central mucronate extensions. Further north, at Normanton and along the Gulf coast, the boomerangs become heavier and clumsier and a more or less acute angle gives way to a curve only slightly perceptible.\(^46\)

The Central and North Australian boomerang. One of the most distinctive types of boomerang is that found in Central and North Australia. It is a slightly curved weapon with one arm longer than the other, rounded ends, a convex upper surface and a flat under surface (fig. 8a, b). It is usually fluted longitudinally except in certain peripheral districts. This weapon is often spoken of as a fighting boomerang, but it is used for both hunting and fighting both by throwing and striking and is non-returning.

\(^{42}\) SAM—Gawler Range, Penalumba, Yorke Pen., Great Bight, Fowlers Bay, west coast of South Australia.

\(^{43}\) SAM—west coast of South Australia, Yardea, Lake Callabonna, Streaky Bay.

\(^{44}\) See 48, Vol. 1, p. 313.

\(^{45}\) Northwest central Queensland (43, pp. 143-45); 16, 17, 19, 20; 36, p. 43; northern New South Wales, central and eastern Queensland.

\(^{46}\) 44, Vol. 13, p. 203.
This boomerang prevails from the Daly River-Katherine River-Roper River area of North Australia, the northern limit of boomerangs, to the Coopers Creek-Warrina district of northern South Australia, and from the upper Georgina area of western Queensland to at least the western Kimberley region (see map, fig. 6). Throughout this extensive distribution the characteristics are quite similar. It should be noted, however, that on the northern, northeastern, southwestern, and also possibly on the northwestern boundaries of this type, the specimens often lack the flutings or are more crudely made.

In the greater part of the distribution delimited, this type of boomerang is the only one present. This is particularly true in the central portion of the area. In western Queensland, northern South Australia, and Western Australia other boomerangs are also in use. There is quite definite evidence to show that these marginal areas have been invaded by diffusion from the more central regions. It is presumable, therefore, that this type of boomerang was developed somewhere in the southern North Australia-northern Central Australia region and the question arises as to whether its diffusion to its present extent was at the expense of other boomerangs previously present within these boundaries. There seems to be no answer to this question at present.

*Beaked boomerangs.* A peculiar variety of the “fighting” boomerang is that with a beak or hook (fig. 8c). Boomerangs with this feature are found in an extensive area from western Queensland to the Kimberley region and

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47 Specimens from most of this area are common in most museum collections. Kimberley is represented in WAM.

48 Fieldnotes; at the Daly River they are traded from the south. 53, Vol. 4, Pl. 5; crude specimens come from the Normanton-Burketown district (AM). The Loritja boomerangs are cruder than the Arunta: the grooves are roughly cut and there has been little attempt to smooth the flatter side. The best examples come from northeast of the Arunta (51, p. 596). In UP there is a very crude specimen said to come from the Ngurla tribe in the Roebourne area.

49 In western Queensland, these boomerangs are traded from the upper Georgina to the Leichhardt-Selwyn areas (43, p. 145); a historical derivation from the north has been implied for the appearances among the Arunta and Loritja tribes of southern Central Australia (51, p. 596). Mention has been made of the cruder examples in the Katherine River and Daly River country, the northern periphery. I have been informed by Mr Norman B. Tindale that specimens are occasionally traded northward by the Allawa tribe in the Roper River district, the most northern tribe in that area to use them as regular weapons. See also 57, p. 99. A similar derivation from North Australia is also indicated for the appearances in the Kimberley region where many traits of North Australian character are to be found (information, Mr L. Glaueurt, WAM), and for the Normanton-Burketown district of adjacent northern Queensland where these specimens are often decorated at one end with bands of paint, red or white or both, as is found upon many North Australian weapons.
from the Gulf of Carpentaria to coastal South Australia (see map, fig. 6). A large part of this distribution, however, is the result of trade, for the area of manufacture appears to be much more restricted. Although information is not complete, it would appear that beaked boomerangs are a development of southern North Australia or northern Central Australia, still the primary area of their manufacture. Investigation in that area might make it possible to restrict the area of origin to a relatively small region within the area of manufacture of the typical "fighting" boomerang.

The only other appearance of a boomerang with a hooked extremity is found in the Coopers Creek district of South Australia, where a number of very unusual forms are used in ceremonies (fig. 8d, e). These objects are

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Fig. 8. a, b, Central Australian-North Australian type boomerang (a, fluted; b, hewn). c, Beaked boomerang. d, e, Odd, hooked boomerang, Coopers Creek. f, Pineapple-head throwing-club. g, Lil-lil. h, Marpungy.

very different from the typical beaked boomerangs but may be historically related insofar as the hook is concerned. Ordinary beaked boomerangs have been traded to this region and may have influenced some local pattern. On the other hand, such a trade may have been so recent that it cannot be held responsible for the local appearance. There are so many points of

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50 The manufacture of these weapons has been reported by Roth (43, pp. 145-46) for eastern North Australia and the adjacent upper Georgina district of Queensland whence they are traded eastward. North Australia also seems to be the center of diffusion for the appearances in northern Western Australia, according to information collected with Kimberley specimens in WAM. The most northern tribes among whom beaked boomerangs have been reported, such as the Wardaman in the Katherine River region, do not make them but barter them from the south. A similar exchange arrangement has also been reported for the southern part of the continent, where the Arunta and neighboring tribes do not manufacture them but secure them from the Warramunga who are known to produce them (51, p. 602).
difference between the typical beaked boomerang and those at Coopers Creek that this may be a case of convergent development.

The beaked boomerang is reminiscent of the marpungy in some of its extreme forms. The centers of distribution of the two, however, appear to be so far apart that for the time being there seems to be no reason for suspecting a possible historical relationship.

*Incised boomerangs.* As a general rule, boomerangs are not decorated with incised designs throughout most of the area west of Central Australia and in many eastern localities. In North Australia and northern Central Australia and in the adjacent Normanton-Burketown region of Queensland the use of painted bands, lines or dots, or sometimes other designs is typical. Horizontal painted bands are common in the Kimberley district.

The use of incised designs on boomerangs seems to be found in two non-contiguous areas: most of eastern Australia from the Gulf of Carpentaria to New South Wales and eastern South Australia on the one hand, and the Kimberley district of Western Australia on the other (fig. 6). These two regions are separated by the area of the un-incised but fluted fighting boomerangs in the central regions which, as we have seen, has been expanding, apparently in all directions. If this expansion has been at the expense of other boomerangs, it seems possible that the two areas of incised boomerangs may have been formerly greater and, therefore, previously nearer to each other. Such a possibility introduces the question as to whether there is any historical relationship between the custom of incising boomerangs in eastern Australia and that in the Kimberley region, a problem which cannot be considered at this time.

*Returning boomerangs.* The boomerang for which the natives of Australia are celebrated is the returning type, which, when properly thrown in the air, describes a loop or series of gyrations and returns to fall within a few feet of the thrower. In some cases, the returning boomerang is thrown to strike the ground, whence it leaps into the air and commences its flight. It is important to note, however, that if it strikes anything during its flight, its course will be broken and it will fall to the ground. It is stated that the paths of no two returning boomerangs exactly agree and that no boomerang follows the same course in each flight. As we have already re-

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81 In 30, the authors speak of boomerangs thrown to strike the ground in New South Wales and Queensland. In one instance, the ground was struck in ricochet fashion three times before the flight commenced, whence the boomerang returned. For Western Australia Moore (37, p. 47) reports that boomerangs were thrown first against the ground in some cases.

82 Such a condition is well known but few writers have taken the trouble to mention it. See 29, p. 248.
marked, the returning boomerang in most instances is regarded as a play-
thing and is seldom used in hunting or for fighting. The physical properties
of these objects have been described in detail by a number of writers and
the dynamics of their peculiar flights carefully studied and recorded by
Walker.\textsuperscript{43} As the result of warping it is often impossible to distinguish
returners from ordinary boomerangs in museum collections.

Returning boomerangs are widely distributed but are not found in all
regions in which the ordinary forms appear (see map, fig. 6). There seems
to be no indication that the returning kind is ever present by itself. The
major regions in which returners are or were used include at least parts of
Victoria, New South Wales, Queensland, South Australia, and Western
Australia.\textsuperscript{44} The main negative area, aside from those in which no boomer-
angs at all are present, is the Central Australia-North Australia region. We
thus find that returning boomerangs, like incised boomerangs, occupy an
area which almost surrounds an area in which they are not used, but where
the so-called "fighting" boomerang is present. Since the latter is definitely
known to be diffusing outward into areas where the former is now found,
the question arises as to whether returning boomerangs formerly occupied
a wider distribution in a part of the region in which we now notice only
the so-called fighting type. This question cannot be answered at present.

\textit{Origin of boomerangs.} There seems to be no good reason for believing
that boomerangs are not indigenous to Australia. As we have already seen
they are lacking in the three northern peninsulas of the continent into
two of which, the northern Kimberley district and northern North Aus-

\textsuperscript{43} See 58, p. 23 \textit{et seq.}; 48, Vol. 1, pp. 311–29; 15, \textit{Boomerangs}. A returner must have a
curvature resembling the arc of a hyperbola, a bi-convex cross-section with one surface more
curved than the other, and a longitudinal twist of from 2° to 3°.

\textsuperscript{44} For Victoria (48, Vol. 1, p. 311 \textit{et seq.}); New South Wales (56, p. 218); Queensland—
South Australia—4, p. 81, Ooldea; 29, p. 248, Coopers Creek. Western Australia—48, Vol. 1,
p. 311 \textit{et seq.}; 37, p. 47; SAM—(Round ends) Eucla, Gascoyne, Cygnet Bay, Roebuck Bay,
Lyons R., Geraldton; (Pointed ends) Broome, Roebuck Bay. 50, pp. 20–21, Victoria, New
South Wales. Lacking in Central Australia, 50, p. 19; 27, p. 268, Warrina; 28, p. 82, Wonkon-

\textsuperscript{45} Love (35, p. 30) reported in 1917 that boomerangs were formerly unknown, and were
being introduced by King Sound natives to the Wororra tribe at Port George IV. In North
Australia boomerangs are now being traded northward into the Daly River country (see 53,
Vol. 4, Pl. 5; 14). In eastern North Australia they are not used north of the Roper River al-
though a few specimens are traded to the north. Tindale for Groote Island (57, p. 99) reported
that they were known "only from exaggerated rumors of their wonderful killing power." He adds that "On showing boomerangs to Talakurupu men (East Bay) they became fright-
ened, and upon striking a throwing attitude, they fled in terror."
Cape York Peninsula, our information is not so specific, but the crude boomerangs found there suggest that they are of recent introduction from the south and such a conclusion is supported by linguistic data.\textsuperscript{66} If our interpretation of the evidence furnished by distribution and the known directions of diffusion is correct, it would seem to follow that both ordinary and returning boomerangs are not only indigenous to the continent but that they have not diffused from Australia to any other region, for they are lacking in the only areas where foreign influences are known to have come, the only regions, therefore, which could have reciprocated culture borrowing.\textsuperscript{67} In a culture where throwing-sticks undoubtedly have been in use for a great period of time we do not have to look far for a possible as well as a most reasonable basis from which boomerangs could have been derived.\textsuperscript{68} A discussion of the question of relationship between Australian boomerangs and the so-called boomerangs of other parts of the world has been given elsewhere (13).

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The following abbreviations are used:

<table>
<thead>
<tr>
<th>Abbreviation</th>
<th>Description</th>
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<tr>
<td>IAE</td>
<td>Internationales Archiv für Ethnographie.</td>
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<tr>
<td>JRAI</td>
<td>Journal of the Royal Anthropological Institute.</td>
</tr>
<tr>
<td>PS-J</td>
<td>Journal of the Polynesian Society.</td>
</tr>
<tr>
<td>RGSA-P[V]</td>
<td>Ibid. (Victorian Branch).</td>
</tr>
<tr>
<td>RSSA</td>
<td>Transactions of the Royal Society of South Australia.</td>
</tr>
<tr>
<td>SAM-R</td>
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\textsuperscript{67} The Australians are too poorly equipped with watercraft to have carried cultural influences to other peoples (see 12).
\textsuperscript{68} Various other weapons have been suggested but there is no evidence to support the claims made: leaf-shaped sword (32, p. 440); sword-club (48, Vol. 1, p. 316); sword (58, p. 340). The throwing-stick is favored by Spencer (50, p. 18).
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BERTHOLD LAUFER: 1874–1934

By ARTHUR W. HUMMEL

By the death of Berthold Laufer American anthropology and sinology lost its most broadly-trained and distinguished investigator. For thirty-five years he was almost the only sinologist in this country, and only in the last years of his life did he have the pleasure of seeing a few younger scholars being trained to carry on the tradition.

Berthold Laufer was born in Cologne, Germany, October 11, 1874, the son of a merchant, Max Laufer. He studied in the University of Berlin (1893–95) and in the Seminar for Oriental Languages (1894–95), taking his doctorate at the University of Leipzig in 1897. Throughout his university career he showed a preference for Far Eastern languages and ethnology, taking courses in Semitics, Persian, Sanskrit, Pāli, Malay, Chinese, Japanese, Manchu, Mongolian, Dravidian, and Tibetan. His doctoral dissertation consisted of a critical analysis of a Tibetan text, Klū bum bsadus pa'i snīṇ po, which with characteristic feeling he dedicated "In love and loyalty to my parents on their silver wedding anniversary, January 8, 1898." He studied Buddhism under Dr Franke, Chinese under Prof William Grube, Malay under the great grammarian, Prof von der Gebelentz, Tibetan under Dr Huth, and Japanese under Prof Lange—all great names in Far Eastern studies.

He came to the United States shortly after receiving his doctorate and remained in this country until his death. His loyalty to his adopted land grew, and of late years he frequently remarked that Chinese and Japanese studies must in this country follow American traditions and cease to be tied to the European pattern. American men and women of means stood solidly behind his studies as shown by the four expeditions to the Amur region (1898–99), the Jacob H. Schiff expedition to China (1901–04), the Blackstone expedition to Tibet and China (1908–10), and the Marshall Field expedition to the same country in 1923.

During 1903–06 Dr Laufer was connected with the American Museum of Natural History; in 1905–07 he lectured at Columbia University; and in 1908 he began his connection with Field Museum of Natural History whose curatorships in Asiatic Ethnology (1911) and later in Anthropology (1915) he held until his death.

His books on ancient jade, Han pottery, plant distribution; and monographs numbering some 150 on an astonishing variety of subjects, became at once indispensable works of reference. He usually confined himself to subjects of ethnological interest in which his wide reading, sound judgment, and training in a variety of languages were all used to good purpose. Stray
indications in T'ang poetry, an obscure reference in a local gazetteer, facts which his own eyes witnessed, were all noted down, and before he knew it, he was ready, after one great synthetic effort, to publish a monograph which for most of his readers seemed to leave nothing more to be said. These studies were often on the most surprising subjects—cormorant fishing, insect-musicians, pigeon whistles, optical lenses, Chinese baskets, loan-words in Tibetan—but in every one he made apt and startling comparisons which gave to his studies unusual cultural significance. Finding

![Berthold Laufer](image)

that Chicago was inadequately supplied with books for such broad research, he purchased more than 40,000 volumes of Chinese books which were equally divided between the Newberry and John Crerar libraries. One work from the latter collection, which in 1928 was transferred to the Library of Congress, contains the lost Sung (1210 A.D.) Kēng chīh ʻu, "Pictures on Tilling and Weaving," of which no other copy is known in the world (see T'oung Pao, Vol. 13, pp. 97–106). As late as 1932 he identified, in the home of a New York collector, four lost albums of pictures on the same theme which were painted for Emperor K'ang-hsi in 1696. Then,
with characteristic foresight he persuaded a patron of Chinese art in this country to present them to the Library of Congress.

Dr Laufer combined a love of scientific accuracy with unusual sensitiveness in the field of human relationships. This made him at times a severe—sometimes harsh—critic, and again a friend capable of unusual thoughtfulness and feeling. In his most valuable monographs his enthusiasms were seldom evident, but his semi-scientific utterances, or popular articles, were sometimes marked by emotive overstatement which the careless reader easily misinterpreted. In his later years collectors of art objects eagerly sought and obtained his criticisms, and younger scholars begged him to read manuscripts which he had no heart to refuse. These distractions diverted him from the studies that were most congenial to his mind, and must perhaps be reckoned among the anxieties that contributed to his premature death (September 13, 1934). Nevertheless since his death there have appeared in print studies—on the lemon, for example—which are among the best he wrote.

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BOOK REVIEWS

NORTH AMERICA


This study is a forceful demonstration "of the inevitability of founding ethnological methodology on a metaphysic of history." It is, moreover, a study of cultural change as affected by uncontrolled assimilation. Assimilation is here distinguished from acculturation, since the latter process should involve some measure of reciprocity and adjustment. Above all it is an epic of how, under the control of the United States government, a strong, friendly, and well adjusted tribe has been reduced in something over a century to a pitiful handful of individuals living in a state of intellectual and cultural barrenness. The development of the Ghost Dance Hand Game ceremony, since it was "the chief intellectual product of Pawnee culture in the last forty years," serves as a motif around which this sweeping change can be visualized.

As a study of uncontrolled assimilation this report is of fundamental importance to all administrators and students concerned with contemporary Indian problems. Lesser's dispassionate presentation of Pawnee history between 1803 and the present is particularly significant in the light of recent more promising trends in Indian policy. It is a tragic story of an intelligent and willing people grossly mishandled.

Putting their trust in the government the Pawnee made vigorous attempts to farm on a large scale, but Sioux war parties constantly thwarted their efforts. Not only did the government utterly fail to protect them but no efforts were made to organize the Pawnee for their own defense. Yet in 1876, when the North brothers recruited a company of scouts, every man in the tribe sought to enlist. Driven by the above causes to an unhealthy reservation in Oklahoma they formed band villages in the old pattern, but these were soon broken up by government policy in favor of individual allotments. The rise of the boarding school furthered the disintegration of group morale and all ceremonial activities were frowned upon. Then with the development of the leasing system and the selling of surplus lands to whites all incentive for work disappeared. The Pawnee were a scattered, dispirited, and hopeless people when the doctrines of the Ghost dance reached them, to stir up the last spark of cultural pride.

The bulk of the report is taken up with an extremely lucid, and well documented analysis of the renaissance and gradual relapse of Pawnee culture resulting from the Ghost dance of 1892. At this time all the closely integrated and soul satisfying manifestations of Pawnee philosophy, ceremonialism, and calendric ritualism had already vanished. Moreover, the economic and political basis of Pawnee life was irretrievably gone. It was therefore impossible to revive the once basic bundle ceremonies, but the last remaining sacred bundles were opened and their contents renewed. Old societies and medicine men dances were reconstructed and, in associa-
tion with Ghost dance rituals and visions, the various games of aboriginal times were taken up with enthusiasm. Since the bundle ceremonials and society activities called for esoteric and infinitely complex traditional learning that had in large part vanished, attempts at their renewal were largely abortive. As a result the games familiar to all of the people rather than to a priestly class, became dominant. Of these, the hand game survived the longest, and it is the detailed history of this institution, a gambling game that became a ceremony, which forms the nuclear core of the report.

The reviewer is particularly impressed by the breadth of understanding which this analysis of a seemingly minor feature of Pawnee culture presents. As the meanings associated with the hand game change, it is strikingly clear why they have changed in terms of Pawnee culture. At no time is the latter static but always in a state of flux and as it changes, the gauge, i.e., the hand game, responds like a delicate measuring instrument. In cross section, at any given moment, Pawnee culture presents an incomplete fusion of both vestigial and partially assimilated new features which would seem to defy any functional interpretation, or any possible cognition of process. Viewed in terms of change, however, i.e., in the light of full time perspective, these successive cultural states, as well as the economic and psychological changes bringing them about, are easily understandable. The fact that a culture rapidly disintegrating in the face of forced assimilation is under observation undoubtedly facilitates this remarkable demonstration of method. Few would deny, however, that all culture is a dynamic continuum, hence an understanding of any culture must always be in terms of change. It follows, therefore, as the author points out, that "historicity is neither an end nor by itself a means but a condition which must be recognized at every step," and that "functionalism in social anthropology which is divorced from time perspective is metaphysically false." In the opinion of the reviewer, this paper, both factually and methodologically, is one of the most important anthropological reports to appear in recent years.

DUNCAN STRONG

Bureau of American Ethnology


This is an excellently annotated presentation of reports and estimates of the white, negro, and Indian population of the eastern United States prior to 1790. It is based on the Dexter and Rossiter publications on American population, on new published material, official and unofficial, and on manuscript material from the Public Record Office in London, the Library of Congress, the New York Public Library, etc. Of value to students of the Eastern Woodlands and Southeastern culture areas is the section (pp. 194–206) dealing with population estimates for Indians of the western section of eastern United States: records for the Northern Department begin in 1677, for the Southern Department in 1708. There is also a valuable
bibliography of early sources (pp. xi–xxii) and a detailed index. "No systematic interpretation of the material has been attempted" (p. vi), but a wealth of factual information has been assembled.

YALE UNIVERSITY


This is the second volume of Plains Cree texts collected by Bloomfield on the Sweet Grass Reserve in Saskatchewan. The first\(^1\) dealt with the exploits of the trickster wishakhetchahk. The present collection contains a wider range of text types, grouped under four headings. The first section, Life and Worship, includes a few short accounts of the Sun dance and other ceremonies, and three love songs. The next two divisions are entitled The Past and The Powers Around Us. These, the bulk of the texts, are the kind of stories that comprise a large part of the conversation whenever middle-aged or old Cree gather and talk. Included are quasi-historical accounts of valorous deeds, tales of wondrous occurrences, and anecdotes concerning contemporaries. The last section, Sacred Stories, offers four additional trickster tales.

To this reviewer, engaged in field work on the Sweet Grass Reserve, these texts come as a valuable aid to ethnologic studies. They have furnished clues to traits that would otherwise not have been ascertained and have served to clear up some vague points. Thus the culturally and psychologically significant euphemisms and word avoidances could hardly have been uncovered through an interpreter. Matters of religious belief resistant to direct questioning have been illuminated by casual remarks in the texts. Thus the manifold connotations of the term manito referring to power, luck, skill, black magic, blessing, as well as to the idea of a single creator, are nicely revealed. Among a people whose religious concepts are as inchoate and unformulated as are those of the Plains Cree, quotations from textual materials furnish the best check on the ethnographer's formulation.

The number of tales concerning the Assiniboine testifies to their close alliance with the Cree. Two accounts of Blackfoot customs give the Cree version of how the Blackfoot treated their wives. European motifs may be traced in a few stories.

Some minor errors in translation occur. The word ayahsiyinwak is translated as denoting the Blackfoot. It refers rather to any hostile tribe. In Text 12 this error is confusing. The place named pâhunâh (p. 219) is Fort Carlton, not Battleford. The mahâtâhîtîwîn ceremony is translated as "potlatch." It bears little resemblance to the Northwest Coast ceremony except in the matter of gift giving.

All the tales recorded are now current among the Plains Cree with the exception of Text 42 which tells of a cannibal monster. The Windigo stories are from the Wood Cree and are rarely told on the plains. A few differing versions of some of these tales had been previously recorded by the reviewer. It was found that the several versions of the same tale tend to be handed down in family lines.

\(^1\) Sacred Stories of the Sweet Grass Cree (National Museum of Canada, Bulletin 60, 1930).
That the texts have been competently recorded and fairly translated has been ascertained by the expedient of reading them back to informant and interpreter.

D. G. Mandelbaum

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Hoijer's grammar of Tonkawa is a welcome contribution to American Indian linguistic science and provides what is on the whole a sound and thorough descriptive treatment of a language that is soon to become extinct. The essential features of the language may be very briefly summed up as follows: Tonkawa is mildly synthetic in structure and predominately agglutinative in technique, fusion being present only to a mild degree. The most fundamental morphological process is affixation, chiefly suffixation, prefixation being on the whole rare. Compounding is also of some importance. An interesting phonological process, consisting of the syncope (except under certain definable phonetic conditions) of the vowel of even-numbered syllables is found to affect themes but not affixes.

A small remnant of the Tonkawa tribe, formerly of central Texas, now lives near Tonkawa, Oklahoma, and the old language is now spoken by only six individuals, all of whom are quite old. Of these six, Hoijer worked with only one, John Rush Buffalo (described as having the greatest command of the myths and tales of his people). This is to be regretted, since it is usually of value, especially in working with dying languages, to use more than one informant, since certain valuable material, chiefly in the way of vocabulary (of great importance in comparative work), may have lingered on in the memory of one individual, but not in that of others. That this may be true also in the case of Tonkawa seems to be borne out by the fact that Dr Alexander Lesser (whose material on kinship terms is given on pp. 130–33) found a Tonkawa woman, Coachina Rush, to be more conversant with the kinship terminology than any of the other speakers of the language. According to Dr Lesser, Coachina Rush "was reputed to be the most informed Tonkawa then alive," and one wonders if it might not have been possible to secure other important lexicographical material from her.

Powell classified Tonkawa as an independent linguistic stock, and up to the present time it has not been possible to demonstrate with certainty any linguistic affinities for this stock. It is more or less generally assumed that certain extinct minor neighboring tribes, such as the Yojuane, Mayeye, Erviapiame, and Sana, spoke languages related to Tonkawa. Since the material on these languages is exceedingly scanty, it can scarcely be hoped that such a relationship can ever be definitely proved. In 1915 Swanton proposed the Coahuilteca stock with the following divisions: "one including Coahuilteco, Comecrudo, and probably Karankawa... and one represented by Cotoname and Tonkawa."1 At the time the

available material on Tonkawa was not very considerable. The other languages which he has included in the proposed stock are extinct and the material on them is very, very fragmentary. It is to be hoped that Hoijer with his greater abundance of material on Tonkawa may reexamine the facts with a view to a further elucidation of this possibility.

The possibility of even remoter connections for Tonkawa has been indirectly suggested elsewhere, but considerably more work in the way of descriptive grammars remains to be done before this interesting possibility can be adequately examined.

Yale University


This paper is another in a series of admirable studies carried on by Dr. Roberts of the early stages of development of Southwestern cultures. It presents the results of excavations at Kiatchlanna, eastern Arizona, 1929. The remains consisted of pit houses, jacal dwellings, and a pueblo structure, representing two separable occupations. The opening section gives a brief résumé of the periodic divisions of Southwestern prehistory and the historical background of the locality considered from the Spanish and Zuni angles. Then follows a liberal section devoted to the house remains, including a full description supplemented by plans and drawings, and a discussion of the chronological position of the types encountered. The analysis of the material culture and disposal of the dead follows, being well documented by text and illustrations.

The house remains are identified as follows:

1. The pit houses are of two types: one is small, roughly circular, with walls more or less vertical from the floor to ground level; the other is large, roughly circular or oval, and characterized by an encircling bench. Both types are dated to Pueblo I, although it is stated that the smaller pit houses were probably the older, inasmuch as they display traits common to late Basketmaker III dwellings.

2. Jacal structures (imperfectly preserved) were built on ground level, the walls consisting of poles covered with mud. In age they were contemporaneous with the large pit dwellings and in type resembled structures of Pueblo I times in southwestern Colorado.

3. A pueblo structure of 49 rooms and 4 kivas, unrelated to the preceding, placed in the early part of Pueblo III.

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3 E.g., a possible connection between Karankawa (a member of the proposed Coahuiltecan stock) and Atakapa (Swanton, *ibid.*, p. 39); a possible connection between the proposed Coahuiltecan stock and the Muskogean stock (*ibid.*, p. 40); a possible connection between Tunica, Chitimacha, and Atakapa, (Swanton, A Structural and Lexical Comparison of the Tunica, Chitimacha, and Atakapa Languages, Bureau of American Ethnology, Bulletin 68, 1919); a possible connection between these latter languages and the Muskogean stock (*ibid.*, p. 56); a possible connection between the proposed Coahuiltecan and Hokan stocks (Sapir, Central and North American Languages, The Encyclopaedia Britannica, 14th edition).
Dr Roberts’ description of the pit houses is one of the first adequate reports on dwellings of this type outside of the San Juan area, and the data are made doubly valuable by the excellent stratigraphic conditions which the site afforded. A hiatus in occupation occurring at about Pueblo II enabled the segregation of architectural and material traits into two well defined groups representing the pit house and pueblo horizons. The precision with which the excavations were conducted is best shown by the splendid results obtained in unravelling the growth history of the pueblo structure. Its construction in five progressive stages is clearly presented in a series of excellent diagrams. The student of pueblo architecture will find no better example of the increment method of growth than in the Kiatuthlanna pueblo.

The burials further demonstrate the two-period occupation of the village, for the occupants of the pit houses were both long-headed and broad-headed, deformed and undeformed, while the crania of the pueblo horizon were all occipitally flattened. The evident physical range of the pit house dwellers led Dr Roberts to assert that “there was an unquestionable mixing of stocks and customs.” One of the most critical points in Southwestern prehistory is touched upon here, for the mixing of types is accompanied by profound changes in the culture patterns. Although investigations generally have not been carried far enough to warrant a clear statement of the problems of infiltrating round-heads, recent work in southwestern New Mexico may contribute to the solution. Here, at a stage comparable to the pit house occupancy of Kiatuthlanna, there lived a brachycephalic, low vaulted, undeformed people who may represent in relative purity the round-heads found in Kiatuthlanna. Material traits, as stone bowls, notched awls, and long stone pipes, are expressly southern and definitive of this group, while a different ceramic and architectural association was encountered by Dr Roberts. Yet the items just mentioned do occur in Kiatuthlanna, strengthening the belief in the fusion of two cultures as indicated by the association of two physical types.

The region represented in this report is important archaeologically although it has received little attention heretofore. It lies between the center of the Basketmaker-Pueblo group to the north and that of the Hohokam to the south, and occupies an intermediate position between the northern San Juan center and southern New Mexico with whose culture it bears some relation, being the recipient of some elements and the donor of others. The work at Kiatuthlanna has contributed materially to the understanding of the interrelations of the northern and southern areas and, at the same time, has pointed out the wealth of information still to be gained from investigations in marginal areas.

Emil W. Haury

Gila Pueblo

Antecedentes y Relaciones de la Cultura Teotihuacana. Eduardo Noguera. (El Mexico Antiguo, Vol. 3, pp. 3-95, April, 1935.)

Mr Eduardo Noguera, the foremost stratigraphical expert in Mexico, has in this paper made a great contribution to our knowledge of the great pyramid city
of Teotihuacan. In undertaking the publication of this important study, the revived "El Mexico Antiguo" gives additional proof that the publication is carrying on the scholarly tradition set it by Professor Hermann Beyer, its founder.

Mr Noguera gives a careful analysis of the material found within the Pyramid of the Sun, the adobes of which had been constructed from the refuse of an earlier occupation. He presents a well-illustrated corpus of material of clay, stone, bone, and shell, and then proceeds to analyze the typological affiliations of the various groups of specimens. It is most valuable to have so carefully defined an unmixed epoch in the history of Teotihuacan.

Mr Noguera finds four strains involved: elements related to the Cuicuilco-Ticoman branch of the Archaic or Early cultures of the Valley of Mexico, a core basal to the "classical" Teotihuacan culture, a strong affiliation in pottery and figurine types to the "Tarascan" culture group of western Mexico, and finally a group of nondescript elements whose connections cannot yet be defined.

His discoveries bear out what seems to have been a general condition in the Valley of Mexico, namely the infiltration of successive tribes with different cultures. There does not seem to have been an unbroken cultural development absorbed by the incoming peoples. What relationships there may have been between the various cultures lay outside the Valley. The Aztec civilization certainly does not derive in situ from either the Mazapan or Coyotlatelco cultures. The diagnostic elements of these complexes certainly were not of Teotihuacan derivation, even if, in the case of Coyotlatelco, some of the various elements might have persisted. In the Pyramid of the Sun, the "archaic" strain, although allied to Ticoman and Cuicuilco, is different, as if emanating from some other local manifestation. Finally the Ticoman-Cuicuilco culture does not derive directly from that of Zacatenco-Copilco.

Thus the value of carefully presented studies of the material culture of various sites, becomes very evident in view of the episodic character of culture-history in the Valley of Mexico, and Mr Noguera is to be congratulated on his careful exposition of the Pyramid of the Sun or Teotihuacan I material. He should also receive great praise for his shrewd appraisal of the comparative data, which, owing to the vast amounts of unpublished material, can only be handled by a student of his experience and calibre.

American Museum of Natural History

George C. Vaillant

AFRICA


This superb volume is not a complete monograph. It excludes the author's linguistic material and most of his folk-tales, merely summarizes data on initiation more fully described in Baessler-Archiv (Vol. 15, 1932), and reserves a detailed treatment of art for the future. Dr Baumann, commissioned to collect specimens,
recognizes the limitations imposed thereby and also deserves commendation for his ungrudging appreciation of others, as in the generous reference to Hambly (p. 8).

A very clear picture emerges of northeast Angola, especially of the Vachokue (Baumann's Tšokwe, Pogge's Kioko). Only the political relations within the old Lunda kingdom remain confused because Baumann fails to coördinate the historical sources. There is fair material about the Luena and Lunda. The Vachokue have gradually spread over vast areas, forming appreciable enclaves in the Congo. Immigrating from the southeast with their closest kin, the Luena, the Vachokue reached the woodlands north of the Kasai, where they became subject to the Mwata Yamvo, i.e., the Lunda dynasty, or (as Baumann has it) of the Luba dynasty dominating the Lunda. Their mere headmen partly remained as district chiefs under governors related to the Mwata Yamvo (pp. 8, 10, 139–47). A reference (p. 8) to the overthrow of the kingdom by the subject people remains unexplained.

The importance of hunting is reflected in ritual, which is virtually absent from farming (p. 49). Manioc forms the main crop; maize appears peripherally, yams sporadically, with sorghum and pennisetum as the chief cereals. Today men help with manioc, but the care of other species still devolves wholly on women (pp. 49–53). Cattle are rare; dogs, goats, sheep, pigs, chickens, doves, and bees are kept, but sheep and goats disappear progressively toward the east. Beeswax is of great importance in trade (pp. 47–49).

The typical Vachokue dwelling is rectangular in plan, with a hip-roof and short ridge-pole, the walls being of either grass or clay. In a typical village the huts are ranged in a circle, matrilineal kinsmen occupying the settlement with their wives, little children, and sisters' sons. It contains bachelors' houses, a council house, menstrual huts, and granaries (pp. 13–30). Skin-dressing is undeveloped, though skin clouts were worn, while barkcloth figures ceremonially. Women affect wigs (pp. 31–41, 61 f.).

Blacksmiths formerly enjoyed high esteem and still practice circumcision with their trade. Wire-drawing is a special craft, as is the carving of ceremonial objects, while basketry, pottery, barkcloth beating, spinning, and weaving are general household industries. Wood-carving, illustrated by magnificent photographs, represents the acme of art. Spinning and weaving are unimportant masculine tasks. Only coiled pottery and coiled basketry are made by Vachokue women, men manufacturing all other clay ware and plait-work. On this point tribal differences are noted (pp. 61–84).

The puberty ritual involves circumcision, death and resurrection performances, and masks; in a subsequent initiation appears stilt-walking (pp. 98–121). Possession figures as the common cause of disease. Divination looms large, diviners ranking next to the chief; they shake the varied contents of special baskets (pp. 158–208). The Vachokue are matrilineal and practice cross-cousin marriage. Inheritance is nepotic, the mother's brother is dominant and his sisters' sons join him when they

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are about six years old. A new chief is the eldest son of the deceased chief’s sisters; for the royal throne an elector chooses from among the sisters’ sons. I cannot harmonize the statements about conjugal residence (p. 125). The Lunda have filial inheritance and succession. Totemism, conceivably present in the royal family, is otherwise lacking (p. 144). Baumann emphasizes the importance of the queen dowager, actual or potential, i.e., as a monarch’s sister.

According to Baumann, Angola forms part of the cultural province including the southern Congo, the Zambesi peoples, and Southern Rhodesia. He suggests connections with India and Indonesia, which must be judged by specialists (pp. 12, 223).

University of California

ROBERT H. LOWIE


This report, based on a year’s work in Tanganyika, summarizes the facts bearing on the native women’s acculturation. Mrs Thurnwald has supplemented her own observations with the experiences, published and unpublished, of officials, missionaries, and ethnographers. The result is an admirably compact and convincing picture. The author reveals her insight in expounding the interrelations of polygyny with the ratio of the sexes and with the rule of continence prior to weaning. She recognizes the problems resulting from compulsory monogamy, sanitary regulations, European educational and living standards. On all these matters her judgment is temperate and sane.

For the anthropologist the first half of the book with its terse summary of pertinent demographic and sociological facts under aboriginal conditions is the more significant. At last we get some accurate figures on the ratio of the sexes: in one district an actual ten-year census in Dr Maynard’s maternity home reveals a ten percent surplus of female births (596 per mil); and less controlled but relatively trustworthy official statistics from seven districts indicate a uniform preponderance of adult women (pp. 10, 25–27). Contrary to widespread impressions, experienced European physicians deny that delivery is easier for native women than for Caucasians (p. 56). Mrs Thurnwald adds her mite to the correction of the current idea of “bride-purchase” in Africa (p. 17 f.) and points out the important part played by women as preservers of tribal tradition, as magicians and doctors, and sporadically even as chiefs (pp. 72–79). One fact that must impress every reader is the variability of custom within this after all restricted area so largely populated by members of a single stock. The majority of the natives are patrilineal, but despite partial Islamization the Saramo remain matrilineal and stress avuncular privileges (p. 17). Certain tribes assign tillage to women, others favored joint labor in the fields even prior to recent developments (pp. 35, 40). The Jagga women milk and feed livestock (p. 36) in contradistinction to most of the aborigines. Sterility is in some tribes regarded as the husband’s fault (p. 44 f.).
Mrs Thurnwald's sound judgment appears in her appraisal of the avunculate (p. 69). Altogether her treatise is both scientifically satisfactory and full of human interest.

ROBERT H. LOWIE

University of California


This is a very useful handbook of general phonetics, by a famous Africanist and a well-trained phonetician of the school of Daniel Jones, in which special attention is given to a number of phonetic features that are important in the study of African languages and which are either omitted or treated in a brief and cursory manner in most books on phonetics. The chapter headings will give an idea of the ground covered: The Difficulties of Learning a new Language; Phonetic Alphabets and Phonetic Orthography; The Organs of Speech; Classification of Sounds: Vowels and Consonants; Phonemes; Vowels of African Languages; Less usual Vowels; Nasalized Vowels; Diphthongs; Consonants: Classification and Description (General); Plosive Consonants; Nasal Consonants; Lateral, Rolled, and Flapped Consonants; Fricative Consonants; Affricates; Semi-vowels; Implosive Consonants (sometimes called "glottalic clicks" and of which examples are found also in Maidu, of north-central California, as Dixon observed years ago, with recent corroboration by Uldall); Ejective Consonants (familiar to Americanists as "glottalized consonants"); Click Consonants; Sounds with more than one Articulation (of which the "kp" and "gb" consonants of West African languages are a peculiar class); Syllables; Stress: Length; Sounds in connected Speech; and Tone Languages and how to study them. A series of phonetic summaries adds materially to the practical usefulness of the volume, the languages covered being Ewe, Yoruba, Fante, Bambara and Malinke, Ganda, Zulu, Nuer, Dinka, and Kikuyu. There is also a brief bibliography and a somewhat more extensive special bibliography on tone languages (pp. 140–41).

The phonetic system used is based on the alphabet of the International Phonetic Association. Examples, diagrams, and tone exercises do much to familiarize the student with the subject matter, which must often seem exotic and even bizarre to the beginner. All in all, this is a truly valuable guide.

And yet it falls considerably short of all that it might be. It cannot be truly said that whoever has mastered the book has all that he needs to know to get a satisfactory phonemic basis for the study of any African language. For one thing the concept of the phoneme, as distinct from that of the phonetic sound or phonetic feature, is not sharply enough defined nor is it carried far enough. Thus, in the treatment of tone, which seems disappointing to the reviewer, the difference between tones (registers and inflections) as such and fundamental tonemes (essential tone phonemes) has not been clearly made. Doke's nine-register system of writing Zulu, for instance, is taken over bodily (p. 202) from his excellent work on "The
Phonetics of Zulu" but it is precisely in tonal respects that Doke is probably misleading to the linguist, for his nine registers do not seem to represent that number of tonemic entities but constitute a blend of distinct tonemes on the one hand and conditional variants of single tonemes on the other. Every tone language, no matter how simple in theory, has such conditional variants and it is doubtful if the tonemics of Zulu, described by Westermann and Ward as "extremely complicated," is in fact as complicated in a purely tonemic sense as Doke’s transcriptions would seem to indicate. Nor do the writers treat of the mora or unit of length, an important concept for the proper understanding of the syllabic structure and tonemics of some African languages. In Jabo (formerly described as Gweabo), for instance, true diphthongs constitute single moras but combinations of two short vowels, each of unitary quantitative value, do not count as diphthongs, though they may easily impress the hearer as such.

The reviewer was further disappointed to note the authors’ apparent unfamiliarity with a number of distinctive phonetic features of certain African languages. Such are the emphatic consonants and the heavy syllables, found, for example, in Jabo, and tonal "anacrusis."³ The bilabial click, credited by the authors to Bushman, has also been found by Boas in Chindao (Portuguese East Africa). One hopes that the indicated phonetic range of African languages will be more nearly complete in a second edition of this important contribution to African linguistics.

E. Sapir

YALE UNIVERSITY


This album of West African art includes thirty-two half-tone plates, chiefly of woodcarvings representing human forms. There are a few examples of work in metal, of pottery, and of incised or pyrographed gourd vessels. Altogether seventy objects are figured, not counting mats, etc., used for backgrounds. The selection has been made from British collections, public and private, and, with few exceptions, represents the West African province of art in the narrow sense: the coastal region and its more or less immediate hinterland. No geographical or, apparently, other order is followed in the arrangement of the plates, some of which contain within one frame objects from widely separated localities.

In one respect the album differs from most publications of the kind having a popular appeal. Mr Richard Carlisle has contributed over sixty pages of detailed description of the pieces presented, with an attempt to relate them to the cultural

¹ See E. Sapir, Notes on the Gweabo Language of Liberia (Language, Vol. 7, 1931, pp. 30–41); this paper, dealing with a tonemically complex language, is not listed in the tonal bibliography.
background of the peoples concerned and to justify doubtful attributions of provenience. Here there are inaccuracies which might have been avoided, and perhaps it is not inadvisable to refer to two instances of this. The provenience of a comb or hairpin "in the Philadelphia [i.e., University] Museum" is said, on stylistic grounds, to be "probably incorrect." The ascription of this object to the Bapindi is due to the late Emil Torday, the principal authority on the region. The treatment of the small head which surmounts it differs considerably from that seen in the Bakongo "herminettes" which Torday figures in "Les Bushongo" and to which Mr Carlile compares the comb in question. Again, an Ibo or Ijo mask in the American Museum of Natural History is said to have been reproduced in the Museum Journal as a "mask from New Ireland." This is not the case: the only mask of this type published in the Museum Journal is the one also referred to by Mr Carlile, which is figured in the issue of March, 1920, and for which a West African provenience was given.

"Arts of West Africa" is designed chiefly to awaken or encourage the interest in native art of educational workers in the British colonies in Africa. Short essays, one by G. A. Stevens, "On the Educational Significance of Indigenous African Art," and another by Gabriel Pippet, "Teaching Wood-carving at Achimota," are included in the text. Both authors have been engaged in teaching art at Achimota, Gold Coast, and their articles show that a beginning has been made in the praiseworthy effort which this publication is designed to promote. Sir William Rothenstein and Sir Michael Sadler contribute sympathetic generalizations, the former in a brief "Introduction" and the latter in a short essay on the "Significance and Vitality of African Art."

It is not clear why music should be expressly barred in the title, since other arts, e.g., literature and dancing, receive only incidental notice in a few paragraphs of the text and in one or two titles of the brief bibliography.

H. U. HALL

AMBLER, PENNSYLVANIA


This remarkable illustrated article has an excellent map of the sites as well as ten plates reproducing photographs by the author, which fully explain the text of about 10,000 words. The modesty of the publishers of this journal, who are civil servants in the Sudan, causes its productions to be less well known than they deserve.

Mr Dunbar's paper is the result of six years' observations of the rocks on the river Nile between Aswan and Wadi Halfa combined with visits to the famous prehistoric sites and a close study of prehistory. His deductions are largely based upon stratigraphy and the results of surveys by Messrs Sandford and Arkell for the University of Chicago. The present writer is sceptical of the early paleolithic origin of most of North African rock drawings in view of the fluctuations which have oc-
curred in the Nile flood levels and the rainfalls since the third millenium B.C. In dealing with Nilotic rock pictures the results of the physiographic researches made by Miss Caton Thompson at Kharga and the Fayoum must not be overlooked. The recent discovery of rock paintings at Jebel Oweinat by Count Almasy, who attributes them to the Ethiopian troglodytes of Herodotus, is also a factor in considering the period at which rock pictures west of the Nile were made and the total disappearance of any population from the sites. Mr Dunbar's paper should be read by all Egyptologists and archaeologists, as the reputed great antiquity of some of these rock drawings is now open to question for the reasons given by him.

A. E. ROBINSON

ST. ALBANS, HERTS, ENGLAND

OCEANIA AND ASIA


In an earlier bulletin Te Rangi Hiroa dealt with the ethnology of Tongareva. That atoll and the two dealt with here are typically Polynesian in culture, and their populations do not vary greatly in number: Tongareva 390, Manihiki 416, Rakahanga 327. Yet within the typical Polynesian culture-pattern exhibited by them all, there are significant variations. These are not due to differences in date of settlement, for all were probably settled about the middle of the fourteenth century A.D. by colonists all of whom probably came from Rarotonga. The variations in culture as between Tongareva, on the one hand, and Manihiki-Rakahanga on the other, are to be explained as due principally to Tongareva being settled by regular colonists who brought with them priests and genealogists and established marae and handed down myth and religion in the orthodox Polynesian manner, while Manihiki-Rakahanga was peopled by the descendants of a single pair, Toa and Tapairu, of good family certainly, but fugitives, lacking priests, genealogists, or indeed companions of any kind. Similar settlement occurred elsewhere in Polynesia, notably at the Chatham Islands, and its inevitable result was the loss of many features of religion and myth that depended on a highly-trained priesthood for preservation. So, also, with social organization:

The early use of the ariki title in Rakahanga shows that the institution was known to Toa and Tapairu and was introduced by them from their island of origin. In the later development of the dual ariki titles and the tribal titles, the names used indicate a local development and that the people were not guided in the formation of nomenclature by any memory of past tradition. The Rakahangans instituted new offices based on Polynesian principles, but had to coin new local terms (p. 43).

The special value of this bulletin lies in the author's discussion of this aspect of the culture of Manihiki-Rakahanga.

The value of the author's discussion of traditional and genealogical material depends upon the truth of his assumption that Polynesian genealogies and traditions
are to be regarded as historical sources. No one who has had contacts with Polynesian ethnography can regard them in any other light. The statement by one reviewer of the Tongareva bulletin, "His account of tradition and history is more interesting as an account of how the natives react to it than as historical sources, as Te Rangi Hiroa regards them," indicates merely how little contact the reviewer had with Polynesia. It is true that some earlier students in that field, for example Forand and Percy Smith, handled traditional material uncritically; but that class of material remains one of the most important that the student in Polynesia has to handle. Te Rangi Hiroa's qualifications for handling it are unique.

The treatment of the scanty material culture of the two atolls is excellent. The old type of dwelling-house is described in detail, its closest affiliations being with Rarotonga and Samoa. The four-legged seat (nohoanga) is like that of the southern Cooks but more crudely made. Wooden food-bowls are also similar but are legless. The only legged example figured (fig. 19c) must surely be wrongly localized: the decoration admittedly follows the Mangaian or Austral pattern. The four-legged coconunt-grader is unknown. The wooden food-pounder is long, with narrow neck and flat terminal knob; stone and coral pounders are entirely absent. The rather scanty plaitwork forms are fully described, illustrated with the author's beautiful diagrammatic drawings which are the envy of all students. The only pendant, doubtfully figured by the author, following Edge-Partington, is in reality a Micronesian piece, though Kennedy has shown the type to be present also in the Ellice group. The two strings of shell beads on this example show that it must come from farther north than the Ellice Islands. No satisfactory example of the shell adze is figured though these must originally have been plentiful. The "mesial peg" in the haft seems a purely local feature. The stone adze (fig. 52) is of the greatest interest, and its place of origin will ultimately be traced. Seven pages are given to the ancient five-piece canoe, the description being based solely on models. It is to be regretted that the complete canoe in the Dominion Museum, Wellington, N.Z., was not utilized; this section is nevertheless good. In discussing a recent-looking paddle of unusual shape (plate 9, no. 5), the author states that it is "wrongly attributed to Manihiki by its donor." A recent-looking paddle, apparently by the same hand, was collected on Manihiki some fifteen years ago and is now in the Otago University Museum. The location is thus shown to be correct, though it is permissible to doubt the antiquity of the type. The section dealing with the ruvet tus fishhook is second only to Kennedy's account of that type of hook in the Ellice Islands, while the treatment of the bonito hook stands first of all accounts of this best-known feature of Polynesian material culture. Te Rangi Hiroa had the advantage of first-class work by Kennedy and Nordhoff to build on, but in draughtsmanship no previous worker is in the same class as he, and eighty-nine drawings illustrate the special characteristics, the range of variation, and the affiliations of the bonito hook of Rakahanga.

Perhaps the most interesting of the remaining sections is the one dealing with drama.

Tongareva, Rakahanga, and Manihiki can now be allotted their true places in the cultural map of Polynesia.

H. D. Skinner

University of Otago

Archaeology of the Pacific Equatorial Islands. Kenneth P. Emory. (Bulletin, Bernice P. Bishop Museum, 123. 43 pp., 22 figs., 5 pls. Honolulu: Bishop Museum, 1934.)

A student, whose extensive experience and comparative studies in the field of Polynesian archaeology particularly equip and qualify him to take up the task, reviews in this publication historical and archaeological data from all possible sources, including personally collected matter, relative to the early human occupation of Howland, Fanning, Washington, Christmas, and Malden Islands. These low coral islands of equatorial Polynesia, historically uninhabited by other than various impermanent groups of castaways, contract laborers, and Europeans, have long been reported as exhibiting stone structures and other manifestations of earlier inhabitants. However, accurate reports on these features, previous to this, have not been made available.

The author examines earlier reports in the light of recent investigations, carefully extricates fact from fiction, supplies a considerable mass of newly observed details, and employs all these data in comparing the early inhabitants of these islands with other Polynesian ethnic groups for the purpose of weighing possibilities of cultural relationship. The results are most interesting. The much advertised Tongan langi on Malden Island turns out to be a typical Raivavae marae which, with from thirty-five to forty other smaller maraes and certain additional matters of cultural import, serves to support the conclusion that a close cultural relationship existed between former inhabitants of Malden and the early Polynesians of Raivavae, Austral Islands, 1,280 miles distant. On the other hand, manifestations at Fanning Island, including a stone-walled enclosure and fishbooks, are strongly suggestive of Tongan influence. Howland, Washington, and Christmas Islands bear evidence of former inhabitants exhibiting mixed or indeterminate cultural peculiarities.

This work reflects a critical treatment of facts that demonstrates a commendable penchant for detailed accuracy, and the conclusions, offered with reasonable caution, adhere closely to the evidence tendered in their support.

W. C. McKern

Milwaukee Public Museum


In 1934 the Institut d’Ethnologie of the University of Paris and the Muséum National d’Histoire Naturelle organized a Franco-Belgian expedition to Easter
Island, Messrs Watelin, Métraux, and Lavachery constituting the staff. After M Watelin’s death en route Dr Métraux assumed charge: during the sojourn in Easter Island (July to December) he devoted himself to ethnography while archaeological research devolved on M Lavachery.

In the present pamphlet of about twenty-five unnumbered pages, Dr Métraux, apropos of a special exhibition of the specimens collected for the Trocadéro Museum, sketches the culture of Easter Island and the results of this latest investigation, which are refreshingly free of sensationalism. He is of opinion that the natives are closely related to the Maori and came from the Gambier Islands about the twelfth or thirteenth century, the dearth of timber and presence of various soft rocks giving a special tinge to their adaptations. As for the transportation of the statues, he points out that the average weight of those placed upright hardly exceeds five to six tons so that the joint labor of four hundred men harnessed to one figure would suffice to drag it over the required distance.

Il n’était besoin que d’un traineau de bois à patins qu’une bonne équipe faisait glisser sur des pierres lubrifiées avec des patates douces pour transporter une statue de la carrière à l’ahu . . . .

Concerning the tablets, Métraux independently supports Mrs Routledge’s inference that the “script” constitutes a mnemonic system and favors its local origin.

R. H. LOWIE

UNIVERSITY OF CALIFORNIA

Archaeology of the Marianas Islands. LAURA MAUD THOMPSON. (Bulletin, Bernice P. Bishop Museum, 100. 82 pp., 23 figs., 11 pls. Honolulu: Bishop Museum, 1932.)

Geologically the Marianas form a link between Japan and the Carolines; they are therefore of special interest anthropologically as a possible cultural link between ancient Japan and Micronesia and Polynesia. An alternative possible cultural relationship of the Marianas, and one that would seem on general grounds more probable, would be with the Philippines, but comparison with that area is rendered impossible by the refusal of archaeologists there to publish the results of important excavations, begun in Luzon more than a decade ago.

Bulletin 100 is written by Laura Maud Thompson, and is based on field work carried out by Hans G. Hornbostel and his wife. Students in Polynesia, from whose angle the present review is written, will sympathize with the Hornbostels in their difficulties, which are present almost everywhere in the Pacific, making it, at any rate up to the present, impossible to put into operation the refinements of method habitually used by archaeologists in Europe and America. But exception must be taken to the omission of scales from the plans in Figs. 3 and 5, while Fig. 6 has a scale expressed in feet, an obvious mistake for inches.

Rough stone structures in Guam usually consist of two parallel rows of uprights, the long axes of the rows being parallel with the seashore or the river beside which
they are situated, or with the line of the ridge if inland. In horizontal cross-section
the upright is usually rectangular. It supports a massive hemispherical cap, the
total height above ground varying from about four feet to about seven feet six
inches. Most of the sites examined had four uprights in each row, making four pairs,
a minority had five pairs, and two sites had six pairs. Between the pairs of uprights
extended burials were found, each member of the pair thus constituting either a
head or footstone. The burials represented secondary interments. Of seventeen
burials investigated at Epitu,

skulls were missing from 4 skeletons, of which the arms and legs were separated from the
bodies of 2. The leg bones from the knee down were missing from 2 skeletons. Five skeletons
were marked by fire. Some of these skeletons were found lying in fire holes.

Burials were found outside the paired alignments. Those within the alignments
had feet towards sea or river in all cases.

Over most of the skeletons, at the head, chest, or knees, were scattered miscellaneous bones
(especially jaw bones), potsherds, slingstones, shell scrapers (whole or broken), stone imple-
ments (whole or broken), and fish-hooks.

The ground plans of monuments on Tinian and Saipan are stated to be similar to
those of Guam but are more elaborate, while on Tinian “capped monuments are
16 feet high.” Such structures and methods of burial are absent from Polynesia and
have not yet been recorded from Micronesia. The scanty Japanese literature avail-
able here does not yield information of similar structures or burials in Japan. Cave
drawings are recorded and among those figured are a number of headless human
figures, an ideograph widely represented in Polynesia. Large numbers of fragments
of coarse pottery are found. Three types of stone adze are present, as contrasted
with nine or ten types found in Polynesia; all three Marianas types are present in
Polynesia, and the commonest of them appears commonly in Japan. Among the
forty-six axes was one double axe. This is interesting in view of the occurrence of
the double axe in Malaya and in Bougainville. No double axe is yet recorded in
Polynesia. Fish hooks are rare: no complete one is figured. Thirty-three gorges
(misspelt as gouge at page 77) are listed, the two halves being set at right angles, as
in the Santa Cruz group and so, presumably, in the Carolines. The forty-four sinkers
are of four types, all present among the eight or nine types of Polynesia.

The authoress has placed students in her debt by an excellent piece of work.
H. D. Skinner

University of Otago

(xxi, 345 pp., 147 figs., 32 pls., map. $6.50. New York: Macmillan Co., 1934.)

This book is not a formal exposition of the prehistoric archaeology of China but
rather a popular account of the author’s multifarious activities in the once Flowery
Kingdom and as such represents seventeen years of field work and study devoted to
geology and paleontology as well as to archaeology. For a more complete account
the professional student should consult the detailed reports by Dr Andersson and
others in the Bulletins, Memoirs, and Paleontologica Sinica, published by the
National Geological Survey of China. Most of the archaeological papers have in
fact been summarized by the reviewer in the American Anthropologist (Vol. 29,
1927, pp. 177-200).

The author begins the archaeological portion of his present record with an ac-
count of his own part in the discovery of the now famous Choukoutien cave near
Peiping. He next summarizes the Paleolithic discoveries by Fathers Licent and
Teilhard in the loess of the Ordos region, within the big bend of the Yellow River.
Lastly, he takes up his own systematic explorations, which, commencing in 1919,
occupied about seven years and ranged all the way across northern China from Lake
Kokonor in Tibet to southern Manchuria near the Pacific shore, a distance of over
1200 miles. Within this zone were found some forty archaeological sites—ceme-
teries, abandoned villages, and cave deposits—all belonging to the Late Neolithic
horizon and by the author called the Yang Shao culture. This culture is uniformly
characterized by implements and ornaments of chipped and ground stone, shell,
bone, a few items of copper, and great quantities of excellent pottery. A consider-
able number of these sites were wholly or partly excavated and the interesting story
here told is made up of incidents connected with this work, as well as of summary
descriptions of the data obtained. The presentation ends with some interesting spec-
ulations on the significance of certain decorative patterns appearing on the pottery,
the central notion being that they are symbols of life or immortality.

Summing up, Dr Andersson dwells on the fragmentary character of the archae-
ological results obtained thus far in China. The earliest remains from the Choukouti-
tien cave are regarded as Early Pleistocene and represent man of this date as already
in possession of implements and the use of fire. The Ordos loess discoveries are con-
sidered as of Middle Pleistocene date and appear to belong to the Mousterian-Aurignac-
nian horizon of Europe. The Yang Shao culture occurs superficially and lies,
technologically speaking, on the border line between the Neolithic and Bronze
ages and is thought to date from about 3000 B.C. The early historic Chinese culture,
finally, dates from about 1500 B.C. These four stages, barring some faintly indi-
cated connections between the last two, are separated by great gaps which only
protracted field work can close. Incidentally, the author now takes the view that
certain of the Yang Shao traits, such as rice culture, the rectangular perforated
knife, and the tripod vessels—all of which survive in modified form in Chinese
culture—are derived from the south, while he still holds that the elaborately painted
pottery is of western origin and related to the ceramics of Anau and Tripolji.

The volume is enriched by numerous interesting illustrations, both photographic
and hand drawn, among the latter being several pen-and-ink sketches of well-
known students now or recently active in this new and promising field.

N. C. Nelson

American Museum of Natural History
MISCELLANEOUS

Christoph Meiners und die Völkerkunde. ALEXANDER IHLE. (152 pp. RM 8.50. Göttingen: Vandenhoeck and Ruprecht, 1931.)

The last half of the eighteenth century was a period of remarkable development in the sciences and social disciplines.

The contributions in physical anthropology, of that period, are much better known in present day literature than the contributions in ethnology and cultural anthropology. Christoph Meiners lived and wrote during that period and the early years of the nineteenth century. His first work was published in 1775, his last, a three volume work, in 1815. Of the one hundred and five titles credited to him it is not possible to give even an adequate sampling.

He covered almost the entire gamut of anthropological problems, including ethnographic ones. One is amazed at the compass of his learning and at his frequently penetrating insight.

Dr Ihle has set forth the development of Meiners' thought with attention to the latter's predecessors and contemporaries.

It is a delightful and illuminating volume which certainly will rank among the most important contributions to the history of anthropology.

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Negro Intelligence and Selective Migration. OTTO KLINEBERG. (xii, 66 pp. $1.25. New York: Columbia University Press, 1935.)

For some years it has been claimed that the migration of the negro to the North is due to his superior intelligence over that of the negroes who refuse to migrate and remain in the South. This asserted selective migration has been given as a reason for the observed measures of superiority in intelligence test performance between the northern and southern negro. Whether or not this selection is real has been made the subject of a very thorough investigation by Klineberg. He comes to the conclusion that there is no ground for believing that the superior negro is selected for migration, but that residence in the North, where he finds a superior and favorable environment, is the cause of the measured superiority. Space limits a detailed account of this important study, but we may briefly describe it here.

In the first place, upon examining the school record of migrant negro children, made in the South before they migrated, it is evident by these results that they are no better than the average negro children in the southern schools from which they came. In the next place, a rather elaborate testing program indicates that the performance of the negro migrant improves with his residence in the more favorable environment.

The investigator compared the test performance of southern born migrant negroes with that of northern born negroes in New York City schools. He found the latter generally superior to the former in practically all counts for verbal tests (group tests, the individual Binet tests) but not in performance tests, which are
supposed to get around superiority due to linguistic ability. However, these last results are not to be taken as very conclusive.

A few items are interesting. In the case of the scores obtained by use of the National Intelligence Tests the improvement of score with length of residence in New York City is clear and definite. For the Binet the results indicate an improvement in I.Q. with length of residence, i.e., many give an I.Q. of 81.4 for less than one year of residence to an I.Q. of 87 for more than four years of residence, which is about the same as that of New York. The findings with performance tests (two of Pintner-Patterson series, and the Minnesota Paper Test) do not indicate any significant environmental effect, and besides are not to be taken as conclusive. On the whole the investigator believes that as far as the results of his experiment go they show that (1) the performance of the northern negro is superior to that of the southern negro; (2) that this is not due to selective migration, (3) but to an improved superior environment. In the mind of your reviewer, an examination of the data and its handling certainly justifies the conclusions.

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UNIVERSITY OF DENVER
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Renauld, E. B. Arrowhead Types of Colorado (Southwestern Lore 1, No. 1: 4–6, Gunnison, Colorado, June, 1935).
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Boxler, K. Indianer am Putumayo (301 pp. 3.70 RM. Freiberg, 1934).


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Humbert-Sauvageot, M. Quelques aspects de la vie et de la musique dahoméennes (Zeitschrift für vergleichende Musikwissenschaft 2, No. 4, n.d.).

Leiris, Michel. Rhombes dogon et dogon pignari (Bulletin, Musée d'Ethnographie du Trocadéro 7: 3-10, Jan.-June, 1934). [French Sudan.]


Stubbs, J. N. Beliefs and Customs of the Maleval Dinka (Sudan Notes and Records 17, No. 2, 1934).


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Lieberenz, P. Im Lande der Renntiere (154 pp. 6 RM. Berlin, 1933).


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Prehistory and Physical Anthropology


Miscellaneous


DISCUSSION AND CORRESPONDENCE

HISTORY AND SCIENCE IN ANTHROPOLOGY: A REPLY

It was interesting to me to read Dr Kroeber’s analysis not only of my scientific work but also of my personality. I may perhaps misinterpret both. Nevertheless I wish to express my complete disagreement with his interpretation. It is quite true that as a young man I devoted my time to the study of physics and geography. In 1887 I tried to define my position in regard to these subjects, giving expression to my consciousness of the diversity of their fundamental viewpoints. I aligned myself clearly with those who are motivated by the affective appeal of a phenomenon that impresses us as a unit, although its elements may be irreducible to a common cause. In other words the problem that attracted me primarily was the intelligent understanding of a complex phenomenon. When from geography my interest was directed to ethnology, the same interest prevailed. To understand a phenomenon we have to know not only what it is, but also how it came into being. Our problem is historical. Dr Kroeber suggests as

the distinctive feature of the historical approach, in any field, not the dealing with time sequences, though that almost inevitably crops out when historical impulses are genuine and strong; but an endeavor at descriptive integration. . . . Process in history is a nexus among phenomena treated as phenomena, not a thing to be sought out and extracted from phenomena.

I confess that to me this does not give any sense. We have descriptions of culture more or less adequately understood. These are valuable material. They yield, if well done, most illuminating material in regard to the working of the culture, by which I mean the life of the individual as controlled by culture and the effect of the individual upon culture. But they are not history. For historical interpretation the descriptive material has to be handled in other ways. For this work archaeological, biological, linguistic, and ethnographic comparisons furnish more or less adequate leads.

If Dr Kroeber calls my first piece of ethnological work, “The Central Eskimo,” (written in 1885), historical, I fail to understand him. It is a description based on intimate knowledge of the daily life of the people, with bad gaps, due to my ignorance of problems. The only historical points made are based on a comparison of the tribe studied with other Eskimo tribes and with the Indians of the Mackenzie basin, on a careful study of evidences of earlier habitations of the Eskimo, and a guess as to the course of their early migrations. The rest is description pure and simple. If in later writings I did not stress geographical conditions the reason must be sought in an exaggerated belief in the importance of geographical determinants with which I started on my expedition in 1883–84 and the thorough disillusionment in regard to their significance as creative elements in cultural life. I shall

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2 The Study of Geography (Science, Vol. 9, pp. 137–41, 1887).
always continue to consider them as relevant in limiting and modifying existing cultures, but it so happened that in my later field work this question has never come to the fore as particularly enlightening.

May I remind Dr Kroeber of one little incident that illustrates my interest in the sociological or psychological interpretation of cultures, an aspect that is nowadays called by the new term functionalism. I had asked him to collect Arapaho traditions without regard to the "true" forms of ancient tales and customs, the discovery of which dominated, at that time, the ideas of many ethnologists. The result was a collection of stories some of which were extremely gross. This excited the wrath of Alice C. Fletcher who wanted to know only the ideal Indian, and hated what she called the "stable boy" manners of an inferior social group. Since she tried to discredit Dr Kroeber's work on this basis I wrote a little article on "The Ethnological Significance of Esoteric Doctrines" in which I tried to show the "functional" interrelation between exoteric and esoteric knowledge, and emphasized the necessity of knowing the habits of thought of the common people as expressed in story telling. Similar considerations regarding the inner structural relations between various cultural phenomena are contained in a contribution on the secret societies of the Kwakiutl in the Anniversary Volume for Adolf Bastian (1896) and from another angle in a discussion of the same subject in the reports on the Fourteenth Congress of Americanists, 1904 (published 1906); the latter more from the angle of the establishment of a pattern of cultural behavior. These I should call contributions to cultural history dealing with the ways in which the whole of an indigenous culture in its setting among neighboring cultures builds up its own fabric.

In an attempt to follow the history of a culture back into earlier times we are confined to indirect evidence and it is our duty to use it with greatest circumspection. Dr Kroeber accuses me of not being interested in these questions. I do not know, then, why I should have used years of my life in trying to unravel the historical development of social organization, secret societies, the spread of art forms, of folktales on the Northwest Coast of America. I think that such a detailed study is worth while not only for its own sake but because it illuminates also general aspects of the history of mankind, for here we see the totality of cultural phenomena reflected in the individual culture. Is it that painstaking work of this kind does not seem to Dr Kroeber worth while, but that it requires the flight of an unbridled imagination to have his approval? I cannot understand in any other way his praise of a public lecture which I gave as President of the New York Academy of Sciences on "The History of the American Race," guarding my statement however, at the very beginning by saying that I should give my fancy freer rein than I ordinarily permit myself. When as early as 1895 I made a careful analysis of the then available material, showing the relations of Northwest Coast mythologies among them-

3 Science, n.s., Vol. 16, pp. 872-74, 1902.
selves and to other American and Old World areas, the object was to demonstrate historical relations. Perhaps I did not go far enough for Dr Kroeber in establishing the center of origin of each element; but there I balk, because I believe this can be done in exceptional cases only. The fact that a phenomenon has its highest development at a certain point does not prove that it had its origin there. The belief in this, which I consider an unjustified assumption, and a more lighthearted weighing of evidence differentiates our methods. In a conversation Dr Kroeber admitted that I wanted a high degree of probability for a conclusion, while he was satisfied with much less. That is an Epicurean position, not that of a modern scientist.

I am sorry that I cannot acknowledge as fair the summary of my work. It is true that I have done little archaeological work myself. My own only contribution was the establishment of the sequence of archaic, Teotihuacan type and Aztec in Mexico, I believe except Dall's work on the Aleutian Islands, the first stratigraphic work in North America; but in the plan of the Jesup Expedition I assigned an important part to archaeological work which in the careful hands of Harlan I. Smith gave important results on Fraser River showing the invasion of inland culture. If farther north it did not give any results the cause was not lack of interest but failure to find significant material. I may also claim to have kept before our scientific public year after year the necessity of careful archaeological work in northern Alaska, which has unfortunately been deviated from its main object by sensational artistic finds, although the main problem remains that of the occurrence or non-occurrence of pre-Eskimo types in the Bering Sea region.

In regard to linguistic work Dr Kroeber's criticism does not seem to me to hit the mark at all. Relationship of languages is a powerful means of historical research. It remains equally valid, whether we assume purely genetic relationship or whether we ask ourselves whether by contact languages may exert far reaching mutual influences. This question is important for the interpretation of relationships but has absolutely nothing to do with a historic or non-historic approach. If it can be settled we shall know how to interpret historically the linguistic data. That I am here as elsewhere opposed to ill substantiated guesses, goes without saying, but has nothing to do with the case. Here also a 40% possibility is no satisfactory proof for me.

Dr Kroeber's strictures on my book on "Primitive Art" are entirely unintelligible to me. He says style has not been treated. There is a whole chapter on style and one specific one on Northwest Coast style intended as a sample of treatment of the problem. Maybe Dr Kroeber has an idea of his own of what style is, as he has an idea of his own of what history is. He reproaches me for not having written on the history of Northwest Coast style. Unfortunately there are no data that throw any light on its development. It appears in full bloom and disappears under the onslaught of white contact. The slight local differences and the relation between the arts of the Eskimo and other neighboring tribes do not seem to me to throw any light on the subject. Does he want me to write its history without such data? Am I to repeat the wild guesses of Schurz?

I have never made the statement that history is legitimate and proper, but his-
torical reconstruction unsound and sterile. As a matter of fact, all the history of primitive people that any ethnologist has ever developed is reconstruction and cannot be anything else. There is, however, a difference between cautious reconstruction based on ascertained data and sweeping generalizations that must remain more or less fanciful. I do recognize quite a number of very fundamental general historical problems in regard to which I have more or less decided opinions, such as the distribution and relationships of races, the relation of America to the Old World, that of Africa to Asia, and so on. It depends entirely upon the evidence how strongly I hold to these opinions. It has happened to me too often that a suggestion cautiously made has been repeated by others as though I had pronounced it as a set dogma.

Now as to the use of statistics in ethnology as a tool of research. Being somewhat familiar with the difficulties of statistical work I do not believe that it is a safe guide in ethnological inquiry. I believe I was the first after Tylor's discussion of 1888* to try it on the field of mythology, and if at that time the correlation method had been as much abused as it is now, and since I had not yet understood its dangers, I might have established some nice coefficients of correlation for elements of mythology. The data of ethnology are not of such character that they can be expressed by mathematical formulas so that results are obtained which are in any way more convincing than those secured by simpler ways of numerical comparison. Behind these always loom the unanswered questions in how far the materials enumerated are really comparable, or in other types of problems, like Tylor's, in how far they are independent.

I regret that Dr Kroeber also does not see the aim I have in mind in physical anthropology. We talk all the time glibly of races and nobody can give us a definite answer to the question what constitutes a race. The first stimulus to my active participation in work in physical anthropology was due to G. Stanley Hall and to the atmosphere of Clark University, and had little to do with racial questions, rather with the influences of environment upon growth. When I turned to the consideration of racial problems I was shocked by the formalism of the work. Nobody had tried to answer the questions why certain measurements were taken, why they were considered significant, whether they were subject to outer influences; and my interest has since remained centered on these problems which must be solved before the data of physical anthropology can be used for the elucidation of historical problems. Equally important seems to me the question in how far the functioning of the body is dependent upon bodily structure. The answer to this problem is the necessary basis for any intelligent discussion of racial physiology and psychology.

Dr Kroeber refers to the discussion on anthropological methods at the time of the Americanist Congress held in New York in 1928. He does not quite completely

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7 Indianische Sagen, pp. 341 et seq.
tell the story of this incident. The discussion had centered entirely around Kulturkreise and other attempts at historical reconstruction. Finally I said that I had all through my life tried to understand the culture I was studying as the result of historical growth, but since the whole discussion had been devoted to historic sequences I had to arise as the advocatus diaboli and defend those who sought to understand the processes by which historical changes came about, knowledge of which is needed to give a deeper meaning to the picture. This was no new position of mine, as I think has become sufficiently clear from the preceding. It is true enough that in general the participants in the discussion did not want to have anything to do with the investigation of “processes” which seemed anathema but preferred to stick to their pet theories which they considered satisfactorily proven.

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MAMMOTH OR “STIFF-LEGGED BEAR”

Dr W. D. Strong gives a portion of a Naskapi tale about Djàkabish including the adventure of his slaying the monster Kàtcheetohúskw who had killed and eaten his parents. Dr Strong considers the monster (owing to his large ears, etc.) as reminiscent of the mammoth. Prof F. Speck gives a Mistassini version of the same tale. The name of the hero is T’saka’bec and that of the monster is Katci’tò’wack’. According to Speck, among both the Naskapi and Montagnais the animal is referable to the Ursidae. He further notes that Katci’to’wack’ is translatable as “Stiff-legged Bear,” and cites pertinent words in support of this etymology.

Both of these authors seem to have overlooked the fact that Skinner, had previously recorded versions from Rupert’s House (Tcikápis, Katci’tos) and the Albany River; in the last variant note “the bears who killed our parents” — which lends support to Speck’s contention. But Skinner as well as Strong and Speck seems not to have noted that in Le Jeune’s Relation of 1637 a very old variant occurs: the hero is Tchakabech, who is “a little Dwarf:” a bear devours his father but the “great Hare” (“Michtabouchiou”) devours his mother, and hair is found in its belly.

In this connection it may be pointed out that in an Ojibwa version concerning the same hero Tcakápis (“The Gnome”) given by the late Dr Jones, “Bears-with-Heads-at-Both-Ends” (ÁT’dawa’kwag) are “the names of those that slew our parents.”

— Jesuit Relations, ed. Thwaites, Vol. 12, p. 31 et seq.
— See also Ellen Russell Emerson, Indian Myths (Boston, 1884), p. 371, which work contains some valuable notes besides much trash.
Dr John Cooper tells me he has collected a version at Rupert’s House. There Kâtcîtôwî’sk (the devouring monster) is rendered “the one who murdered.” I regret I cannot confirm this etymology by analysis. Dr Cooper has also collected a variant at Moose Factory with Kâ’gitôwâ’skô as the monster slain by Tcakâ’bic; the informant was quite sure it was some sort of a bear.

This brings us face to face with Speck’s etymology. Speck’s tci’towa’o, “he is stiff,” is backed by Natick ch’étou, “it is stiff,” and Cree tcitawisiw (restored from Watkins’ chetow-isew), “is stiff.” However the Moose Factory variant shows that the Montagnais-Naskapi variants with medial _tc (ch)_ have this in accordance with the well-known law that a palatal stop consonant appears as _tc (ch)_ before original _e_ and _i_ vowels in Montagnais-Naskapi. That is to say, that though the Mistassini associate Katci-to’wask‘ with tci’towa’o, this derivation is a purely popular and fanciful one, devoid of scientific foundation. The Moose Factory variant suggests the translation “growling bear” but very accurate recording would be needed to establish this. And then the variant may have been associated with the stem “to growl, neigh, bellow” (kito—; also in Menomini; used even of bears) and have been transformed to mean something else. I am inclined to be rather sceptical as to whether a real etymology can be given. Whether or not the terminal portion originally meant “bear,” certainly it was felt to mean such in more than one locality.

Note, too, a giant bear occurs in Seneca and Ottawa mythology.

I wish here to state that I agree with Speck that the monster in question is to be regarded as a purely mythical animal, and not reminiscent of the mammoth. With as great or as little plausibility we might consider the Giant Wolf in which the Cree believe as reminiscent of Pleistocene fauna. Similarly the Giant Beaver of the Montagnais, Cree, and Ottawa. Since Dr Strong is interested in possible reflexes of Pleistocene fauna in Indian mythology I am a little surprised that he makes no reference to the tale of Bosh-kwa-dosh or the Mastodon. Personally I see no reason to regard the animal as anything more than mythical. In the same way we are not to interpret Tchakabech (Tcikápis, Tcakâpâs, etc.), “Little Dwarf,” “Gnome,” as anything but fanciful.

Postscript, November, 1935.—Field-work, made possible by a grant-in-aid from the American Council of Learned Societies, among some of the Indians (and Eskimos) of James and Hudson’s Bays this summer and early fall makes it possible to add some details. At Moose Factory two informants state that it was [a] great bear[s] (misimaskwa in simplified transcription) that slew Tcakhâpê’s parents. At Rupert’s House one informant says it was Kâtcîtôska that slew Tcakhâpec’s parents; another gives the variant Kâtcitowask’a; neither knows what animal is intended. The second informant gives another variant, mistamit, translated at Moose Factory as “Big Horse” which clearly is reflection of ordinary Cree, though slightly incorrect; “Big Dog” would be very literal; “Big Quadruped” is probably intended. At Fort George Kâtcîtôska reoccurs; the animal is unknown; my interpreter pronounced Tcakhâpec; from the syllabic script I should suppose Tcikhâpâc to be correct. At the Great Whale River the variant Kâtcatausk’a occurs; it is translated “Because he was strong” which can not be

confirmed by a linguistic analysis; Tchhkappc probably is the correct restoration of a syllabic text. No details were mentioned as regards the nose. In answer to a query regarding this at the Great Whale River I was told that the head was like that of a bear. Then the statement that the ears were like those of a bear followed voluntarily.

BUREAU OF AMERICAN ETHNOLOGY

MISS OWEN’S “FOLK-LORE OF THE MUSQUAKIE INDIANS”

When I reviewed Miss M. A. Owen’s “Folk-Lore of the Musquakie Indians of North America” in Current Anthropological Literature,² I pointed out some of the more obvious errors contained therein; but as there was much that I could neither confirm nor refute I especially noted this fact. I also said that the myths were valuable (for they at least partly occur elsewhere). Had my knowledge of non-Algonquian American Indian languages been sufficient at the time I should not have been so lenient. For I wondered if some of the various Indian words quoted, though clearly not ordinary Algonquian, might not be ceremonial. Not being able to prove or disprove this, I refrained from all discussion. Later on my knowledge of the Fox (Musquakie) Indians increased to an extent that I felt justified in saying “The ethnological data are untrustworthy.”¹ I should have much preferred to have the matter rest there. Unfortunately Pater W. Schmidt in his “Der Ursprung der Gottesidee” (Vol. 5) has incorporated (in German translations) large extracts from Miss Owen’s work. In justice to the learned author it must be said that he had noted my criticisms; but as long as these were not directed against the portions on religion, he felt justified in making the extracts. His acute analysis enabled him to see that Miss Owen’s data were decidedly reminiscent of Siouan, or rather Algonquianized Siouan tribes (p. 662). Why, we shall see presently.

Owing to the prestige Dr Schmidt enjoys I feel that it is incumbent upon me to show why Miss Owen’s data “are untrustworthy.” Scattered in Miss Owen’s book are Indian words glossing English ones and vice versa. Some of these Indian words are Algonquian: almost all are identifiable as Musquakie = Fox (these all could also be Sauk or Kickapoo; nearly all also Shawnee; a few are Pan-Algonquian); some are definitely not Sauk, Fox, nor Kickapoo specifically and Central Algonquian in general. But some of the words are Siouan. An Algonquian word may be even on the same line as a Siouan one. The Siouan words have been identified not only by the published material but also the unpublished material in the vaults of the Bureau of American Ethnology. In some cases it is not possible to know the precise Siouan language: this is partly due to the inadequate orthography employed, but it is partly due to the fact that some words are apparently Pan-Siouan. However, it is sufficient clear that the bulk are either Winnebago or Iowa. As Winnebago and Iowa are rather closely related, it is difficult, at least for me, to be absolutely posi-

¹ Printed by permission of the Smithsonian Institution.
² Volume 2, 1913, pp. 233-37.
tive as to which is the source in any given instance. Yet I feel reasonably sure that Iowa is the source in some cases, and Winnebago probably in one case. However, there are several Winnebagoes on the Musquakie "reservation" (for it is not a legal one) near Tama, Iowa. And the Iowa reservation is close to that of the Missouri Sac and Fox on the Kansas-Nebraska boundary, on which there are some Fox (Mus-
quakie). Moreover the Iowa reservation in Oklahoma is close to the Mississippi Sac and Fox one, though this fact is probably not pertinent. As all the vocabularies do not contain exactly the same English words this accounts for the fact that I have had at times to go beyond Winnebago and Iowa in citing Siouan. I do not pretend to list every Siouan word in Miss Owen’s book. That a Siouan specialist may do in the future: I merely state here that there are also some (supposed) Indian words which I can not identify.

Here then are some Siouan words: (p. 1) He-nau-ee (mother: Iowa; cognate in Winnebago, etc.); (p. 8) Hee-to-gwaw (grandfather: Iowa; cognates elsewhere); Sclar (eagle: Iowa; cognate in Winnebago); Moocha (bear: Iowa; if really "grizzly bear" then probably Winnebago with cognates in many Siouan languages); ma-coupee (full of magic; Osage, Winnebago, etc.). The words given on this page for beaver, fish, raccoon are presumably Musquakie; Mus-qua-kie given as the equivalent of Fox(=fox) is an error; “Red Earth” is a native term for Fox Indian. Again (p. 10) Hee-coo-nee (grandmother: Iowa); (p. 11) wo-skay-pee-sku-nee-og (all the demons, “Indian-bad,” with Algonquian ending for the animate plural: wo-skay identified by Winnebago; pee-sku-nee identified by Iowa and Oto); (p. 14) ma-cou-pee-sku-
nee (bad medicine, “medicine bad:” ma-cou identified by Osage, etc.; pee-sku-nee as before); Mar-ko-ga (owl: Iowa; cognates in Winnebago, etc.); (p. 36) Wau-kau-
thee (snake, really “rattlesnake:” Iowa; cognates for wau-kau in many Siouan lan-
guages); (p. 41) Ow-wah-see-chee (Religion dance, “Dance for the Dead?” identification probable by Osage); (p. 82) No-chu-ning (a male personal name: I do not recognize it as Musquakie; ng-bar it; it could be Iowa with the meaning “No heart of fear,” “No heart,” or “White Cloud;” very accurate transcription is necessary to determine which); (p. 134) lay-ow-low-see (drum, the noun: Iowa); wah-
now-skee-way-ne-lo (ghost-whistle: wah-now-skee, “ghost” identified by Osage; way-ne-lo is the same as way-ne-lo, courting flute); (p. 138) Lau-no-way-watch-
o-nee-tar (“the Musquakies call it, ‘the blessed, or beloved pipe:’” obviously Iowa).

Here are some words which are readily identifiable as Musquakie (=Fox), Sauk, or Kickapoo (as is known, the three are closely related): (p. 8) beaver (Ha-
ma-qua); (p. 10) wee-ka-ya-up (wigwam); (p. 34) Mee-sham (“covenant with the gods,” really “sacred pack”); Musquakie (passim: true rendition “Red Earth”); Gechee Manito-ah (passim: in this form pretty definitely Musquakie, Sauk, or Kickapoo). General Algonquin are (p. 8) Ah-tha-ba-nee (raccoon), Na-ma-thee (fish); (p. 134) Ma-see-ka (turtle, rather, “snapping turtle”).

On p. 6 we read “Moo-in, the bear.” Moo-in, “bear,” is Eastern Algonquian (Passamaquoddy, etc.), not Central Algonquian. Similarly Chee-nau-og (p. 14) is Eastern Algonquian, not Central Algonquian.
The climax is reached on p. 13: "All the Petuns, all the Ho-da-su-nee. . . ." This means "All the Nation of Tobacco, all the Iroquois."

All this can be readily explained if Miss Owen's informant or informants was (were) bilingual in at least two Indian languages; he (she, they) must have been able to read French and English, or must have been remarkable traveller(s), or possessed all these qualities.

In view of what I have shown above, I think it will be conceded that the data in Miss Owen's book can not be utilized for scientific purposes till we know the source for each item, and until each item has been verified.4

Bureau of American Ethnology

TRUMAN MICHELS0N

A SURVEY OF SUMATRA

Ethnological literature regarding the East Indian archipelago, though abundant, is written for the most part in Dutch, a language which relatively few students command. Dr Loeb's recent volume,1 therefore, being the only work available in English on Sumatra, save for Marsden's 1783 publication, helps fill a wide gap in the knowledge of most ethnologists concerning the Indonesian area. Prof Heine-Geldern's essay on the archaeology and art of the island, which appears as a twenty-seven page appendix, is an excellently planned and written digest of the rather sparse data available on the subject. Dr Loeb's field experience in Sumatra has been confined to the Mentawei Islands, except for a brief trip to the Minangkabau country; the ethnological section of the book, consequently, represents principally an effort at compilation, and offers little that is new to the specialist. It is, moreover, open to several criticisms.

In the first place, the material is not properly weighted in certain respects. The chapter on the Batak occupies a full quarter of the volume and the Mentawei section also receives a disproportionate share of the total space, while the east Sumatra coastal Malay states and the interesting mountain tribes of south Sumatra (Red-

4 The Sac and Fox Tales (Journal of American Folk-Lore, Vol. 15, p. 170 et seq.) by Mrs M. Lasley, transmitted by Miss Owen, contains at least one name (Lonlay) which is Iowa (see p. 176), and the story itself in which it occurs, is like the Iowa one (Ibid., Vol. 38, p. 427 et seq.). On p. 173 of the Tales there are a number of sentences supposedly in an Indian language. I can vouch that although the tales are supposedly Sac and Fox, these sentences are in no Algonquian language: they appear to contain some Siouan verbal forms; in view of the deficient orthography I leave this for a Siouan specialist to determine. Mrs Lasley's Indian name is given as Bee-wah-thee-wah, "Singing Bird." I do not recognize this as Algonquian.

Incidentally I may add that Catlin when discussing the berdache dance of the Sac and Fox gives a number of sentences which are presumably in some Indian language, but I can not identify it.

jang, Lebong, Semendo, Pasemah, and Serawai) are not treated at all. Since the coastal Malays comprise almost a third of the total population of the island, and since the south Sumatra mountaineers form an important connecting link between the Batak and Minangkabau on the one hand and the Lampong on the other, these omissions would seem to defeat the purposes of a comprehensive survey. In each chapter, moreover, social and religious customs receive most attention, while material culture is nowhere treated in detail and the somatic and linguistic aspects are almost entirely neglected.

Tenuous and, in my opinion, unsubstantiated hypotheses constitute grounds for a second criticism. Thus the following statements occur:

The sib organization was brought from South India along with other traits of higher culture. . . . On linguistic grounds this is shown by the lack of a native term for mother's brother in Indonesian (p. 17).

There is strong linguistic evidence that perhaps somewhere between the first and second millenium B.C. Sumatra was subjected to direct Dravidian influence, and that certain sociological customs, including avoidance customs and joking relationships, cross-cousin marriage, matrilineal and patrilineal sibs, moieties, exogamy and totemism, were imported from southern India into Sumatra and the Pacific (p. 120).

The author does not present his "strong evidence" anywhere in the volume, but presumably he refers to the data he recently adduced in an article[2] in which he states that since mama is the Tamil and Singhalese term for mother's brother and akka and nangi are Singhalese terms for elder and younger sibling respectively, these terms, when found in Indonesian tribes having sib systems, indicate clearly the Dravidian origin of sib organization in the archipelago. Mama, or variants thereof, however, is a widespread term for uncle in sibless sections of Indonesia (e.g., south Borneo Ngadju mama, central Celebes Mori ma'ma), and can therefore hardly be considered as specifically Dravidian in origin or necessarily linked with the sib form of organization. Adriani has pointed out that the term probably results from the reduplication of the general Indonesian term for father, ama, which thus becomes ama-ama, reduplication in Indonesian languages being employed to convey the meaning of "like a" or "similar to," and mama consequently connoting originally "like a father." The term could be quite understandably restricted to mother's brother in groups organized on a unilateral basis. The fact that the Gayo of Sumatra, a tribe with a patrilineal sib system, use the word pon for mother's brother also contravenes Dr Loeb's linguistic hypothesis. So also with the terms for older and younger sibling: the "Dravidian" words akka and nangi, or variants thereof, for these relatives are found among such sibless groups as the Ngadju (aka, andi) and Kenya (seken, sadin) of Borneo and the Toradja (tukaka, tua'i) and Sadang (kaka, adi) of Celebes. On page 82 the author remarks that "naturally" (!) the Batak had neither shamans nor priests before the days of Hindu influence, and on page 302 he states that the art of iron working was introduced by the Hindus into the archipelago. Since both shamanism and metallurgical technique are widespread

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in remote districts of Borneo and Celebes, and since there is no necessary reason for assuming that Hindus brought them to these islands or even that they came in by secondary diffusion from Hindu-influenced sources, these two propositions are open to doubt. Another questionable statement occurs on page 288, where the author writes that "it seems obvious that the Kubus are a degenerate race."

The numerous errors of fact that occur in this survey cannot be mentioned in detail here, but a few may be indicated. The Gayo have no institution of divine kingship (p. 38). The Batak house type is not necessarily attributable to Hindu influence (p. 82). The magical substance used by the Batak priests is pupuk, not pangulubalang (p. 86). The Limopuluh Koto are "the fifty towns," not "the fifteen towns" (p. 98). The ruler of the old Minangkabau empire had his capital at Pagar Ruyung, not Palembang (p. 98). The teeth of the Niassans are filed down in an even line, not to points (p. 136). The Dutch voyager who touched at Engano in 1596 (not 1592, as stated) was de Houtman, not Hantmann (p. 212). The sororate is usual, not infrequent, in Atjeh (p. 232). The Gayo are not less isolated, nor are they more advanced in governmental organization, than the Batak (p. 254). Ratu Senuhun was a queen, not a prince, of Palembang (p. 281). Mamak shamanism is not of the same type as that of the Kubu, since the possessing spirit speaks through the lips of the Mamak medium, while the Kubu priest remains silent during his trance (p. 290). The Sakai do not build their dwellings on rafts, as do the neighboring Akit (p. 292).

On page 47 the author notes "Brenner ... writes that the Bataks have two main sibs, the Tartharol and the Teivaliol." I have been unable to discover any statement in von Brenner to the effect that the well known moiety names of the Todas of southern India occur in central Sumatra. Dr Loeb's chapter on the Lampang is in large part an adapted translation of an article by van Hoëvell which appeared in the Tijdschrift voor Nederlandsch-Indië in 1852, but he retains the use of the present tense without regard for the fact that conditions in south Sumatra have been considerably altered since the middle of the past century. Finally, the author oversimplifies in presentation the sib system of the Minangkabau, and accepts the probably legendary existence of four original sibs as fact. De Rooij, Willinck, and Leyds, all experts in Minangkabau ethnology, have combatted this hypothesis, and their views deserve more consideration than they receive. Again, these authorities, as well as others, have demonstrated, contrary to Dr Loeb's assumption, that the suku is not necessarily the sib. De Rooij's remark on this point is important:

'Suku does not invariably have the meaning of sib, and in a large part of the Padang Highlands the suku consists of different sibs, in which case the word suku refers to one of the groups (normally four) into which the various sibs of the village are combined. Frequent misspellings occur throughout the book, particularly of native terms, and a list of corrigenda is needed to rectify these errors. Finally, the volume lacks footnote references entirely, and citations and direct quotations from various

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2 De Indische Gids, Vol. 12, No. 1, p. 649, 1890.
sources are included without indication of their location by either volume or page. The bibliography is far from complete, and although limitation of space undoubtedly required rigid abridgement of the list, numerous important works which are omitted should have found a place there.

Lest the foregoing critical remarks be taken as too derogatory of the work of the principal author, the final statement must be made that the book represents, with the exception of the omission of treatment of the coastal Malays and the south Sumatran mountaineers, an adequate and generally satisfactory summary of the peoples and cultures of the island, and should be supplemented with similar ethnological surveys in English of the other islands of the extremely interesting and important Indonesian archipelago.

RAYMOND KENNEDY

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DE HEVESY ON MUNDA AND FINNO-UGRIAN LINGUISTICS AND EASTER ISLAND SCRIPT

In a short note with the above heading in the American Anthropologist,1 Truman Michelson criticizes Dobo for his enthusiastic appreciation of de Hevesy’s discoveries concerning the identity of the Indus and the Easter Island scripts and the genetic connection of the Munda languages with Finno-Ugrian.

The first of these discoveries is generally considered as one of the most important made during the last ten years. Competent authorities like Langdon of Oxford write that there can be no doubt concerning the identity of the Indus and the Easter Island scripts. As far as I know the only doubt expressed up till now is by Doctor Michelson.

The second discovery that there exist in India Finno-Ugrian languages belonging to the Ugrian branch is of course not a lesser one, because it shows that before the advent of the Aryans there must have been in India an immigration of a Finno-Ugrian speaking people.

Doctor Michelson thought it useful to publish excerpts of a critique by Wilhelm Printz. The fact that de Hevesy in a work published many years ago under a pseudonym committed certain errors has nothing to do with the present question. Prof Printz’s critique can only be regarded as a disloyal one. The question here is not if the author made certain mistakes here and there, such as in Sanskrit instances, but whether it is true or not that the Munda languages are Finno-Ugrian.

Moreover Printz says that de Hevesy’s book is based solely on an uncritical study of the pertinent dictionaries. It is astonishing how such a false assertion could be made: no less than sixty-five pages (pp. 45-110) of the work are exclusively devoted to morphology. These morphological proofs also lead scholars like Prof Coedes, Corresponding Member of the Institut de France and director of the École

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1 Vol. 36, 1934, page 632.
Française d'Extrême Orient, to accept de Hevesy's views and to characterize them as a "pas mince découverte" (Bull., l'École Franç. d'Extr. Orient, 1934).

There can be no doubt as to the importance of the two discoveries mentioned above, but there is still a third one. This is that W. Schmidt's Austric and Austro-Asiatic families of languages are non-existent. It was proved by de Hevesy in a paper in the Bulletin of the School of Oriental Studies (London), in his book "Finnisch-Ugrisches aus Indien" and in a paper presented before the Third International Congress of Linguists at Rome. The evidences brought forward were so convincing that already fully 98% of the specialists agree with de Hevesy.

The American Anthropologist being principally of an Americanist nature, some of its readers are likely to be misled by the tenor of Doctor Michelson's note. I therefore thought it useful to correct any possible misunderstanding.

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MEMORANDUM FOR THE STUDY OF ACCULTURATION

Recognizing the importance of the study of acculturation, and the varying points of view from which the problem has been approached, the Social Science Research Council, early last year, appointed the undersigned as a Committee to analyze the work on the problem already done, to study the implications of the term "acculturation," and to explore new leads for further investigation. After a number of meetings, the following outline was drawn up as a first step toward clarifying the problem and to serve as an aid in the classification of studies already made.

The work of the Committee will be facilitated, and its final report the more complete, if its members have knowledge of as many of the studies of acculturation now being carried on as is possible. To this end, the tentative outline which has been drawn up to help organize its work is presented with the suggestion that information concerning acculturation studies now in progress be sent to the Chairman, or any member of the Committee, at the addresses indicated below. It will be particularly helpful if, in sending such material, the extent to which the data do or do not fall in with the categories set up in this outline might be indicated. It is expected that the results of the Committee's work will be made available to persons who communicate with it. The file of their names, and of the problems on which they are engaged, will also be available for the exchange of information and methods.

OUTLINE FOR THE STUDY OF ACCULTURATION

I. Definition

"Acculturation comprehends those phenomena which result when groups of individuals having different cultures come into continuous first-hand contact, with subsequent changes in the original cultural patterns of either or both groups."

(Note: Under this definition, acculturation is to be distinguished from culture-change, of which it is but one aspect, and assimilation, which is at times a phase of acculturation. It is also to be differentiated from diffusion, which, while occurring in all instances of accul-
turation, is not only a phenomenon which frequently takes place without the occurrence of the type of contact between peoples specified in the definition given above, but also constitutes only one aspect of the process of acculturation.)

II. Approach to the problem

A. Listing of materials available for study

1. Published materials—of prehistoric contacts (to indicate how acculturation has characterized human contacts from early times), as well as of contacts between primitive groups, between primitive and literate groups (both mechanized and non-mechanized), and between literate groups of either or both categories.

2. Unpublished materials of studies in acculturation which are completed or in progress.

B. Classification of the above materials

1. Do these studies treat of entire cultures or specific phases of culture?

2. If the studies are restricted ones, what phases of the culture are treated?

3. What are the motivations of the studies (insofar as this affects the type of material treated), e.g., are they scientific, or are they designed to aid in the formulation of administrative, educational, or missionary policy?

C. Techniques employed in the studies analyzed

1. Direct observation of acculturation in process.

2. Recent acculturation studied through interviews with members of acculturated groups.

3. Use of documentary evidence which gives historic testimony concerning early contacts which have resulted in acculturation.

4. Deductions from historical analyses and reconstructions.

III. Analysis of acculturation

(Note: The significance of physical type in determining attitudes operative in acculturation, as well as the importance of the concomitant occurrence of race-mixture or its prohibition, must not be overlooked as a factor which may pervade any situation, process, or result envisaged in this section.)

A. Types of contacts

1. Where contacts are between entire groups; or are between an entire population and selected groups from another population, e.g., missionaries, traders, administrators, special craftsmen, pioneers and their families, and immigrant males (all these considered with special reference to the elements of culture likely to be made available by the members of such special groups to the population among whom they live).

2. Where contacts are friendly, or are hostile.

3. Where contacts are between groups of approximately equal size, or between groups of markedly different size.

4. Where contacts are between groups marked by unequal degrees of complexity in material or non-material aspects of culture, or both, or in some phases of either.

5. Where contacts result from the culture-carriers coming into the habitat of the receiving group, or from the receiving group being brought into contact with the new culture in a new region.

B. Situations in which acculturation may occur

1. Where elements of culture are forced upon a people, or are received voluntarily by them.

2. Where there is no social or political inequality between groups.
3. Where inequality exists between groups, in which case any of the following may result:
   a. political dominance by one group, without recognition of its social dominance by the subject group;
   b. political and social dominance by one group;
   c. recognition of social superiority of one group by the other without the exercise of political dominance by the former.

C. The processes of acculturation

1. Selection of traits under acculturation:
   a. the order in which traits are selected (in specific cases);
   b. the possible relationships to be discerned between the selection of traits under the various types of contacts leading to acculturation, and the situations in which acculturation may occur (as set down under III A and B above);
   c. partial presentation of traits under forced acculturation;
      a’’. types of traits permitted and forbidden to receiving group;
      b’’. techniques employed by donor group for imposing traits;
      c’’. types of traits whose acceptance can be forced;
      d’’. limitations of forced acceptance;
   d. resistance of receiving group to traits presented to them;
      a’’. reasons for this resistance;
      b’’. significance of understanding resistance to traits as well as acceptance of them.

2. Determination of traits presented and selected in acculturation situations:
   a. traits presented by the donor group because of
      a’’. practical advantages, such as economic profit or political dominance;
      b’’. desirability of bringing about conformity to values of the donor group, such as humanitarian ideals, modesty, etc.;
      c’’. ethical and religious considerations;
   b. traits selected by the receiving group because of
      a’’. economic advantages;
      b’’. social advantages (prestige);
      c’’. congruity of existing culture-patterns;
      d’’. immediacy and extensiveness of changes necessitated in certain aspects of the culture by the adoption of functionally related traits;
   c. traits rejected by receiving group.

3. Integration of traits into the patterns of the accepting culture:
   a. the factor of time that has elapsed since the acceptance of a trait;
   b. the element of conflict produced within a culture by the acceptance of new traits at variance with pre-existing ones, and the degree of conflict which ensues;
   c. the process of adjustment in acculturation:
      a’’. modification and reinterpretation of traits taken over;
      b’’. modification of pre-existing patterns resulting from the taking over of new traits;
      c’’. displacement of older traits in a pattern by new ones;
      d’’. "survivals";
      e’’. transfer of sanctions;
      f’’. shifts in cultural focus caused by acculturation.
IV. *Psychological mechanisms* of selection and integration of traits under acculturation

A. The role of the individual
   1. As member of the selecting group; personality of the first individuals to accept foreign traits and their position in society as influencing selection and acceptance of new traits.
   2. As member of the donor group; personality of the individuals who are in contact with the receiving group, their attitudes and points of view, and the way in which the group to which they belong is regarded by members of the receiving group, as making for favorable and unfavorable reception of traits.
   3. The individual as member of a special group in his society (priestly class, sib, secret society, etc.) and his position in this group, as accelerating or retarding acceptance of new traits.

B. Possible consistencies in personality types of those who accept or reject new traits.

C. Differential selection and acceptance of traits in accordance with sex lines, differing social strata, differing types of belief, and occupation.

D. Initial hostility and subsequent reconciliation of individuals to the new culture as a factor in integrating new culture-traits, and caused by
   1. intensity of contact;
   2. duration of contact and resulting habituation to new cultural elements;
   3. social, economic or political advantages resultant upon acceptance;

E. Psychic conflict resulting from attempts to reconcile differing traditions of social behavior and different sets of social sanctions.

V. The results of acculturation

A. *Acceptance*: where the process of acculturation eventuates in the taking over of the greater portion of another culture and the loss of most of the older cultural heritage; with acquiescence on the part of the members of the accepting group, and, as a result, assimilation by them not only to the behavior patterns but to the inner values of the culture with which they have come into contact.

B. *Adaptation*: where both original and foreign traits are combined so as to produce a smoothly functioning cultural whole which is actually an historic mosaic; with either a reworking of the patterns of the two cultures into a harmonious meaningful whole to the individuals concerned, or the retention of a series of more or less conflicting attitudes and points of view which are reconciled in everyday life as specific occasions arise.

C. *Reaction*: where because of oppression, or because of the unforeseen results of the acceptance of foreign traits, contra-acculturative movements arise; these maintaining their psychological force (a) as compensations for an imposed or assumed inferiority, or (b) through the prestige which a return to older pre-acculturative conditions may bring to those participating in such a movement.

Robert Redfield (Chairman), University of Chicago
Ralph Linton, University of Wisconsin
Melville J. Herskovits, Northwestern University
NOTES AND NEWS

FREDERICK WEBB HODGE ANNIVERSARY PUBLICATION FUND

In December of 1886, Dr Frederick Webb Hodge joined the Hemenway Southwestern Archaeological expedition to Arizona, and began a career in anthropology which will reach its fiftieth anniversary in 1936. The occasion is to be marked by the creation of the Frederick Webb Hodge Anniversary Publication Fund, under the guidance of the following Sponsoring Committee: H. B. Alexander, Franz Boas, Herbert E. Bolton, Fay-Cooper Cole, Carl E. Guthe, E. L. Hewett, Aleš Hrdlička, A. V. Kidder, Jesse L. Nusbaum, Bruno Oetiteking, Elsie Clews Parsons, Edward Sapir, Frank G. Speck, A. M. Tozzer, Henry R. Wagner, Clark Wissler. This Committee will appoint an editorial board, self-perpetuating, to select works in the field of American anthropology for publication by the Fund. Southwest Museum, of which Dr Hodge has been Director since 1932, will administer the Fund as an endowment trust.

All publications will be sold, at approximate cost, the income of the Fund being used as a reserve to meet the heavy initial cost of printing and to cover possible deficits. Contributors to the Fund who so desire will receive a pro rata credit on its publications, enabling them eventually to recover in publications the amount of their contribution in dollars. Contributions should be sent to Hodge Fund, Southwest Museum, Los Angeles, California.

Dr Hodge is one of the pioneers of American anthropology. A founder of the American Anthropological Association, he edited its journal, the AMERICAN ANTHROPOLOGIST, during its first fifteen years, meeting much of the initial expense from his own pocket. The “Handbook of American Indians North of Mexico,” always the standard work of reference on this subject, is but one among many of his editorial and original contributions to the study of aboriginal America. Dr Hodge headed the Bureau of American Ethnology for eight years. His long career has been one of constant support and encouragement to the study of American prehistory. The Fund which is to bear his name offers to his many friends and admirers an opportunity to do him personal honor, at the same time increasing the meager existing facilities for publication of research in the important field of American prehistory.

APPROPRIATIONS FOR GRANTS-IN-AID

At its May, 1935 meeting, the Committee on Grants-in-Aid of the National Research Council made the following awards in the field of anthropology:

Fay-Cooper Cole, University of Chicago: “racial criteria in the study of hair;”
Paul Kirchhoff, Columbia University: “native agriculture in South America;”
Cornelius Osgood, Yale University: “study of the existing anthropological collections from the Athapaskan Indians of Canada and Alaska which have been deposited in museums of northern Europe, especially Russia;”

Vincenzo Petrullo,
University of Pennsylvania: "ethnological studies of the Yanuro peoples in Venezuela."

The next meeting of the Committee will be held in March, 1936. Applications to be considered at this meeting must be on file with the Secretary of the Committee, Dr. Clarence J. West, not later than February 15, 1936.

PRE-DOCTORAL FELLOWSHIPS FOR GRADUATE STUDY

The Social Science Research Council has announced the award of eight pre-doctoral fellowships for graduate study. These fellowships provide one thousand dollars and tuition charges, and are designed to aid exceptionally promising students of the social sciences to obtain research training beginning with the first year of graduate study. Fellows are required to devote their full time to graduate study, in some other institution than that in which they received their undergraduate training.

The fellowships will be offered again for the academic year 1936–37. The closing date for the receipt of applications on blanks to be secured from the Fellowship Secretary is March 15, 1936. Inquiries should be addressed to the Social Science Research Council, 230 Park Avenue, New York City. Each candidate must submit a letter from the chairman of the department in which he has pursued his major undergraduate study, in support of his application, before blanks will be sent to him.

THE SOCIETY FOR AMERICAN ARCHAEOLOGY

In connection with the general meeting of anthropologists at Pittsburgh, December, 1934, there was organized a Society for American Archaeology. "The objects of the Society are to stimulate scientific research in the archaeology of the New World by: creating closer professional relations among archaeologists and between them and others interested in American archaeology; guiding, by request, the research work of amateurs; advocating the conservation of archaeological data and furthering the control or elimination of commercialization of archaeological objects; and promoting a more rational public appreciation of the aims and limitations of archaeological research."

Officers elected were Arthur C. Parker, President; M. R. Harrington, Vice-President; Carl E. Guthe, Secretary-Treasurer; W. C. McKern, Editor.

Membership is of two classes: Affiliates and Fellows. "Affiliates who have engaged in scientific research in American archaeology and who have published upon their researches in recognized scientific media may be elected Fellows." Annual dues are $3.00, which includes subscription to all the Society's publications.

TWO NEW PUBLICATIONS

"The Uganda Journal," which, says Man (1935), "sets out to do for Uganda what Sudan Notes and Records has done for the Anglo-Egyptian Sudan."

"Southwestern Lore," published at Gunnison, Colorado, by the Southwestern Colorado Archaeological Society and the Museum of Western State College, is edited by C. T. Hurst. Annual dues in the Society are $1.00, which includes subscription to the publication.

"Hopi of the Second Mesa" by Ernest and Pearl Beaglehole (Memoirs of the American Anthropological Association, No. 44, 1935) should be amended as follows:

Page 51, line 10 should read: After her hā·'si' the girl grinds blue corn and then prepares white corn meal for ġwi'vdo·si'. Finally she makes sweet corn meal, ġiḍząva'vdo·si'. In these tasks . . .

Page 55, paragraph 5, text and translation should read: 'e'ñem (they) ŋima'ń-dọda' (grind) sis'vi'lanika'e (to pay) 'ovad 'e'be' (for the wedding robe) bīt' bu' (and also) wįi'gposéz'i' (for the big moccasins) bĩ (also) wįi'gwe'wą' 'e'be' (for the big wedding belt).

We Deeply Regret to Announce the Death of Dr Walter Hough, Head Curator, U. S. National Museum, on September 20, 1935 in his seventy-seventh year. Dr Hough was a past President of the American Anthropological Association.
SOME REFLECTIONS ON THE
METHOD AND THEORY OF THE
KULTURKREISLEHRE

By CLYDE KLUCKHOHN

I

PROFESSOR KROEBER has recently discussed in an interesting
manner the background of the anthropological theory of Professor
Boas. It would seem useful to place other theoretical viewpoints in a wider
context, for in those systematic studies where the data are human beings
or products of the activity of human beings it is of peculiar importance that
the student should be aware of the explicit and implicit assumptions upon
which a given conceptual scheme is based. It is a naïve fallacy to suppose
that philosophy of method is irrelevant to the central problems of the dis-
interested research student. Clearly, the sheer denial of “metaphysical”
presuppositions constitutes in itself a sweeping assertion of a philosophical
view of the world. The “facts” do not “speak for themselves.” They have
meaning only when ordered in terms of a system of categories, and any
system of categories is inevitably bound up with definite assumptions about
the nature of things.

It may well be that the chemist can pursue his researches perfectly
effectively without regard to philosophy of method, but in such a subject
as cultural anthropology method and theory are so intimately and im-
mediately related both to the subject matter itself and to the student’s
own fundamental attitudes that it is imperative to examine the connections
of a particular method and theory with more general systems of thought.
Otherwise we are too likely to examine a given proposition only in the light

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1 For valuable criticisms and suggestions in the preparation of this paper I am indebted
to my wife, to Professors Tozzer, Koppers, and Spier, and to Dr Douglas Oliver, Mr Paul
Benedict, Mr Lauriston Sharp, and Mr Henry Sims. To Professor Tozzer I am further obli-
gated for the stimulation of his keen and continued interest.

2 History and Science in Anthropology (American Anthropologist, Vol. 37, 1935, pp. 539–
70). Cf. also Franz Boas, History and Science in Anthropology: a Reply (American Anthro-
of our own premises and sentiments which are—usually quite unconsciously—treated as absolute. The resultant intolerance of other conceptual schemes leads us to see mutually exclusive alternatives, where actually one scheme of concepts sheds its own special illuminations, another another.

If we do not endeavor to discover the premises as well as the central concepts and the applications of anthropological theories we tend to become guilty of a natural confusion of thought, that of judging concepts which may follow quite consistently from one set of premises on the basis of another set of premises to which we ourselves subscribe. For clarity of thinking it is essential to bring the problems of the presuppositions of any theory into full consciousness. We ought to know exactly what they are in any given case, and we ought to form some opinion as to how far such premises are justified in terms of our present knowledge about the world. Fortunately, there seems to be increasing recognition of the necessity for constant and rigorous reevaluation of our premises. Dr Vaillant, for example, in the recent summary of his work in the Valley of Mexico repeatedly suggests most searching scepticism as to certain assumptions of orthodox archaeological method.

It seems improbable that any group of premises can thus far be regarded as entirely preferable to all other groups. On the contrary, it would appear that the analysis of data through different conceptual schemes arising out of different sets of basic presuppositions is likely to result in widely varying but not necessarily mutually contradictory insights into the problems of culture. Cultural anthropology can ill afford to lose any legitimate contributions to the understanding of the complexities of its subject matter by reason of dogmatism or intolerance in the name of science. Hence it is desirable to know as exactly as may be the premises of any systematic method and theory and their ideological roots. An effort ought further to be made to realize just what limitations are imposed and what advantages are offered by a given complex of premises. Similarly, we should try to understand just what the proponents of a given conceptual scheme conceive the primary aims of their study to be. Such an attitude would, I think, help to prevent waste of scholarly energy in polemic founded upon mutual misunderstanding with attendant emotional involvements.

From this point of view it is proposed to examine the Kulturkreislehre. Although it may be admitted that the usefulness of concepts in enabling us to understand particular assemblages of data must be the ultimate criterion of their value, an adequate test of the Kulturkreislehre as applied to a concrete area would demand an extensive and intensive study of considerable length. Moreover, since we have ample objective evidence of the
contemporary virility of the *Kulturkreislehre,* it would seem justified to assume that its theoretical concepts have proven of some practical utility. In view of these considerations and in the belief that the study of the concepts of a discipline as such is of value, this paper will deal primarily with the method and theory of the *Kulturkreislehre* in the abstract and will regard the critique of the applications which have been made of it as a problem of first importance but one which may reasonably be regarded as separate and, for purposes of this paper, as largely irrelevant.

It would have been desirable to have based our discussion upon a somewhat detailed study of the development of the *Kulturkreislehre.* But limi-

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2 The papers in Anthropos and the monographs in the Anthropos linguistic and ethnological libraries and in the Wiener Beiträge attest to the range and the world-wide distribution of the researches inspired by this point of view. An increasing number of sympathetic articles in the Zeitschrift für Ethnologie and other periodicals give evidence of its influence in the German-speaking world. But perhaps the most striking demonstration of its robustness is the extent to which its concepts have recently been applied in related fields; by Pinard de la Boullaye in comparative religion, by Menghin, Heine-Geldern, and Kern in prehistory, by Lebzelter in physical anthropology. Finally, hypotheses arising out of the application of its concepts (for example, the Mon-Khmer theory of Schmidt and the related archaeological reconstructions in southeast Asia of Heine-Geldern) have stimulated fruitful lines of research by those in no sense committed to the *Kulturkreislehre.*

4 The indispensable book is, of course, Professor Graebner’s Methode der Ethnologie (Heidelberg, 1911). It is also worth while to consult certain relevant passages in Graebner’s model, Bernheim’s Lehrbuch der historischen Methode (6th ed., Munich-Leipzig, 1914). The following additional references are also of fundamental importance: Rätzel, Anthropogeographie (Stuttgart, 1899), esp. Vol. 2, pp. 577 ff, 605 ff, 651 ff; Geschichte, Völkerkunde und historische Perspective (Historische Zeitschrift, Vol. 93, 1904, p. 1 ff); Frobenius, Die naturwissenschaftliche Kulturlehre (Allgemein. verständliche naturwiss., Abb. 20, 1899); Graebner, Kulturkreise und Kulturschichten in Ozeanien, and Ankerman, Kulturschichten in Afrika (Zeitschrift für Ethnologie, Vol. 37, 1905, pp. 28–90); Graebner, Die melanesische Bogenkultur und ihre Verwandten (Anthropos, Vol. 4, 1909, pp. 726–80, 998–1032). A critical résumé of the background of the *Kulturkreislehre* prior to 1911 is given by Schmidt, Die kulturhistorische Methode in der Ethnologie (Anthropos, Vol. 6, 1911, pp. 1023–36). See also Schmidt, Kulturkreise und Kulturschichten in Südamerika (Zeit. für Eth., Vol. 45, 1914, pp. 1014–1130); Schmidt and Koppers, Gesellschaft und Wirtschaft der Völker, the first part of the Völker und Kulturen division of the Mensch aller Zeiten series (Regensburg, 1924: this important book is usually referred to as “Völker und Kulturen,” a practice which will be adhered to in this paper). Another work which the Vienna school considers an authentic methodological contribution is Pinard de la Boullaye’s Étude Comparée de la Religion (3rd ed., Paris, 1929), see esp. Vol. 2, Ch. 6, Methode Anthropologique Nouvelle. As for references in English, some important passages from Graebner’s “Methode” are translated and published under the title “Causality and Culture” in The Making of Man (V. F. Calverton, ed., New York, 1931). In Schmidt’s The Origin and Growth of Religion (trans. by H. J. Rose, London, 1931) there are some very condensed considerations of the method and theory: see esp. p. 232 ff. Likewise in the first chapter of Schmidt’s High Gods in North America (Oxford, 1933) and in pp. 1–34 of
tations of space make it necessary to proceed directly to a brief review of its latest systematic presentation, "Beiträge zur Methodik der Völkerkunde" by Dr Van Bulck. As with Graebner attention is focused upon the attempt to establish chronology through typology and chorology, and comparative chorology is regarded as the groundwork of ethnology. But Van Bulck's "Beiträge" would appear to be, to some extent, a reworking of the Kulturkreislehre, especially in terms of criticisms which had been repeatedly leveled against it. Van Bulck takes much more realistic account of the complications and difficulties; he makes an urgent plea for anchoring the Kulturkreise in every possible way to facts established by stratigraphic archaeology, physical anthropology, linguistics, documentary history. He lays less emphasis on migration and more on other forms of diffusion. He draws attention to the importance of the individual in primitive culture, especially as a factor in culture dynamics which he discusses in detail, considering (1) die Kulturfaktoren: (a) mental characters of groups and individuals, (b) the geographic milieu, (c) the cultural milieu, (d) the significance of the personality of individuals; (2) diffusion: (a) by contact of peoples, (b) by contact of isolated individuals, (c) by migration, (d) through stratification connected with conquest, (e) by religious proselytism, (f) by trade, (g) by radiation from a civilization; (3) culture change as manifested in: (a) conservatism, (b) internal development, (c) enrichment, (d) disintegration. The central problem he phrases as follows:

How will it be possible to realize the dynamic out of the static? . . . The static is simply an abstraction. . . . It would therefore be an error to conceive of culture history as something static which neither diffused outwardly nor inwardly altered. Nor is culture history a sequence of static culture strata or culture epochs which follow upon one another. The culture stratum like the culture complex—though demonstrated out of the static culture picture—has not been constant as such. For the only constant factor in culture process is its dynamic development. . .

The ethnographer can never come to grips directly with the dynamic. When the ethnographer describes the Kulturleben of a people, he really abstracts from the actual life and offers in his presentation only a static cross section through the flux of the living. Genetic general history encounters this difficulty also. . . . But this problem makes especially difficult the task of the ethnologist, for his research is concerned in large part with the past and the distant past. . . . The problem is not hopeless, however, for in the present lives the past as well as the future. Kulturleben

his "Primitive Man" in European Civilization (E. Eyre, ed., Oxford, 1934: note that Schmidt's contribution was written in 1930). But none of these give really satisfactory accounts of the Kulturkreislehre.

* Vienna, 1932.
is not a world line, unbroken by creative periods of culture change. If that were
the case, one would have to give up the culture history of the non-literate peoples,
for the earlier would be forever lost and in the present we could only find the present.
But if, on the contrary, the earlier lives on in the present, then analysis and deduc-
tion become successful, for the complex points to its components. These elements
permit the reconstruction of the earlier. But we cannot thus comprehend the earlier
dynamic as such—but only the static aspects of the earlier dynamic cultural proc-
ess . . . . Culture strata, culture groups, culture complexes must not be allowed to
remain as torpid abstractions but must be cross-sections of Kulturleben at various
points. We must try to discover and depict as many as possible of these imperfect
cross-sections. The farther we can go in this direction, the nearer can we come to
recapturing the dynamic course.  

The culture historical viewpoint as redefined by Van Bulck can be
summarized as follows. The first and basic problem of ethnology is that of
working out cultural connections. Its proper aim, as a branch of history
in the widest sense, is to reveal to the greatest possible extent the spatial
and chronological antecedents of the known presence of a given cultural
fact at a particular time in a particular place. Only after we know—roughly
at least—the cultural and environmental context in which a given custom
or institution or artifact evolved can we properly bring psychological and
environmental interpretation into play. A cultural phenomenon can only
be fully understood through the ideologies of those cultural groups in which
it evolved, not simply in the context of its present-day setting.

The ethnologist must begin with a definite, rather small area, studying
all the data both primary and secondary. Following Graebner, Van Bulck
shows how the techniques used by historians in the treatment of their
sources might be applied by ethnologists. He suggests means by which the
ethnologist should check, test, control his direct and indirect sources,
whether objects of material culture, accounts of travellers, monographs of
professional ethnographers, photographs, secondarily recorded anecdotes,
or whatever. By thus using standard and somewhat objective techniques,
the ethnologist can establish the relative trustworthiness of his materials
and the use he is justified in making of them. In dealing with secondary
sources the ethnologist must ever bear in mind that the data have passed
through the medium of at least one human mind, and they must conse-
quently be scrutinized to detect accretions or distortions. Similarly, the
ethnologist must check for the possible interdependence of several accounts.
Various useful external and internal criteria are discussed: for Zeitbestim-
mung—does a given report give reliable information for one period and not

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another?—for Ortliehkeit—does a certain report apply to only part of the area in question? Similarly there must be an Ort-und Zeitbestimmung for every specimen used as well as the application of the Kritik der Echtheit. Van Bulck stresses the point that working through the material relating to even a rather small area in critical and exact fashion takes a great deal of time so that the ethnologist must not expect to jump from one region to another in the turn of a hand. But eventually the data on neighboring regions must be treated similarly.

The student then makes distribution maps and proceeds to the analysis of the distribution of culture traits. He must be sure that his studies of culture elements encompass all the principal aspects of culture: material, spiritual, social, religious, economic. He must beware of a too atomistic handling of his material: he must bear in mind the living, dynamic nature of culture and relate his investigations to the culture bearers as well as to abstracted traits. Nevertheless he must endeavor to determine as far as possible the traits brought into the area in question by commerce, conquest, radiation, religious proselytism, etc., though realizing always that these categories are methodological devices which are more often than not mingled in concrete instances. There must similarly be careful study to form an opinion as to whether the living culture dealt with is relatively homogeneous or whether stratifications übereinander or nebeneinander or durcheinander exist.

The establishment of spatial and temporal distributions reveals that certain culture elements appear in association in a fashion too consistent to be fortuitous. Such geographical culture complexes or Kulturkreise do not constitute unities in the sense of being minutely homogeneous without variation or contradiction: special environmental, individual, and historical factors often eliminate or modify in the passage of time particular traits in particular areas. There are nevertheless certain persistent aggregates. The Kulturkreise must be defined not by any a priori considerations but simply by observation of associated occurrences of culture elements in spatially discrete areas. In regions where contact and fusion have demonstrably taken place, particularly precise investigation is needed, for here combinations of an arbitrary, unstable, and non-organic kind tend to be formed.

Under what circumstances may historical connection be regarded as probable in the face of spatial discontinuity? Any one of the criteria\(^7\) pro-

\(^7\) The Hauptkriterien are Form, Quantität, and Adhärenz (really a special case of the Quantität). The Hilfskriterien are Kontinuität and Verwandschaftsgrad. The Form Criterion is essentially the typological method, but many safeguards are insisted on to make it certain
posed by Graebner and de la Boullaye to indicate relationship and stratification is weak when taken by itself, but when many of them are united through the Quantitativskriterium the evidence may sometimes be regarded as strongly presumptive, occasionally as decisive. To utilize these criteria properly the ethnologist must have studied the receptivity of particular culture traits both in general and in the specific cultures in question. And in every case an effort must be made to utilize the controls afforded by the Hilfswissenschaften: physical anthropology, prehistoric archaeology, documentary history, linguistic palaeontology. In sum, a migration hypothesis must be based upon evidence gathered from as many as possible of the following sources: primary and secondary documents, oral tradition (which must be checked as to whether traditions of different groups are contradictory and as to whether they are contradicted or supported by place names, genealogies, and the like), character of tribal and other divisions, linguistics, physical characters, and the archaeological and ethnographical distribution of culture traits as interpreted in the light of the several criteria.

If satisfactory documentary history or demonstrably trustworthy traditions are lacking in any given case, we must have only a limited confidence in our conclusions, which cannot be regarded as more than heuristic working hypotheses, which can only under exceptional circumstances and with many reservations be used as a basis for further hypotheses. In no

or probable that the resemblances do not rest simply upon the limitations of similar materials or upon the necessities of common function. The Quantity Criterion is basically the principle that we must, in assessing connection, take into account not merely typological resemblances of a single trait but also the whole number of such similarities between traits or groups of traits. The wider the separation the proportionately stronger the evidence of these two criteria must be to justify our basing conclusions or hypotheses upon it. In any case the Criterion of Continuity must be applied: the possibility of a former continuous contact must be demonstrated. The probability of two separated areas being validly connected is increased by the discovery of enclaves between, in which the Kulturkreis in question is at least partially represented. We must also utilize the Criterion of Degree of Relationship. Do the resemblances in question increase in strength and number as we approach the two chief areas now separated by a cultural discontinuity?

De la Boullaye’s criteria: (1) critère de présupposition nécessaire—any culture element which presupposes another is more recent than the one which it presupposes (taken over from Sapir, Time Perspective in Aboriginal American Culture, Ottawa, 1916, p. 15); (2) critère de compénétration relative—ideas and rites are older in proportion as they are more intimately related to each of the parts of the culture, while they tend to be more recent in proportion as they are more isolated; (3) critère d’atrophie—traits which appear moribund are often older than those which are vigorously functioning; (4) critère d’association sociale—that which is observed particularly or exclusively among the upper classes tends to be more recent, while that which is confined to depressed classes is usually older in the region.

* Van Bulck, op. cit., p. 233.
case must we base even a working hypothesis simply upon evidence from only one of the *Hilfswissenschaften*. Likewise the ethnologist must not rest content to accept data from the *Hilfswissenschaften* at face value; he must make some test of its relative reliability. However, evidence which is almost valueless for the special discipline is occasionally useful to the ethnologist. For example, the linguist regards with suspicion the attempt to establish the genetic relationship of languages by comparison of vocabularies. But for the ethnologist word distribution maps will often suggest (or afford partial confirmation of) hypotheses.

Any working hypothesis arrived at must be constantly revised or altered as new facts are brought to light. Only a hypothesis which best enables us to understand all the facts may be retained.

In some cases the *Kulturschichten* (the several *Kreise* which may exist contemporaneously in a specific region ordered in a *relative* chronology with respect to their first appearance in that region) will emerge. But since the ethnologist seldom hopes to advance beyond a rough outline of migrations, it is not always possible to delimit the culture strata of an area with confidence. The endeavor should be to link the older strata in each region solidly to archaeological levels and to tie the younger substantially to historical documents. With these two end points controlled the chances of working out correctly the intervening sequence are much greater.

We must now attempt to give both sharper focus and wider horizons to our picture of the *Kulturkreislehre*. Let us turn to the premises and assumptions of the theory, beginning with those which in part at least are recognized. Although it cannot be maintained that this point of view completely disregards the individual, it is probably true, as Professor Herskovits has suggested, that two premises are basic:

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9 There are certain explicit premises which have been so fully discussed both within and without the *Wiener Schule* that they will not be considered here. Such are human monogenesis (see Schmidt-Koppers, *op. cit.*, p. 71); the hypothesis that well consolidated cultures underwent separate and to some extent parallel developments under conditions of relative isolation before the barriers of the last glacial maximum had disappeared; the hypothesis that migrations were for long exclusively by land and that hence inter-continental movements of peoples (except those between Europe and Asia, of course) in most cases necessarily passed through geographic areas of rather restricted size.

10 Schmidt and Koppers insist upon a certain freedom of the human will and upon the significance of human personality and individuality for the development of culture. See *ibid.*, pp. 36–39; also Schmidt, Fritz Graebner (Anthropos, Vol. 30, 1935), p. 207; cf. Koppers: "... der Einmaligkeit der historischen Handlung ... letzten Endes in der geistig-freien Persönlichkeit des Menschen in besonderen aber im schöpferischen Individuum, Grund und Quelle hat" (Das historische Prinzip und die vergleichende Religionswissenschaft, ms., a lecture delivered
1. Poverty of man's ability to devise new means of meeting his environment.

2. Imitativeness of man and contagiousness of culture.
Professor Boas has called attention to two closely related assumptions:
3. The essential mental uniformity of mankind. 
4. The stability of the union of culture elements.
Some respectable evidence has been adduced in favor of all four of these assumptions, but they would all seem to merit further separate and more exhaustive investigation. The method both of establishing and of refuting them has been too much that of highly imperfect induction. That is, the establishers and defenders of the Kulturkreislehre cite a number of instances in which certain culture elements have been associated with something approaching permanence. Their critics seem to feel that they refute this position by bringing forward a few examples where the contrary has been true. Such a criticism, however, has but little significance unless we assume that there are "laws" of culture like those of chemistry or physics so that a generalization must be rejected if a single particular does not accord with it. Would it not be eminently desirable for some defender or critic or, better
at the International Congress of Religious Science, Brussels, Sept. 16–20, 1935). I should like to express my gratitude to Professor Koppers for giving me access to this paper in advance of publication.

12 Review of Graebner's "Method" (Science, n.s., Vol. 34, 1911), p. 806. If we interpret this to mean "in all individuals, in all races, in all peoples, there exists etwas Gemeinsames under all the differences" (Van Bulck, op. cit., p. 34), Boas is correctly stating an important presupposition of the Kulturkreislehre. Schmidt does not, as has been alleged, deny all validity to the principle of the Elementargedanken. He regards this as probably a correct generalization but thinks it has been given too great a place in method and theory. Schmidt's view is similar to that expressed by Dr Spinden (paper read to the Anthropological Seminar, Harvard University, March 1, 1935) when he said that the great fact of psychic unity was attested primarily by the revealed ability of peoples to accept and appropriate inventions originating in other cultures. Similarly R. R. Marett in the Fraser Lectures, 1922–32 (London, 1932), p. 188; cf. also H. J. Rose, Concerning Parallels (Fraser Lecture for 1934, Oxford, 1934).
14 E.g., Boas, op. cit., 1930. It is obvious that one can also cite examples of known persistence of traits: the Chinese did not take over milk products although in contact with milk-using peoples throughout at least the whole of their recorded history. In any case the culture-historical school in no way asserts that all culture traits persist unmodified in all areas. Further, as Professor Lowie has recently pointed out, Boas himself, in discussing the relations of the Northwest and Siberia assumes "eine uralte Verbindung." As Professor Lowie asks, how does this fit with a categorical denial of the assumption of cultural stability? (Cf. Queries, American Anthropologist, Vol. 35, 1933, pp. 288–96.) The problem is of course one of first importance. Professor Dixon in "The Building of Cultures" has given some compelling demonstrations of the mutability of certain accidental trait complexes.
still, some student unidentified with either position to collect and analyze all definitively documented cases bearing on assumptions 2 and 4? It suggests itself that these would lend themselves to statistical treatment so that we could have an objective means of judging whether a generalization was based upon adequate numbers and of estimating the probabilities that a given association of cultural variables rested simply upon chance concatenations.\textsuperscript{16} This would be of especial importance since these two assumptions are fundamental to the central tenet of the Kulturkreislehre: that the association of culture elements rests solely upon a historical connection and not upon an inner relation of the elements themselves.\textsuperscript{16}

Now let us consider some assumptions which are either less explicitly recognized\textsuperscript{17} or which we can hardly accept in the form usually stated. For example, it is continually asserted that ethnology is to be conceived of as a branch of history and assumed that the method is an historical one. The method is largely historical in form, but the concepts of the theory are in large part not taken from history but rather from the Naturwissenschaften, particularly biology. Of this almost countless illustrations could be given. In Graebner's 1905 paper he says that the differentiae of the Kulturschichten may be thought of as "type fossils suitable to indicate the sequence of the ethnographic strata." The Schichten concept is clearly geological, and the idea of "cultural fossils" is clearly an analogy from palaeontology.\textsuperscript{18} Further, Boas is at least partly right in maintaining that the whole conception of the Kulturkreis rests on the biological dictum of the permanence of unit characters,\textsuperscript{19} and, as Lowie points out, there seems in general in diffusionist

\textsuperscript{15} For a discussion of some points of method involved see Morant, Cultural Anthropology and Statistics (Man, Vol. 35, 1935, No. 37).

\textsuperscript{16} Schmidt-Koppers, \textit{op. cit.}, pp. 70–72. Actually one suspects that there is need for some clarification here, for while it is stated that the persistence of the combination of traits rests only upon historical genetics, yet in the discussion of the concrete Kreise it often seems implied that there is an internal relation of some elements which makes for the stability of the union; e.g., mother-right and agriculture, hunting and totemism. The whole subject of coherences in the organic world would seem a fertile but vast and unexplored field. Zuckerman's discussion of the concept of species cohesion (Functional Affinities, London, 1933, pp. 115–18) is suggestive in this connection.

\textsuperscript{17} It is convenient to postpone the discussion of the unstated assumption that the internal development of cultures in time (Tiefentwicklung) plays such a modest rôle that, compared to the development in space (Breitenentwicklung), it may be neglected (cf. Lebzelter \textit{Zur Methodik menschheitsgeschichtlichen Forschung}, Zeitschrift für Ethnologie, Vol. 64, pp. 190–204, 1932).

\textsuperscript{18} It may be objected that such examples are purely "pictorial analogies." But they make clear or vivid the arrangement of data in terms of what?—in terms of concepts borrowed from the disciplines which have suggested the pictorial analogies.

\textsuperscript{19} Boas, \textit{op. cit.}, 1911, p. 807.
theories to be considerable application of the biological dogma that infinitesimal differences make for survival or destruction. 20

Although verbally it is insisted again and again that the development of human culture is sui generis and not comparable to phenomena of organic or inorganic nature, yet these authors appear to treat cultural facts as the facts of physical or biological science—or as mathematical abstractions. They appear to follow out a cultural analysis and taxonomy which seems to me strictly comparable to racial classification. The physical anthropologist measures and observes a sample of a population. If, after due statistical safeguards have been taken into account, the population on the basis of this sample shows "a combination of metrical and morphological features" extremely like that of another population, he most often assumes genetic connection, more or less regardless of geographical distance. The Vienna ethnologist acquires a sample of the culture of a given people from the literature and from museum specimens. If, on the basis of this sample, this culture appears to exhibit the same combination of qualitative characters as another culture, he assumes a genetic connection between the two. He denies that relations can be demonstrated by tracing out the distribution of single traits, 21 just as the physical anthropologist will deny that racial relationships can be established through hair form or cephalic index alone. Although Schmidt and others insist that the connection between the traits which distinguish a Kulturkreis is an historical one, they consistently use biological metaphors ("genetic relationship," "organic unity") and in many ways appear to treat the Kulturkreis as if it were an organism. The sequence of the Kulturkreise 22 within an area has reminded many of the rejected pseudo-biological Stufenlehre. But Schmidt and Koppers rightly reply that it is one thing to endeavor to establish an historical chronological sequence and another simply to assume that the simpler is younger, the more complex more recent. 23 But on the whole it seems to me that the Kulturkreislehre reflects the domination of biological thought as much as does the evolutionism of Morgan or Whitehead's philosophy of organism or the holism of General Smuts. 24

20 Lowie, op. cit., 1933, p. 293. 
21 Van Bulck, op. cit., p. 20 ff. 
24 I cannot see that any sufficient grounds either empirical or a priori have been established for rigorously excluding biological concepts. On the contrary, on the a priori side, it would
Nor is the kulturgeschichtliche Methode by any means purely inductive, as many of its protagonists have maintained. These investigators start with observation, it is true, but presently they seem, as Kroeber has remarked, "to leap at synthesis before they have pursued exhaustive analysis." One searches in vain for a full and lucid explanation of how the ethnologist may know precisely which traits form a particular Kreis. Certain steps in the process do not seem to be made completely explicit. One has the sense of being confronted rather suddenly with the full grown Kulturreis. And from the created Kulturreis many deductions are made. For example,

seem that since culture has been produced by and distributed by a biological organism and is conditioned on every hand by biological limitations and considerations, there would be reason to think that some biological schemas would be applicable. Cf. Kroeber, Historical Reconstruction of Culture Growths and Organic Evolution (American Anthropologist, Vol. 33, 1931, pp. 149–56). Radcliffe-Brown uses very illuminating analogies between organic and social life: see esp. On the Concept of Function in Social Science (American Anthropologist, Vol. 37, 1935). Lebzelter would proceed even further in the application of biological concepts to the Kulturreiselere (op. cit.). On the other hand, it must be pointed out that not all physical anthropologists accept the method of establishing genetic connection suggested above. Cf., e.g., Boas, Anthropology and Modern Life (2nd ed., New York, 1932), p. 37. Similarly, note the great importance given to convergence in many very recent biological writings. Cf., e.g., Le Gros Clark, Evolutionary Parallelism and Human Phylogeny (Man, Vol. 36, 1936, No. 2).

Not that this is necessarily anything against it. We too often make of "inductive" a shibboleth and give an unwarranted sens pejoratif to "deductive." The situation is an excellent illustration of a too common tendency among scholars to dispose of problems by brandishing a word whose content has not been thoroughly analyzed or understood. Because inductive methods have achieved spectacular results in the natural sciences, social scientists, yearning for equally striking reduction of their more complex phenomena, tend, without fully thinking the matter through, to assume that induction, applied in the same way, would be equally useful to them. It is also worth remembering that, while very largely the method of natural science, induction is the despair of philosophy. See Stebbing, A Modern Introduction to Logic (3rd ed., London, 1933), p. 400 ff: "The Problem of Induction." See also Russell, Our Knowledge of the External World, esp. pp. 34, 222. Russell even says "In the final form of a perfected science it would seem that everything ought to be deductive" (p. 34). Cf. MacIver, Is Sociology a Natural Science (American Sociological Society, Vol. 25, 1931), p. 25: "We are apt to set induction over against deduction, regarding the former as a simple, easily understood, all-sufficing and alone legitimate process of passing from particulars to the general. It is safe to say that, so understood, induction is a chimera and is never the method by which scientific generalization is reached." See also MacIver, Social Causation (American Sociological Society, Vol. 26, 1932), p. 30 ff.

The fact that such entities are postulated and postulated in chronological relations is an essential part of the conceptual scheme, but the names assigned with their specific implications represent only an incidental and non-final result of an application of the method. Koppers in his review of Menghin's "Weltgeschichte der Steinzeit" (Anthropos, Vol. 26, 1931) says (p. 238) that this nomenclature needs revision or reform. (Menghin had suggested speaking of Grundkulturen—term first suggested by Kern—and Stammkulturen in place of Ur-, Primär,
as Lowie has pointed out, the view that any Pueblo tribes adopted matrilineal descent from the Athabascan-speaking peoples can be defended only on the basis of deductions from the union of totemism with matrilineal descent in the postulated Kulturkreise. But, in view of many misunderstandings, it cannot be too strongly emphasized that the leaders of the Kulturkreis movement do not regard the several Kreise as definitively and irrevocably established. Hence the Kulturkreislehre by no means stands or falls in terms of objections which may be raised against any specific Kreis which has been defined.

In the more recent literature there is some recognition that the method is in part deductive. Van Bulck asserts that in certain circumstances the ethnologist may justifiably proceed from the general to the particular. He says that both facts and hypotheses must be used: the points of departure are the discrete facts, but in the course of research hypotheses are used as heuristic aids.

It has often been claimed by protagonists of the Kulturkreislehre that their method is objective, indeed that it is synonymous with "objective method in ethnology." In careful discussions of theoretical points it is realized that flawless objectivity is not attained, but since the claim of

and Secondärkulturen: such a shift has, of course, significant theoretical implications.)

The Kreis concept seems to me probably a valuable one, but there can be no doubt both that its conceptual and logical ramifications need deeper and wider exploration (cf. footnote 16, supra), that the relations of the concept to established data need more meticulous and exhaustive examination, and that (if the Kreise withstand the first two tests) the individual Kreise must to a fuller extent "be forged in the teeth of stubborn and irreducible facts."


13 Koppers, for example (op. cit., 1931, p. 228) recognizes that the very phrase "socially significant happenings" in the Kulturkreislehre definition of culture history implies a value judgment. Likewise using the expression "from simple to undifferentiated" as applied even to material culture, implies, he admits, often not merely a quantitative but also a qualitative judgment.
objectivity is often made, albeit sometimes with qualification, it seems worth while to point out some respects in which the method appears subjective. In the first place, as has already been suggested, the means of establishing the Kulturkreise have hardly yet attained to full objectivity. Even when it is asserted that the traits used in determining the Kreis must be those which are essential to the Gesamtheit of a culture, does not this unavoidably involve a value judgment? I, for example, would feel that Van Bulck has neglected a very important category: the aesthetic. In the second place, the very definition of culture elements is inevitably somewhat subjective—as is the application of the Form Criterion. The Quantity Criterion, however objective it may be by itself, is applied, it must not be forgotten, to data selected by the Form Criterion. As Boas, Malinowski, and others have insisted, the method of establishing cultural identities is not without serious subjectivities, and, as Goldenweiser has observed, "if our judgment of cultural identities is far from erring, then geographical factors can no longer be disregarded." Nevertheless it would seem logically unsound to reject a method on the ground that it does not perfectly attain its ideals, and this method certainly represents an advance over the undisciplined application of opinion to each successive problem.

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21 Schmidt, High Gods in North America, p. 8, does include the aesthetic.
22 Cf. Lowie, On the Principle of Convergence in Ethnology (Journal of American Folklore, Vol. 25, 1912), esp. p. 27. Throughout this paper I have used the expressions "culture trait" and "culture element" without defining them. This should not be taken to imply that I do not recognize the difficulties involved in such definition. I feel that such "atomization" often does real violence to the cultural phenomena in question, but it is necessary to the clarification of some issues and, I believe, justifiable, provided one takes the problem into account.
24 In particular it is felt that alternative explanations in each specific case are not sufficiently weighed. Cf. Lesser, Functionalism in Social Anthropology (American Anthropologist, Vol. 37, 1935), p. 392.
25 Diffusionism and Historical Ethnology (American Journal of Sociology, Vol. 31, 1925), p. 22. The geographical factor is not one which the Kulturkreislehre proposes to disregard in any case, except when certain conditions have been fulfilled. In this connection the increasing realization of the complexity of their subject-matter on the part of writers of the culture-historical school should be emphasized. See, e.g., Koppers, op. cit., 1935; Schmidt, Ursprung, Vol. 6, pp. ix, x. The leaders of the Kulturkreislehre have repeatedly stressed the general principle that the resemblances must be of a highly specific order. But the essential dilemma of the subjective nature of the Form Criterion must be regarded as unsolved, and it seems unlikely that any purely objective formula for dealing with all instances could be devised.
It is true that no purely objective, or at least no even approximately quantitative, criterion has been proposed as a standard for the degree of complexity which must be involved in a trait before resemblances can be regarded as significant. Nor has a touchstone for the total number of traits which must be demonstrated as related between two areas been put forward, although it has been suggested that the minimum number would be different in the case of more ancient and more recent complexes. The fact, of course, that mainly complexes of traits are dealt with makes for a safeguard. In any case the fundamental difficulty is realized and the cautions urged should, if meticulously followed, prevent many or serious errors. In any case does not the very nature of the subject matter make an inflexible formula absurd? The relevant question is: considering each case on its own merits in terms of the general principles laid down, does a judgment based on a number of traits represent an approximation to the truth? It would seem a fallacy to demand, as some of the critics of the Kulturkreislehre apparently do, that methodological principles must lead us to the precise uniformities of the physical sciences. It seems likely that the cultural anthropologist ought to be content with principles which will establish trends and tendencies with a high degree of statistical probability.

27 There remains the associated dilemma of the weighting of data. Sapir has criticized Graebner for this (Time Perspective in Aboriginal American Culture, Ottawa, 1916, p. 87). This monograph, though containing many severe criticisms of Graebner, is consistently referred to by the followers of the Kulturkreislehre as the most important American contribution to methodology (cf. Schmidt, Origin and Growth, p. 226 ff). Lebzelter (op. cit.) on the basis that more refined taxonomical principles must proceed a mature ethnology, has proposed that ethnologists endeavor to distinguish between those characters which are essential (konstruktis) and those which are incidental to a given culture.  
28 "We, too, are well aware . . . of simpler and more complicated forms. We are also aware that there are simple, elementary, be they material or purposive, identities and similarities, which therefore are of no import in establishing culturally related connections" (Koppers, review of Dixon, "The Building of Cultures," Anthropos, Vol. 24, 1929, p. 696).  
29 For another discussion of these general principles see Steward, Diffusion and Independent Invention (American Anthropologist, Vol. 31, 1929, pp. 491–96).  
30 The contemporary physicist even finds that some phenomena escape from "the straight-jacket of uniformity." Cf. Lindemann, The Physical Significance of the Quantum Theory (Oxford, 1932), esp. pp. 15, 145 ff. Cf. also E. Schrödinger, Science and the Human Temperament (New York, 1935). It would seem probable that the student of culture should avoid the word "cause" and think in terms of such concepts as "mutual dependence," "necessary association," and "conditioning." It would also seem that such relations, at least in certain limited problems, might best be indicated or demonstrated by statistical methods. Cf. Klimek and Milke, An Analysis of the Material Culture of the Tupi Peoples (American Anthropologist,
Some writers have attributed to Schmidt and Koppers a kind of external and objective view of culture which they themselves disavow. On the ground that in "Völker und Kulturen" they seem to regard social organization in certain Kulturkreise as dependent on economic organization, a belief in economic determinism and an acceptance of a mechanical, materialistic causality has been ascribed to them. But, rather, as Walk has shown, their position is that external environmental circumstances have conditioned (in the sense of stimulating or prohibiting) the rise and development of man's spiritual and social culture. As follows naturally from their general philosophical position, it is a question of the timeless entities which exist as inner potentialities in the human spirit realizing themselves in time and in the manifest forms of material and social culture.

The writers of the Kulturkreislehre reject the physico-mathematical notion of causality as meaningless in culture-historical phenomena which are in time and hence not reversible and in which it is impossible to define identical circumstances with minute exactitude. They do not, however, like Spann, simply substitute the concept of Gliedlichkeit for that of Ursächlichkeit. They retain the concept of causality but their concept appears to refer almost exclusively to an antecedent-consequent relationship between events. But it is not, one infers, identical with time-sequence. Schmidt implies that Wheeler expresses the Kulturkreislehre view of causality when he says:

In history . . . we are dealing with a set of unique terms in an everchanging system. Causality is based on selection with reference to the purpose in hand. All history is idiographic: that is to say, its aim is the conceptual reconstruction of Historical Reality in the greatest possible truth. This aim therefore will be the construction of a conceptual series in Time whose inner coherency as tested by general reality is a maximum, that is whose terms follow most necessarily from one another. And these are Causal series.


L. Walk, Der Kausalitätsbegriff bei Schmidt-Koppers und Othmar Spann (Schmidt Festschrift, Vienna, 1928, pp. 969–77).


Cf. Kategorienlehre (Jena, 1924), esp. pp. 54 ff, 92 ff; Gesellschaftslehre (Jena, 1923), pp. 8, 530 ff.

Schmidt, review of Wheeler (Anthropos, Vol. 8, 1912), pp. 252–54; cf. also footnote 135.
The same thing may be expressed by saying that an Historical Causal Series is one through which runs an unbroken identity. Cause and effect, therefore, in History is the temporal relation between terms in a coherent series, each of which carries on an Identity. Its value or completeness varies in degree; and the aim of historical research is to raise this degree, that is to strengthen the Continuous Identity. This is true whatever be the social product which is dealt with—a people, an institution, or an implement.46

While its defenders would place their system in Goldenweiser’s Objective-Historical category, the Kulturkreislehre clearly has deep-going affiliations with the rationalistic deductive logics.47 In this connection it is enlightening to remember that Schmidt is a priest, that he was trained as a theologian, and is steeped in the dialectical subtleties of Thomas Aquinas and Albertus Magnus. And it is by no means irrelevant to our understanding of the Kulturkreislehre to realize that Koppers also is a priest, and that both Pinard de la Boullaye and Van Bulck are members of the Society of Jesus. Pinard de la Boullaye was a professional logician before he became a comparative religionist.48 I do not in the least mean to imply that the scientific worth of their researches is thereby negatived. Kant was surely right in maintaining that cognition is impossible without the application of interpretative principles—and those underlying the metaphysics of the Roman Catholic Church are as intellectually respectable as any others in the present state of our knowledge about man and the universe. We must, I think, rigorously avoid the temptation to dismiss the Kulturkreislehre as founded upon a “bias.”49 I sometimes wonder if it is not in part true that


47 Its expounders ought to agree fully with MacIver when he says: “It is inherent in the nature of thinking that we never know a particular except as an instance of some dimly or clearly conceived type. By the analysis of various particulars we correct, or modify our idea of the type, we do not create it de novo” (op. cit., 1932, p. 31). It is implicit in most of the thinking of the Kulturkreislehre that universals are before things.

48 We must not forget, of course, that Graebner and Ankerman had no such connections, and it is worth remembering that Schmidt receives a drubbing in the “Methode.”

49 It is true, of course, that Schmidt and Koppers, as priests, are almost compelled to reject “Evolutionismus” (while accepting evolution as a valid principle abundantly established in paleontology and biology) as based upon the assumption that human beings are subject to the rigid determinism which seems to prevail in nature generally. It is also true that some of Schmidt’s (in particular) observations upon anthropological matters (e.g., his discussion of primate relationships in Eyre’s “European Civilization”) seem to have a direct ulterior relation to certain tenets of the Roman Catholic Church. But the tendency for scientists, particularly in quasi-popular works, to give a prominent place to facts and theories which fit their personal Weltanschauung is almost universal and would seem not illegitimate so long as no violence is done to the facts as known. Nevertheless it must be acknowledged in reading the
because its "biases" are somewhat more apparent than those of some other ethnological schools that the doctrines are therefore perhaps the less dangerous. And how much difference, after all, is there between a "bias" and a "premise." A "bias," I suppose has what the psychoanalysts call "affect-content." But if we are quite honest, how many of the fundamental presuppositions upon which the thinking of any of us are founded are truly free from emotional content? Are not the fundamental operations of science at best little more than consistent arrangements of data in accord with a few dominating concepts?

We all approach any problem with a view of the world, with often unrealized presuppositions as to the nature of things which deeply condition both the data we perceive, the ideas we conceive, and the relations we infer between them. It is important to remember this, it seems to me, and to beware of the fallacy of the behaviorist and other naively mechanistic approaches. In considering the Kulturkreislehre the illuminating fact to be reckoned with is that it is most intimately related to the Absolutist philosophies, to the scholastic philosophies of the middle ages, and, of modern philosophies, particularly to that of Hegel with its central proposition "nicht causae sondern rationes beherrschen die Welt."

Yes, the Kulturkreislehre can only be fully comprehended in terms of the intellectual climate in which its covert assumptions have matured; just as it is meaningful to remember that the theoretical position of Professor Boas (and of other American anthropologists) has very real connections with certain rather dominant currents of recent American thought, notably the philosophical thought of James and Dewey. Indeed such a typically American anthropological concept as that of the culture area is more fully comprehended in such a frame of reference. Compare the revival of Morgan in Russia.

more serious works of Schmidt and Koppers one also rather often gets a sense of their lack of detachment in viewing certain questions.

Cf. Whitehead, Adventures of Ideas (New York, 1933), p. 198: "... the first point to remember is that the observational order is invariably interpreted in terms of the concepts supplied by the conceptual order." And Poincaré, Science et Méthode (Paris, 1909), p. 12: "La méthode, c'est précisément le choix des faits" (cited by Homans and Curtis, An Introduction to Pareto, New York, 1935).


As Herskovits has remarked, the culture area is a two-dimensional concept, the Kulturkreis a three-dimensional. The culture area is "a technique of description which involves one time plane" (op. cit., 1930, p. 62). For a recent criticism cf. Woods, A Criticism of Wissler's North American Culture Areas (American Anthropologist, Vol. 36, 1934, pp. 517–23). For
II

I shall now present some facts and opinions relevant to a number of crucial issues by approaching them through discussions published by English-speaking anthropologists of more than one school of thought. I believe that this will not only enable us to see more clearly both what the ideas of the *Kulturkreislehre* are and what they are not, but that we shall also gain thereby some material for reflection upon the anthropological significance of anthropological controversies generally.

We may well begin with Professor Dixon's "The Building of Cultures." This is often referred to by American anthropologists as giving an excellent criticism of the *Kulturkreislehre*. And a sober and attentive reading does almost inevitably create the impression that here we have a plain-spoken but careful, sane, altogether specific and logical evaluation. But—to borrow a phrase from Professor Dixon himself—"let us look a little more closely into the facts."

Note first of all that although "The Building of Cultures" was published in 1928, Dr Dixon makes no reference to any modifications in the *Kulturkreislehre* subsequent to 1914, although by 1924 "Völker und Kulturen" and the first edition of "Étude Comparée" had appeared, and in 1926, in the second edition of "Der Ursprung der Gottesidee," Schmidt had given a concise summary of his point of view (pp. 743–67).

Dixon, like a number of other American scholars, persistently refers to the *Kulturkreislehre* as "the theory of culture strata." But *Kreis* cannot be translated as "stratum," or, if it is meant simply to name the theory
for its leading idea, the choice seems unfair and distorting, for that part of the theory referring to Schichten, while an important concept, is only one and is not fundamental to the others. The Kreis is the core of the theory. It is like calling the Roman Catholic Church "the church of the infallibility of one man;" that is, it seems to be the fallacy of naming by the one concept to which the critic has strongest objection.

Now let us turn to a quotation:

One school, which may be called the diffusionist holds that culture traits have been invented or discovered but once, and that from that single instance all other examples of its occurrence have been derived by means of borrowing or diffusion. According to this view pottery is an invention made early in man's history at some definite time and place, and from that centre of origin all known cases of the use of pottery have been derived, it being "unthinkable" that such invention could ever have been made twice.\textsuperscript{33}

Since Dixon gives the Kulturkreislehre the place of honor in his chapter called "Theories of Diffusion" it seems fair to assume that it is the exponent of this theory who holds the possibility of independent invention to be "unthinkable." Certainly he does not except them. Aside from a number of instances in the "Methode," the possibility of independent invention is admitted (to cite but a few instances) on pages 1021 and 1114 of Schmidt's work on South America which Dixon has selected for special criticism. In "Völker und Kulturen" the possibility of the independent invention of the conical roofed house, for example, is not rejected, and on page 681 of the same work independent origin is not merely admitted as a possibility but is actually assigned to account for the presence of the signalling gong in South America.\textsuperscript{34}

Dixon finds that "the best available way... of showing where and how this hypothesis breaks down"\textsuperscript{35} is the analysis of a paper published in 1914, one of the first experiments in the use of these concepts and this method: Schmidt's "Kulturkreise und Kulturschichten in Südamerika." In the introduction Schmidt himself called attention to the probability that much of the evidence and the conclusions resting upon it would be proved incorrect. He has since on more than one occasion recognized that his prophecy was fulfilled, and a completely redone version is to appear in the Zeitschrift für Ethnologie. Now to hit at the weakest point is clearly skillful tech-

\textsuperscript{33} Dixon, \textit{op. cit.}, p. 33; cf. also p. 55.

\textsuperscript{34} I should like to point out a minor inexactitude. Dixon says "the culture strata defined are based upon an analysis of the cultures of the peoples of Oceania" (p. 228). This is a half-truth: "Oceania and Africa" is the whole truth.

\textsuperscript{35} \textit{Op. cit.}, p. 228.
nique if one is an avowed attacker, but is devoting almost one's entire attention to the weakest exemplification of a theory the proper method of a disinterested appraiser?

However, let us examine the attack. Dixon devotes about a sixth of his total discussion to the question of diffusion of the blowgun. His points are based entirely on the following premises:

According to the culture-strata hypothesis the blowgun was one of the traits belonging to one of the later waves of immigrants to reach America from southeast Asia and neighboring Oceania, and its presence is accounted for by diffusion by way of Bering Straits.\footnote{Op. cit., p. 230.}

The first premise is correct, but the second totally incorrect. It is true that in his introduction Schmidt states that he believes a majority of the diffused culture traits reached the Americas through Bering Straits. But on p. 1096 he categorically states that the blowgun belongs to the frei- vaterrechtliche Kulturkriese and on p. 1098 he says that the relatively few traits representing these culture complexes in South America were probably brought by the more or less accidental arrival of a few boats on the west coast of South America. The justification for this view is not relevant here: the point is that the dilemma which Dixon so positively states has two horns really has three.

On p. 227 Dixon says:

Fuegians, at the southern tip of South America, were accepted, perforce, as representatives of far-wandered Australians and Tasmanians or collateral branches of these

and on p. 229:

"... in order to bring the cultures of Australia and Tasmania to Tierra del Fuego."

But Schmidt makes it plain that he does not regard the Fuegians as "representatives of far-wandered Australians." What he did maintain was that the cultures of all these areas arose in large measure from a culture which is extinct in pure form and whose exact geographical centre is as yet undetermined but was very probably somewhere in central eastern Asia.

On page 235 Dixon writes as follows:

The cross sections of all bows are either round or flat or some intermediate shape, classed for convenience with one or the other extreme. The fact, therefore, that any particular bow is round or flat has relatively little evidential value, as there are no other alternatives than a round or flattish form.
Isn’t this very much like saying that all pottery vessels are of either open or closed form and that for purposes of classification we ought to lump them all in these two extreme classes, disregarding whether they are jars or cups or bowls? Surely it is plain that Schmidt’s diagrams of cross sections show more than two alternatives.

Similarly when Dixon says “all known paddles throughout the world belong essentially to one or the other of these types.” The joker here is, of course, in “essentially.” What did Dixon say of Nordenskiöld’s chart in Part 3 of the “Ethnographical Studies”?

Dixon states (p. 233):

That the tallest tribes of the whole continent, the Ona and Patagonians, should exemplify a culture supposedly brought by pygmy folk is also rather hard to reconcile with common sense, but the difficulty is solved by declaring that because [italics Dixon’s] of their tallness these tribes must be put into a subgroup of later origin, powerfully influenced by cultural contact with the “pygmoind” folk!

Schmidt (p. 1023) does not say that this subgroup is created because of their tallness. He says that for other reasons (which are subsequently given) a later arrival is indicated.\footnote{A view substantiated, incidentally, by impartial students. Cf. S. K. Lothrop, The Indians of Tierra del Fuego (Contributions from the Museum of the American Indian, Heye Foundation, Vol. 10, 1928), especially pp. 200–201.}

... diese Untergruppe auch in mancher anderen Beziehung, wie wir sehen werden, starke Abweichungen offenbart und d a m i t [emphasis mine] als eine jüngere Einwanderung sich darstellt.

Dixon writes (p. 236):

Thus of the nineteen specific traits mentioned by Schmidt, as characteristic of the blended cultures in Oceania, we find that in the South American regions where these cultures are supposed to be present, ten are wholly absent, four are found as frequently outside the area as within it, three are relatively rare, and one is not really comparable, although actually comparable forms occur outside the region concerned. When the claim, therefore, is investigated it appears that but a single trait out of nineteen—a housetype—is actually characteristic [italics Dixon’s] of the region.

Now, first of all, I share Professor Whitehead’s distrust of arbitrary deductions in ex absurdo arguments. Second, the ambivalence of characteristic like that of essentially is obvious. Third, after careful study, I was unable fully to reconcile Dixon’s figures nineteen, ten, and four either with Schmidt’s data or with Dixon’s discussion thereof.
In concluding Dixon remarks:

Now this correlation between the series of traits does not, as a matter of fact, exist, so that the only conclusion possible is that the assumption that we were dealing with a real complex is without foundation. In connection with one of the other Oceanic culture strata which the hypothesis finds in the New World, Kroeber has specifically investigated this point and finds the same result.

Kroeber and Holt, however, do not investigate the association of a “series of traits” but only of two. Moreover, Dixon might well have referred his reader also to Schmidt’s detailed reply to the Kroeber-Holt article: “Die kulturhistorische Methode und die nordamerikanische Ethnologie.” Schmidt first points out that Holt had by no means examined all the pertinent literature; he next critically reexamines the data and makes a case for showing that instead of the facts revealing forty-one instances where the Graebner theory fails and thirty-one where it holds true, as Kroeber and Holt had claimed, there were forty-six instances where it works as opposed to nineteen where it fails. I am not here interested in the relative merits of these two examinations. But I am interested in the fact that Dixon cites one reference but not the other.

I hope my purpose in calling attention to these points in Dixon has been clear. I have no wish to defend Schmidt’s views on South America. Even Professor Koppers has admitted that some of Dixon’s objections to Schmidt’s results are probably justified. Some of the issues I raise are in themselves inconsequential. I am not interested in purely verbal differences or discrepancies, but I feel that these are such in number and nature as (together with Dixon’s bibliographical omissions) to justify the conclusion that Professor Dixon has approached the Kulturkreislehre not from the point of view of giving his readers a balanced, critical picture of its merits and demerits but rather from the point of view of demolishing it. It is a polemic, not a critique.

Now if “The Building of Cultures” were the single American example of this attitude, the matter would have only trifling significance, but it is my impression, after reading with some care most, I believe, of the pertinent books and papers, that the attitude of the greater number of American anthropologists who have discussed the Kulturkreislehre in print

90 “I do not mean to imply thereby that all the results obtained by the historical method are of permanent value. Not at all. I readily admit that some of Dixon’s strictures in this regard may be correct” (op. cit., 1929, p. 699).
seems similar to Dixon’s in that the judgments pronounced or implied hardly bear evidence of being fair and detached verdicts which attempt to weigh all the evidence.\footnote{On this point it is worth observing that Herskovits in The Cattle Complex in East Africa (American Anthropologist, Vol. 28, 1926) limits his discussion of the Kulturkreislehre to works published in 1914 or earlier. He does not even cite “Völker und Kulturen” in his bibliography nor a single title by Koppers. Similarly, in his article The Culture Areas of Africa (Africa, Vol. 3, 1930) he contents himself with citing the “Methode.” (It must be admitted that his discussion here is very summary.) But Radin in The Method and Theory of Ethnology (New York, 1933) putatively intended to give a critical survey of the whole field of ethnological theory for a scholarly audience, discusses the Kulturkreislehre at some length but quotes from a single book: Schmidt’s “Origin and Growth of Religion,” a handbook meant primarily for students of comparative religion, touching on theoretical and methodological questions only incidentally. The reader is not even made aware of the existence of “Völker und Kulturen,” “Étude Comparée,” or of any other books and papers written after 1911. Similarly, Goldenweiser in History, Psychology, and Culture (New York, 1933) has allowed his discussions of this school to stand as if there had been no modifications or development since 1914. Wallis in Culture and Progress (New York, 1931) discusses this point of view without even mentioning Schmidt, nor does he cite a single title by Schmidt in an otherwise extensive bibliography.

My objection here is not a purely academic one. There have been significant modifications in the theory since 1914 (cf. Kroeber, op. cit., 1935, p. 549). As Lips has recently observed, Schmidt and Koppers have given to Graebner’s method and theory, which were based very largely on material culture, a linguistic and sociological foundation (Fritz Graebner, American Anthropologist, Vol. 37, 1935, p. 324). Indeed they stress their opinion that the forms of non-material culture are more fundamental, for technology is more immediately influenced by environment. Actually Schmidt rejects the dichotomy as a dialectical fallacy in any case: “Das Gestalt der Seele ist die Kultur. Alles was Kultur ist, ist nur so weil es in der Seele gewesen ist und ist aus der Seele gekommen.” Cf. Menghin, Weltgeschichte der Steinzeit (Vienna, 1931), p. 12: “Man kann materielle Kultur geistgebundenen Stoff, geistige Kultur stoffgebundenen Geist nennen.” Further, there has been less emphasis on diffusion by migration, and more emphasis on the supplementing and controlling of inferences by data from the various Hilfs- or Nachbarwissenschaften. (Trimborn [op. cit.] has recently suggested that geography be also considered in this group.) This general tendency is very marked in all the more recent publications (e.g., Koppers, op. cit., 1935; Schmidt, Ursprung, Vol. 3, p. 38). (The difficulty that the archaeological data bear only on material culture—occasionally inferentially on non-material, e.g., the kiva in the Southwest—has perhaps been insufficiently recognized.) Finally, there has been a progressive refinement and clarification of the conceptual scheme: this is still proceeding within the movement as the work of Van Bulck and Lebzelter shows. In view of these various changes certain of the leading criticisms made in the American publications cited have only an historical interest; that is, they may be pertinent with reference to what Graebner said in 1911, but they are irrelevant or misleading when considered as applying to the theory as it existed at the time of the publication of the critique.\footnote{E.g. Goldenweiser, op. cit., 1933, p. 149.}
labels are libels, and while, if you give a certain sense to "historical," it may be that this school is not entitled to that description, nevertheless to sweep it aside as "pseudo-historical" and in the same breath to call Boas and his school "historical" seems opinionated. Observe also the varying translations of Kreise. circles, spheres, horizons, zones, strata, cycles. Does this suggest any real apprehension of the concept?

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63 Cf. Radin, The History of Ethnological Theories (American Anthropologist, Vol. 31, 1929), p. 16: "In short the so-called historical method of Professor Boas is really a purely logical and analytical one and is naively unhistorical." Similarly Kroeber, review of "Primitive Art" (American Anthropologist, Vol. 31, 1929), pp. 139–40: "But it would be misleading to consider a non-historical method, essentially allied to that of Wundt, historical merely because it recognizes the historical complexity of certain cultural phenomena... And history is what Dr Boas, in this as in all his work with one or two brief and hesitant exceptions, has avoided doing, and apparently sheers off from distrustfully." For an elaboration of this thesis see Kroeber, op. cit., 1935. For the other point of view see Goldenweiser, op. cit., 1925, and Boas, op. cit., 1936. In connection with the Kulturkreislehre approach, see Firenne, What are Historians Trying to Do? (Methods in Social Science, Chicago, 1931), p. 435: "The subject of historians' study is the development of human societies in space and time."

64 Graebner tells us that the Kulturkreis is the "culture complex geographically considered" (as the Schichten are the complexes of an area historically considered). The rendering "geographic culture complex" therefore suggests itself. However, in the authorized translation of Schmidt's "Handbuch" and in his own paper in the AMERICAN ANTHROPOLOGIST, the literal translation "culture-circle" is used. On the other hand, in "High Gods in North America" the phrase "organic cultural unities" is employed. Koppers (1935) speaks of the Kreise as "large culture entities." Each of the above equivalents tends to have certain connotations and denotations which are not implicit in the German original. Hence it would probably tend to prevent confusion of thought if Kulturkreis were taken over into the English anthropological vocabulary, as part of its corpus of technical foreign words.

65 As suggested in the preceding footnote there is a real problem here, and it must be granted that part of the difficulty is inherent in the fact that the leaders of the Kulturkreislehre have not defined the concept with a maximum of lucidity. (Cf. footnotes 16 and 26, supra.) A recent attempt at clarification within the movement is noteworthy. Lebzelter (op. cit.) proposes that we speak of Kulturenkreis rather than Kulturkreis: "We designate a group of individual cultures which are bound together by a corresponding number of essential and incidental characters as Kulturenkreis. Therewith it is expressly stated that the Kulturenkreis is a systematic category at a rather high level, an abstraction which never has reality as such. Kulturenkreise likewise naturally do not migrate. When it is said that a Kulturkreis has, let us say, 'diffused into South America' therewith is meant, more exactly put, that peoples who were the bearers of the secondary cultures which we today unite into a Kulturenkreis brought this secondary culture into the region named.'

As Pinard de la Boullaye has pointed out, misunderstanding has arisen from the failure to distinguish between typoculturelle and couche culturelle. For instance, it is legitimate to speak of "the Plains Indian type of culture" but this does not rule out the possibility that this culture results from a fusion of separable strata. In general, de la Boullaye's discussion of the Kreis and strata concepts are remarkably clear. He has also made an interesting tentative (with his
The "fallacy of the extreme" is very frequent: that is, a single statement is played up prominently, an attempt is made to show it absurd, it is then either implied or directly inferred that by this and a few other instances the whole theory is disposed of. Whether the statement in question is fundamental to the whole point of view or whether it represents the school as a whole or in its latest views does not always seem to concern the critics. For example, Graebner's reference to a kulturgeschichtliches Nonsens is thus singled out by Sapir, Lowie, Goldenweiser, Herskovits. This is certainly as Boas calls it, "a daring proposition," but none of the four authors referred to informs us that Graebner qualified his statement. He did not say "the diffusion of isolated cultural elements—even of myths—is im-

lignes isthiques) in the direction of rendering the establishment of the Kreise more nearly objective.

Lebzelter (op. cit.) has also made stimulating contributions to this end. Starting from the premise that "each separate culture is to be understood basically as an attempt at an optimal meaningful adaptation to the given environmental situation," he says: "What we need for the construction and for the foundation of culture history is the following: if we want to find the postulated free mother-right culture, we must first have full and intelligent analyses of individual cultures . . . (it would be desirable of course to have such analyses of all cultures with this social structure). Through the analysis of characters all the adaptations to the so widely different environmental conditions (among which are a number of 'inventions' and still more 'discoveries') could be abstracted and in this way one would get not ten or twenty but rather a hundred or more corresponding traits. These correspondences might be homologies, arising indeed on the basis of common heredity out of older culture strata or they might have come into being through borrowing or analogies (convergences, parallelisms)."

"A migration of single cultural elements, also of tales, over wide distances, without the spread of other cultural possessions at the same time, may be confidently designated as a culture-historical absurdity" (Methode, p. 116).

It is notable that no review or discussion of the "Methode" by an American anthropologist attempts a comprehensive survey of the whole book. Invariably Graebner's controversial ideas are seized upon. It is natural and proper to develop points of difference, but, with the exception of a passing reference by Boas (op. cit., 1911, p. 805) and a kind of tabulation of contents taken over by Radin from Schmidt there is not even a mention of the whole first section of the book. It would seem that the reader's attention might at least have been directed to those pages which have or had an unobjectionable positive contribution to make.

Sapir, op. cit., p. 49.
Goldenweiser, op. cit., 1933, p. 147.
Boas, op. cit., 1911, p. 809. Boas, in contrast to the other authors cited, quotes the whole sentence from Graebner. I should like specifically to state that the remarks in this paragraph are not thought of as applying to this review by Professor Boas, which however much one may disagree with it in particulars, is a masterly treatment, easily the most significant criticism which has appeared in English.
possible." He said that it was impossible "over wide distances." Further
Goldenweiser and Herskovits, whose discussions appeared a number of
years after the appearance of "Völker und Kulturen" and other works
had been published, write as if Graebner's stand on this point fairly repre-
sented the school as a whole at the time of their writing, whereas Schmidt's
view is markedly different.  

Similarly, when Sapir writes

The notion of a culture stratum, composed of a large number of elements that are
technically independent of each other, journeying without great loss of content, as
though isolated in a hermetically sealed bottle, from one end of the world to the
other is unthinkable and contradicts all historical experience,

he is simply disregarding Graebner's many recognitions that elements are
modified in the course of transmission and that elements are often com-
pletely lost. Koppers has observed that Schmidt and Graebner never
dreamed that culture complexes found in Oceania were found in unaltered
form in South America and further states:

. . . a priori there is no objection to the fact that America is in possession of individ-
ual culture traits and culture complexes which are not found in the Old World
either in their entirety or in greater part.

And to write as Radin does:

There is something compelling in the effrontery with which Graebner lays down the
laws that govern culture growth, . . . a certain irritating pretentiousness . . .
or as Dixon does:

the adherents of this school are more reasonable and less arrogantly assertive of the
certainty of their conclusions than are the modern diffusionists,

hardly seems justifiable. It is true that in applications of the method in-
dividual writers do treat the Kulturkreise as if they were definitely proven
entities—just as physicists until recently wrote as if ether had a certain
objective existence, although they all realized perfectly that it was simply
a working hypothesis which seemed to fit the observed facts—which is

72 Lowie, loc. cit.
74 Cf. Schmidt-Koppers, op. cit., p. 64.
75 Sapir, op. cit., p. 49.
76 Cf. Methode, pp. 125–51 passim.
78 Method and Theory of Ethnology (New York, 1933), p. 79.
79 Idem, p. 73.
80 Dixon, op. cit., p. 183.
exactly the expressed attitude of our theorists to the Kulturkreise. Illustrations of "arrogance" and "effrontery" seem to be singularly lacking. On the contrary, the note that conclusions are tentative, dubious, is continually struck by Graebner, Schmidt, and Koppers.

No, these ethnologists are much more conscious of some of the problems inherent in their theory and practice than many American writers assume. For example, Dixon objects to Graebner's emphasis on Oceania as if it were a consideration which had never occurred to Graebner, whereas actually Graebner himself pointed out in "Methode" that he realized a certain one-sidedness was bound to result, but he felt it necessary to restrict himself in the main to phenomena in his own special field. He further says in his introduction that he does not think for a moment he has correctly answered all the problems of method he has discussed. He never refers to the method he proposes as the method of ethnology as his critics have imputed. He is primarily interested in making ethnologists conscious of methodological problems:

It is useful for every science to become self-conscious not merely about its nature, but also about the ways in which it may arrive at knowledge and about the limitations of that capacity.

He does not claim for his method a limitless objectivity nor does he assign to objectivity the only important rôle:

There is no absolute objectivity at all from the point of view of theory of knowledge, even to some extent of things we can perceive directly through the senses, and we must always thank the intuitive spirit for the great truths. Very true—but it is not at all a question of absolute objectivity in the philosophical sense but rather that the shaping imagination should derive the principles for its creative activity not unfettered, producing them purely subjectively out of its own psychological attitudes, conditioned in a hundred and thousand ways (for in that case even approximate general conclusions would be out of question) but rather that it should derive them out of the objects and problems which the science in question present.

82 Cf. Koppers, op. cit., 1931, p. 235. "... Die Ethnologie ihrerseits im grundlegenden Kulturkreisfrage keineswegs überall zu einem definitivem Resultat gekommen ist" (op. cit., p. 237). He admits that the original sociological structure in individual Kreise has not yet been satisfactorily determined in every case. Cf. also individual works of younger writers, e.g., Flor, Haustiere und Hirtenkulturen (Wiener Beiträge zur Kulturgeschichte und Linguistik, Vienna, 1930, Vol. 1, pp. 1-238).
83 Graebner, op. cit., p. 5.
84 Idem, pp. 2-3.
As far as intuition goes, I am the last to deny its fundamental significance... But what after all is the criterion of truth for the insights won by intuition?—the correspondence with the facts and with the fundamental principles of systematic knowledge.87

The Kulturkreislehre has been harshly criticized from the start both in the German speaking world and elsewhere, and its defenders on more than one occasion have overcompensated by a dogmatic and extreme reiteration of their views.88 Therefrom have arisen some regrettable misconceptions. Likewise a not uncommon turgidity of expression (particularly in Graebner) has occasioned some understandable misunderstandings. This is especially true in regard to the problems of psychological interpretation and “independent invention.”

Boas writes: “This exclusion of the psychological field seems to me to give to the whole ‘Method’ a mechanical character.”89 It is perfectly true that Graebner proposed to regard ideas or objects as comparable if convincing outward similarities apparently unconditioned by limitations of material or function could be established.90 But he proposed to do so only as a means of attaining a “first approximation.” He assumed that for purposes of a single problem (distribution) the question of the psychological attitudes of the culture bearers toward an object or institution could be largely disregarded91 Indeed he felt that the only hope of getting a relatively objective answer to this single question was to avoid the complex-

87 Op. cit., pp. 5–6. Cf. Goring, The English Convict (abridged edition, London, 1919), p. 117: “... We must pass to some extent from the strict and narrow confines of ascertained certainty into the wider latitudes of theory, where the laws which govern the imagination in the construction of ideas are more paramount than those which regulate the intellect in the analysis of facts. The interpreting of facts involves operations different and distinct from those by which facts are established—it involves work of synthesis and exposition, not analysis and discovery.”

88 Schmidt himself acknowledges the harsh and controversial tone of some of his earlier writing (Ursprung, Vol. 6, p. vii). There is this background to the quasi-emotional attitude which I think has undoubtedly been common among American anthropologists toward the Kulturkreislehre. But I feel sure that it is in part related to what Schmidt has called “the ethnological Monroe doctrine.” American scholars do sometimes seem to project doctrines of “splendid isolation” and “America for Americans” into the remote past. We also have to reckon perhaps with the associated fact that a number of the leaders of American anthropology in the last generation have been in some sense culturally uprooted individuals.

89 Boas, op. cit., 1911, p. 805.

90 Probably it is correct to say that Graebnerians in practice have too often disregarded “the principle of limited possibilities.” Cf. Goldenweiser, op. cit., 1933, p. 35 ff.

91 Cf. Koppers, op. cit., 1929, p. 697: “... accidental trait-complexes act the same or at least a similar part with logical trait-complexes; in fact for discovering and ascertaining relationships, the former may be more decisive than the latter.”
ities and dangers of psychological interpretations. But he in no sense assumed or maintained as a matter of theory that the significance of a cultural fact was exhausted when its distribution had been ascertained. As the concluding paragraphs of the "Methode" make clear, Graebner fully realized that psychological insights and interpretations were indispensable to ethnology. In his "Ethnologie" he says that the uniformities of "cultural process" are "laws of mental life," and "that their scientific and methodical study is possible only from the psychological point of view."\(^\text{92}\)

Boas has recently written

the theory minimizes the possibility of the independent origin of similar ideas. Graebner, with whose name the theory is particularly associated denies its existence.\(^\text{93}\)

Now to say that the followers of the Kulturkreislehre minimize the possibility is a fair statement, but it is untrue to say that Graebner denies it.\(^\text{94}\) In fact, it is interesting to observe that in 1911 Boas wrote "which Graebner practically denies."\(^\text{95}\) The present position of the Kulturkreislehre with respect to "independent invention" is summarized by Koppers:

The culture historians do not wish to deny entirely the possibility of an independent invention of identical or similar culture phenomena at different places; they are, however, of the opinion, on the basis of observations and experiences at present available, that we do not so very often have to reckon with this possibility.\(^\text{96}\)

He further states that it is held that in individual cases either independent

\(^{92}\) Ethnologie (Kultur der Gegenwart, Leipzig and Berlin, 1923), p. 582. It would be justifiable to state that the followers of the culture-historical school (again, in practice) have paid scant attention to what Spier has called "the culture trait as a symbol of personal behavior" (review of "The Building of Cultures," American Anthropologist, Vol. 31, 1929, p. 144). Cf. also R. Redfield, The Regional Aspect of Culture (American Sociological Society, Vol. 24, 1929), p. 38: "The cultural fact, however, is not merely the form of the tool or of the overt behaviour, but rather the significance that tool or behaviour has to the people who use or perform it." Up to this point also the phenomenon of cultural configuration has been largely neglected. There are some references to this important aspect of the problems of culture. Cf., e.g., Schmidt, High Gods in North America, p. 13. But the influence of cultural configurations has hardly been sufficiently taken into account in the systematic theory of the Kulturkreislehre. On the other hand, the Kulturkreislehre in no sense shares Spengler's notion that the dynamics of culture are more or less independent of the culture carriers nor Spann's somewhat similar Totalitätsbegriff.

\(^{93}\) Boas, op. cit., 1930, p. 104.

\(^{94}\) Cf. Graebner, op. cit., pp. 95, 97, 105, 107, etc.; cf. also Lips, op. cit., p. 324.

\(^{95}\) Boas, op. cit., 1911, p. 807.

invention or diffusion requires to be demonstrated or shown as probable. It cannot be regarded as methodologically sound to assume either. This seems correct; for to insist that independent invention must be assumed as a fact until diffusion is demonstrated bears, as Kroeber has pointed out, a palpable logical affinity to the exploded biological dictum of spontaneous generation.

Boas, however, very correctly speaks of the invention of "ideas." This is an emphasis stressed by Graebner, Schmidt, and the rest; a fact missed by some of their critics who have attempted to refute them on the basis that it would be absurd to believe that maize, the domesticated llama, etc., had been "invented" in the Old World. But all that Graebner, Schmidt, Koppers have ever maintained was that the idea, the principles of the domestication of plants and animals had been brought from the old World. Similarly, Flor makes it perfectly plain that he does not think that the distribution of all species of dogs can be traced to a single epoch or area.

Now let us turn briefly to the Kulturkreislehre as viewed by Professor Radcliffe-Brown and Professor Malinowski. Here also, it seems to me we may observe a certain rather consistent unfairness. Malinowski, for example, expresses himself with a not unamusing maliciousness:

The disputes of historical anthropologists (for there is little concensus between

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97 Kroeber, op. cit., 1930, p. 686, following Rätzel.  
99 Flor, op. cit.  
100 Here, as previously, I disavow all pretense of presenting positive viewpoints in their wholeness. These are not here the center of our interest. While the quotations here cited would of course give only a garbled impression of the constructive attitudes of these anthropologists, it seemed justifiable for our purposes to take certain printed remarks out of their context and to examine them as more or less isolated judgments. I shall not even present a balanced picture of their criticisms of the Kulturkreislehre, for many of their points have already been dealt with. I should like, however, to call attention to Malinowski's contention that the framing of the issue between diffusion and independent invention rests on false premises, since really these two categories grade subtly into each other (Culture, The Diffusion Controversy, New York, 1927, p. 28 ff). But the necessity for and use of classification is hardly to be denied because we recognize that any categories dealing with a continuum have a certain artificiality. "Diffusion" and "independent invention" are useful polar concepts. They should, of course, be recognized as such and not misused.  
101 As de la Boullaye has remarked, the rivalry of the different schools of anthropological thought "les entraine d'une part a représente de façon fort incorrecte la pensée de leurs adversaires et, d'autre part, semble les empêcher de développer logiquement leurs propres conceptions."  
102 Might not Malinowski equally correctly have added: "Nor is there among functional anthropologists"(?) See Radcliffe-Brown (Man, Vol. 35, No. 48, 1935).
Radcliffe-Brown likewise seems to imply that the theory and method of all ethnologists who profess an interest in history and in the phenomena of diffusion may be dealt with and disposed of as a unit, and again, disposed of by a question-begging epithet: "conjectural history," "conjectural reconstruction," "hypothetical history." Surely it is unjustifiable to lump all the results of historical ethology together. For me, that a people who spoke a language closely related to the language we call Navajo came into the American Southwest after a culture which we call Pueblo was already considerably developed is as sound a basis of departure as that Coronado's expedition was motivated by a lust for gold—or almost any assertion founded upon documentation by eye-witnesses. Radcliffe-Brown himself has conceded that the connection of Madagascar with Indonesia is more than a conjecture. So would it not be more scientific to avoid the sweeping damning with a label?

It is true of course that the theoretical structure of the Kulturkreislehre rests upon assumptions, at least some of which may legitimately be questioned. But when Malinowski writes:

... Culture is not contagious! It has neither been invented nor diffused, but imposed by the natural conditions which drive man upon the path of progress with inexorable determinism.

he is making most daring generalizations which require at least as much proof as the converse. And Radcliffe-Brown does not write of "conjectural sociological laws." And yet that there are sociological laws is also an undemonstrated hypothesis. Further, Radcliffe-Brown's methodology for

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105 Radcliffe-Brown seems recently to have realized himself that there was something objectionable in these adjectives and suggests using "circumstantial" instead (Kinship Terminologies in California, American Anthropologist, Vol. 37, 1935, p. 533).
106 Radcliffe-Brown has recognized this on another occasion (The Methods of Ethnology and Social Anthropology, South African Journal of Science, 1923), e.g., p. 140.
107 For further related considerations see Lesser, op. cit., 1935, especially pp. 388–91.
109 Radcliffe-Brown (On the Concept of Function in the Social Sciences, American Anthropologist, Vol. 37, 1935, p. 402) says that a scientific law is a generalization that has been verified or demonstrated by a systematic examination of evidence afforded by precise observations systematically made. Even so, it has yet to be demonstrated that satisfactory generalizations of such a kind can be made from cultural phenomena. Kroeber (op. cit., 1935, p. 562) has
establishing them seems to imply as many latent assumptions (undemon-
strated inductively or otherwise) as does the methodology of the Kul-
turkreislehre. Finally, Radcliffe-Brown appears to have insufficiently realized
what Lesser has well put:

Inference, however, is a mode of thinking which is basic not only to the recon-
struction of history, but to the derivation of functional relations in the present as
well.\footnote{Inference, however, is a mode of thinking which is basic not only to the recon-
struction of history, but to the derivation of functional relations in the present as
well.}

In some recent statements of Radcliffe-Brown and Malinowski\footnote{In some recent statements of Radcliffe-Brown and Malinowski, at
least three significant premises seem implicit: (1) that the Kulturkreislehre
postulates that the only end of ethnology is the tracing out of a sequence
of events; (2) that "conjectural history" and "the functional study of
observed that Mrs Hoernlé's cited examples of cultural laws are really only descriptive
summaries of uniquely occurring phenomena. (I do not mean to imply that Mrs Hoernlé and
Professor Radcliffe-Brown have precisely the same conception of "laws."\footnote{For
example, our picture of non-objective aspects of any culture comes to us only after
it has passed through the subjective lens of the mind of the ethnographer. In addition, even
"functional" field workers rely necessarily for detailed information about complex cultural
attitudes upon a limited number of informants. As Sapir has recently written, the anthropologist
"always hopes that the individual informant is near enough to the understandings and tra-
ditions of his society to report them duly, thereby eliminating himself as a factor in the method
of research!" (Cultural Anthropology and Psychiatry, Journal of Abnormal and Social Psy-
chology, Vol. 27, 1932, p. 234). In short, the data Radcliffe-Brown will use for his inductive
generalizations are twice subjectively conditioned (or thrice if an interpreter is necessary!).
He assumes, in effect, that these factors may be disregarded.\footnote{For example, our picture
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society” are mutually exclusive alternatives; (3) that “the nature of culture”\textsuperscript{113} can be understood simply by studying the interrelations and functions of the various aspects of a given culture at a given time and place and then studying these data comparatively. Let us consider each of these propositions in turn.

1. The *Kulturkreislehre* in no sense assumes as a matter of theory that the task of ethnology is ended when the distribution of culture elements and culture complexes in space and time has been ascertained with the greatest possible fullness and exactness. On the contrary, Koppers has recently written:

No, a psychological, philosophical penetration of fact, too, is part of it, but must follow not precede our historical researches.\textsuperscript{114}

2. Radcliffe-Brown charges\textsuperscript{115} that the various historical schools have taken on the nature of cults. This is unfortunately true to some extent at least. But do not he and Malinowski by their implications of mutually exclusive alternatives tend to create the cult of functionalism? Even if we agree with Malinowski (which we can hardly do without important qualifications) that the *Kulturkreislehre* concerns itself only with disembodied fragments of culture, this is not equivalent to saying that its approach is useless. For, to carry Malinowski’s biological analogy further, anatomy is important as well as physiology.\textsuperscript{116} Indeed one wonders how completely function can be understood until static forms have been minutely dissected out.\textsuperscript{117}

In this respect at least, the leaders of the *Wiener Schule* reveal a more sympathetic tolerance of an alien conceptual scheme. Menghin declares that the *Kulturkreislehre* and the *Strukturllehre* can well exist alongside each other, as two aspects of scientific procedure working at different levels.\textsuperscript{118} Schmidt has recently praised the work of the functionalists and

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\textsuperscript{113} See the article Social Anthropology (Encyclopaedia Britannica, 14th ed.), p. 864.

\textsuperscript{114} *Op. cit.*, 1929, p. 698.

\textsuperscript{115} *Op. cit.*, 1931.

\textsuperscript{116} Indeed one may also say that the kind of “cultural histology” which Bateson appears to be developing also promises to be very fruitful.

\textsuperscript{117} Cf. Kroebcr, *op. cit.*, 1931, especially pp. 155–56. Radcliffe-Brown (*op. cit.*, 1935, pp. 396–97) seems to construe his analogy too narrowly and to be unwilling to follow it logically through. For example, the physiologist must also consider the products of structures, e.g., blood sera. Such products can be and are studied as such. Similarly surely with the products of cultural function—artifacts, “traits” abstracted temporarily from their context.

\textsuperscript{118} Diskussionsbemerkung zum Vortrag von Prof. Dr Krause (Mitteilungen der Anthropologischen Gesellschaft in Wien, Vol. 59, 1929, pp. 265–69).
of the somewhat similar \textit{Strukturlehre} in giving us more satisfactory pictures of individual primitive cultures.\textsuperscript{119}

3. The followers of the \textit{Kulturkreislehre} readily admit the value of studying the function and interrelations of different aspects of culture. Nor do they deny what Goldenweiser has called "the interpretative illumination from the psychological level." But they do deny that it is possible "to discover the general laws of function for human society as a whole until the temporal and spatial relations of the data upon which such generalizations must be founded have been worked out as exactly as possible. Here many anthropologists of other schools of thought would agree with them. Sapir, for example, has recently written:

When the cultural anthropologist has finished his necessary preliminary researches into the overt forms of culture and has gained for them an objectivity of reference by working out their forms, time references and geographical distribution, there emerges for him the more difficult and significant task. . . .\textsuperscript{120}

In the same trend Professor Benedict:

\begin{quote}
Any clear understanding of the processes of cultural integration must take its point of departure from a knowledge of the facts of diffusion.\textsuperscript{121}

The difficulty of naïve interpretations of culture in terms of individual behavior is not that these interpretations are those of psychology, but that they ignore history and the historical process of acceptance or rejection of traits. . . . At different points in the interpretation of cultural forms both history and psychology are necessary; one cannot make the one do the service of the other.\textsuperscript{122}
\end{quote}

Dr Lesser has expressed the crux of the matter very succinctly:

\begin{quote}
The processes which control events lie imbedded in time as well as place, hence the determining conditions and the associations and connections of events are in the past as much as (if not more than) in the present. . . .

The conditions which functional investigation must take account of can be generalized as historicity—the fact that institutions, customs, beliefs, artifacts, have careers in time, and that their form and character is molded more by what has happened to them in the course of that history than by what particular things they occur associated with at any one time.\textsuperscript{123}
\end{quote}

Without doubt much which is useful can be learned simply by observing


\textsuperscript{120} The Emergence of the Concept of Personality in a Study of Cultures (Journal of Social Psychology, Vol. 5, 1934), p. 413.

\textsuperscript{121} Patterns of Culture (New York, 1934), p. 242.

\textsuperscript{122} Ibid., p. 232.

and reflecting upon the relations of individuals to each other and to their culture within a limited time span, but before we generalize upon data so gathered, must we not endeavor to discover whether the particulars which we would use to establish our "law" are to be understood in terms of historical movements or contacts rather than in terms of general principles which would tend to cause individuals or groups of individuals to react to certain environmental stimuli in a rather uniform way? Must we not usually consider the possibility that if the historical experience of a people had been otherwise their behavior patterns at present might be significantly different?

We must distinguish, of course, between the "explanation" of events, origins, processes, configurations. History is probably a conditioning determinant to a different degree in these categories, but is, I think, relevant to all. Malinowski and Radcliffe-Brown themselves insist that custom and custom, and custom and artifact are reciprocally conditioning. How then can we even get at the full psychological context of a given cultural fact unless we know something about at least its proximate relations, and these can often only be followed out after connection in time and space have been determined. Professor Lowie in his "Primitive Religion" has given fully convincing illustrations of the psychological unintelligibility of particular situations torn out of their historical setting. How can the central issue as to whether the union of culture elements is due to an internal functional interrelation or to the external accidents of history (or, as seems most probable, to both) be decided on any other level than that of opinion unless we have worked out as far as possible the times and places in which certain groups of elements have been found together?

As MacIver has shown, one essential difference between the social and the physical sciences is that the former are concerned with an inner and an outer system of reality, the latter with an outer alone:

Every social situation consists in an adjustment of an inner to an outer system of reality. The inner system is a complex of desires and motivations. . . . Each system, the inner and the outer, is coherent in itself and the two together form also a single coherence. . . . The interpretation of a social phenomenon is never more than approximate. It depends on an understanding of the relation of inner to outer, an understanding which demands experience as well as knowledge, insight as well

\[134\] For a discussion of the relevance of historical reconstruction to process and dynamics in a particular situation (with views contrary to those put forward above) see Spier, Problems Arising from the Cultural Position of the Havasupai (American Anthropologist, Vol. 31, 1929), especially pp. 218–22.

\[134a\] New York, 1924.
as calculation. . . . An explanation in this field is always a partially verified hypothesis and there is no such thing as a complete verification. The idea of complete verification depends on an over-simple concept of induction. A negative instance does not necessarily disprove a conclusion, nor does any quantity of positive instances completely prove it. Because insight is necessary, hazard is always present.\textsuperscript{125}

Radcliffe-Brown and Malinowski appear to wish to neglect the outer causal system.\textsuperscript{126} For, as MacIver has observed:

To tackle the subject of causation we must, in short, study society genetically. The time dimension is seriously lacking in sociological studies today, and our presentation of social change is apt to be merely a series of successive pictures as lacking in the dynamic of real life as those we see upon the screen.\textsuperscript{127}

And the \textit{Kulturkreislehre} seems in general to be too preoccupied with the purely external aspects of culture change,\textsuperscript{128} tending to overlook possibilities of immanent causation. There seems need for still greater incorporation into \textit{Kulturkreis} methodology of the systematic consideration of the mechanisms of culture change;\textsuperscript{129} especially internal development\textsuperscript{130} as manifested in attitudes of the individual and of a society as an integrative system. These are reflected especially in the sentiments and non-logical acts of individuals and of groups, and are also mirrored in subtle nuances of semantics and other aspects of linguistic behavior. These are all aspects

\textsuperscript{126} Largely on the ground that the outer or historical system cannot be satisfactorily established in the case of non-literate people. But, as has been shown above, there is evidence that some "circumstantial history" is valid enough to be used with confidence. In any case it is no solution of a problem to operate as if it did not exist.
\textsuperscript{128} Graebner actually also conceived an inner and outer causal system in cultural phenomena, but states the problem much less clearly; cf. Methode, p. 161 ff.; cf. also Wheeler, \textit{op. cit.}, esp. p. 192. Graebner's position is that not merely does starting with the objective cultural phenomena reduce the possibilities of erroneous psychological interpretation, but that also the prior dealing with the outer system tends to establish principles of interpretation suitable to the phenomena in question.
\textsuperscript{129} Although Menghin (\textit{op. cit.}, 1929, p. 266) asserts that since the \textit{Kulturkreislehre} is \textit{Wissenschaft} (in Strzygowski's sense) its just end is to answer the questions: what? when? where? (typology, chronology, chorology), and the question of the causes (at least of culture change) does not present itself. That is the problem of the \textit{Entwicklungsforschung}.
\textsuperscript{130} Krause has strongly presented this criticism; see Kulturwandel und Volkstum (Mitteilungen der Anthropologischen Gesellschaft in Wien, Vol. 59, 1929, pp. 247–65), especially p. 249. The \textit{Strukturlehre} has arisen very largely out of criticisms of the \textit{Kulturkreislehre} with regard to this and related problems by A. Haberlandt and Krause; cf. Völkerkunde, Anthropologie, Ethnobiologie (Ethnologische Studien, Vol. 1, No. 3, 1931, pp. 135–66). See also Ploetz, Sozial Anthropologie (in Kultur der Gegenwart, Leipzig and Berlin, 1923).
of culture to which the theory of the *Kulturkreislehre* is only beginning to
give its attention, although such considerations are decidedly pertinent to
the presuppositions upon which the treatment of the data of distribution
have been based.\(^{131}\) For as Boas has remarked, the problem must be
envisioned not as "the simple mechanical aspect of transmission" but must
be formulated to take account of "the complex social conditions that admit
transmission and that bring about internal changes."\(^{132}\)

The insights afforded by both "historical" and "functional" approaches
are quite indispensable. It cannot be denied that sound historical recon-
struction must be in accord with inferences drawn from the observation
of living societies. But this is not to say that the relationship between
ethnology and social anthropology is to be conceived, as Radcliffe-Brown
has alleged, as a one-sided dependence of ethnology upon social anthro-
poLOGY.\(^{133}\) On the contrary, their relationship is certainly that of a complex
mutual interdependence. Functional field studies\(^{134}\) help to provide both
the raw data and the principles which the *Kulturkreislehre* (as one type of
the historical approach) must use in its reconstructions. The *Kulturkreis-

\(^{131}\) Cf. Sayce, Various Forms of Culture Spread (Congrès International des Sciences An-

\(^{132}\) Lebzelter (*loc. cit.*) points out that, in so far as environment did not prohibit their adop-
tion, most culture elements would by this time have spread over the whole earth, were it not
that given men and groups of men took an un receptive attitude toward given traits. Hence,
he says, we must think of the phenomenon of diffusion as "*ein Elektionsprozess, kein Selekti-
onsprozess.*" He further says: "Whether the transference takes place in space and time or
only in time, in any event it is a case of many individuals taking over an object, attitude, or
idea and handing it on unaltered, and of a not small number of individuals who modify it and
spread it further. *Thus arise geographical and historical chains of ideas with similar content.
The central task of ethnology is the tracing of these chains of ideas and their realization in space
and time. . . . A culture which has acquired its form through different influences, accommo-
dations, developments, etc., is not to be regarded as an agglomeration of various elements
which can be disposed of simply through a routine analysis of the sum of its parts." There are
other evidences that such considerations are being more fully reckoned with within the


\(^{134}\) Radcliffe-Brown has held (*op. cit.*, 1923, and elsewhere) that since the aims of ethnology
and social anthropology are different, they will require separate sets of field investigations.
While it seems probable that the general theoretical standpoint of the worker inevitably in-
fluences the content of field studies, yet it is noteworthy that Schmidt and Koppers maintain
(*op. cit.*, 1935) that the ideal of field work is a full objective description from all angles and that
they express themselves as pleased with the accounts based upon "functional" ethnographical
studies. In justice to Radcliffe-Brown it should be pointed out that recently (*op. cit.*, 1935) he
himself has written, "The two kinds of explanation do not conflict, but supplement one an-
other." Likewise (*op. cit.*, 1931, p. 165 and elsewhere) he recognizes that "comparative soci-
ology" has "diachronic problems to deal with." Nevertheless his earlier statements are still
read and give rise to misunderstanding—hence it seemed proper to discuss them.
lehre in its turn might be thought of (ideally at least) as providing the functionalists with cultural data established in time and space, upon which generalizations or "sociological laws" must be based. And the two methods decidedly overlap, as Hoernlé has pointed out, in the special field of culture change.

The systematic formulations of the Kulturkreislehre and of Radcliffe-Brown and Malinowski are too narrow. As Professor Boas has repeatedly insisted, we must distrust any attempt to solve all the problems of culture with any relatively simple schema. It may well be that it is a justifiable convenience to consider either the facts of distribution or the facts of functional interrelation in a single time span as a closed system temporarily. But we must not allow ourselves to forget that such a universe of discourse can be no more than an heuristic expedient, for it does violence to the intricate realities. The two conceptual schemes under discussion by no means exhaust the possibilities, though they probably represent two basic generic approaches. The more conceptual schemes (a conceptual scheme, to be meaningful, must, of course, have a certain minimum of concord with the subject matter and with the wider principles of organized thought) consistently followed out, the better it will be for anthropology. There must constantly be finer discriminations of detail, both in particulars and in methodological principles, if anthropology is to leave what Whitehead has called "the half-way house of classification."

The assumptions of the Kulturkreislehre are not invulnerable, and like its critics, its defenders use some ethnological concepts uncritically and others with ambivalence of reference. We may justly question whether the method and theory of the Kulturkreislehre as at present formulated is likely to give a satisfactory answer to all the questions of cultural anthropology.

But the services of the Kulturkreislehre are by no means limited to its contributions to the problems of distribution and diffusion. It has also

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138 Menghin asserts that the Kulturkreislehre does not, as a matter of theory, deny the existence of laws (op. cit., 1929, p. 267).


137 "Reality is a thing of infinite diversity, and defies the most ingenious deductions and definitions of abstract thought, nay, abhors the clear and precise classifications we delight in. Reality tends to infinite subdivisions of things, and truth is a matter of infinite shadings and differentiations" (Dostoevsky, House of the Dead).

138 "... Cultural understanding is a manifold and ... the more content we put into it, the profounder it becomes. ..." (Lesser, op. cit., 1933, p. 336).

139 On these services see Kroeber, Diffusion (Encyclopaedia of the Social Sciences, Vol. 5, New York, 1931), p. 142.
compelled anthropologists to think about many methodological problems, and it has likewise maintained a healthy insistence upon strenuous criticism of sources and attention to the written sources as well as the immediate results of an investigator's field work. Most of all, they have defended the thesis that no science can be satisfied with a card index of perceptual data. In the words of MacIver:

The finding of hidden phenomena is a very useful and very necessary occupation. . . . But when the facts are gathered and discovered, when they are disentangled and identified, when they are counted and measured, the real task of the scholar is not ended. . . . The true scholar is no harvester of facts which, when gathered, are stored in his barns an occasion for thanksgiving and a source of livelihood till the next crop comes in.

In this country we have, it seems to me, often been content to collect and to cull, to remain on the purely descriptive level. As Tozzer has recently remarked this type of research is physical whereas research should be intellectual. It is of quite fundamental importance that we should have the most complete and accurate accounts of the greatest possible number of cultures, but we can hardly agree with Radin that this is "the only question of importance." Perhaps the central reason for careful examination of the Kulturkreislehre is that it attempts to provide a schematization for the archaeological and ethnological facts of the whole world—at a time when the recognition that even very early peoples were no respecters of continents is being forced upon us. The followers of the Kulturkreislehre have at least resolutely devoted themselves to the true task of scholars: they have endeavored to ferret out and establish unperceived relationships between facts, and we will be unwise to condemn them too austerely if the relations they think to have discovered are not always approved in detail by their fellow scholars.

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140 Cf. Bernheim, op. cit., p. 183: "Geist ohne Methode schädigt die Wissenschaft nicht minder als Methode ohne Geist."


144 "The only question of importance, then, is to discover some means whereby we can best obtain a complete account of an aboriginal culture" (op. cit., 1933, p. xi).
IN January and February, 1935, a six weeks’ expedition was undertaken by Mr Bernard Bevan, Mr Jack Rickards, and the writer through north- and southeastern Chinantec territory, for the purpose of obtaining linguistic and ethnological material about this very little known region.

The Chinantec tribe inhabits a portion of northern Oaxaca; their northeastern boundary closely coinciding with that between the States of Oaxaca and Vera Cruz. On the west, they are bounded by the Mazatec,
on the southwest and south by the Zapotec, and on the southeast by the Mije.

The expedition entered Chinantec territory by the Valle Nacional via Tuxtpec, and passed through Chilatepec, Jacatepec, and Ozumazin. From the Llanos de Ozumazin we entered the southeastern Chinantec area proper, visiting Tepinapa, Jocotepec, Lachixola, Lacova, and Lalana; then returning to Tepinapa, passed through Toabela, Lovani, and Petlapa. After this we continued into Zapotec territory at Villa Alta, Comaltepec, and Choapam; into the Chinantec again at Teotalcingo; into Mije at Totontepec; and finally reached Oaxaca via Yalalag and Mitla.

This southeastern Chinantec area forms a well-defined linguistic territory, quite distinct from other Chinantec areas such as that of the Valle Nacional or that of Yolox, and to facilitate the naming of these regions the actual Chinantec designation¹ might be employed, viz.: Wahmi¹ for the southeastern branch, Hû mé¹ for the Valle Nacional branch.

THE CALENDAR

Up to the present time, the only known calendars still in use in Central America are those of certain Maya tribes in Chiapas and in Guatemala. The fortunate discovery of a calendar still in use amongst a non-Maya tribe is therefore of some importance.

The Chinantec calendar was first discovered on February 9th in the small and remote village of Lachixola, whose inhabitants, with the exception of three or four men, can speak no Spanish.

The calendar was obtained from Pedro Perez, Secretario Municipal, and due credit must also be given to Mr Bernard Bevan whose clever in-

¹ The system of transcription employed is the French system of l’Institute d’Ethnologie de l’Université de Paris, as adapted by Prof J. Soustelle to the Otomi.

k palatal without aspiration
j j in English “journey”
l lateral sonant 1 with edges of tongue more firmly against the alveoles
h like ch in German “ach”
y English y
ö German ö
a open a, in German “lachen”
ɛ open e, in English “air,” in German “Ähre”
ɔ open o, English a in “law”
^ nasalisation
/ nasal aspiration previous to m or n

¹ strong compression previous to issuing of sound. This important moaning sound is somewhat like that in French “onze” and occurs frequently in Otomi and Matlaltzinca
- short quantity
- long quantity
ö even low tone
ø even high tone
' stress (accent)
' glottal stop
Weakly pronounced sounds are printed above the line.
terrogation for that particular date initiated the finding of the full list of months. Later on, two further and almost identical versions of the calendar were obtained at Petlapa and Teotalcingo.

The Chinantec calendar consists of eighteen months each of twenty days, and five extra days or nemontemis. The three versions, apart from dialectic differentiations, correspond exactly in nomenclature and in sequence. They are as follows:

<table>
<thead>
<tr>
<th>Lachixola</th>
<th>Petlapa</th>
<th>Teotalcingo</th>
</tr>
</thead>
<tbody>
<tr>
<td>Feb. 9th</td>
<td>Feb. 14th</td>
<td>March 1st</td>
</tr>
<tr>
<td>1. hi 'luá'</td>
<td>hi 'luá'</td>
<td>hi 'luá'</td>
</tr>
<tr>
<td>2. hi ja-h</td>
<td>hi ié</td>
<td>hi ié</td>
</tr>
<tr>
<td>3. hi ka ja-h</td>
<td>hi ko je-h</td>
<td>hi ko je-h</td>
</tr>
<tr>
<td>4. hi hu-h</td>
<td>hi hu</td>
<td>hi hu</td>
</tr>
<tr>
<td>5. hi hu</td>
<td>hi hou</td>
<td>hi hú</td>
</tr>
<tr>
<td>6. hi nó</td>
<td>hi nó</td>
<td>hi no</td>
</tr>
<tr>
<td>7. hi ló</td>
<td>hi ló</td>
<td>hi ló</td>
</tr>
<tr>
<td>8. hi kúa</td>
<td>hi kúa</td>
<td>hi kúa</td>
</tr>
<tr>
<td>9. hi ló-h</td>
<td>hi li</td>
<td>hi li-h</td>
</tr>
<tr>
<td>10. hi i</td>
<td>hi ja-h</td>
<td>hi ja-h</td>
</tr>
<tr>
<td>11. hi ri-kulh</td>
<td>hi ru kúh</td>
<td>hi ru kúh</td>
</tr>
<tr>
<td>12. hi mo-h</td>
<td>hi mo</td>
<td>hi mo</td>
</tr>
<tr>
<td>13. hi mûé (i)</td>
<td>hi mûe (a)</td>
<td>hi mûe (a)</td>
</tr>
<tr>
<td>14. hi nyö-t</td>
<td>hi nyö-t</td>
<td>hi nyö-t</td>
</tr>
<tr>
<td>15. hi ta nyi-t</td>
<td>hi ta nyi-t</td>
<td>hi ta nyi-t</td>
</tr>
<tr>
<td>16. hi ta-ja'</td>
<td>hi ta ja'</td>
<td>hi ta ja'</td>
</tr>
<tr>
<td>17. hi yq</td>
<td>hi lié'</td>
<td>hi lié'</td>
</tr>
<tr>
<td>18. hi lié (5 days)</td>
<td>hi nó (5 days)</td>
<td>hi nú-</td>
</tr>
</tbody>
</table>

Explanations of the Calendar Given at Teotalcingo

1. Tiempo de sembrar todavía. [Lachixola: tiempo de rozando.] (Planting season still on.)
2. Tiempo de no sembrar; descanso; limpiar la milpa. (Do not plant. Rest. Cleaning the fields.)
3. Arozando las milpas; tumbar arboles. (Rooting out fields. Cutting down trees.)
4. Sembrar. (Sowing-time.)
5. Mero tiempo de sembrar. (Actual sowing-time.)
6. Mero tiempo de sembrar. (Actual sowing-time.)
7. Dejar crecer la milpa. (Letting fields grow.)
8. Limpiar milpa. (Cleaning fields.)
9. Limpiar milpa. (Cleaning fields.)
10. Se da frijol en Petlapa y Lalana. (Beans are grown in Petlapa and Lalana.)
11. Se da frijol en Petlapa y Lalana. (Beans are grown in Petlapa and Lalana.)
12. Tiempo de piscar. (Time for picking.)
13. Tiempo de sembrar picante y frijol. (Planting of chile and beans.)
14. Tiempo de llovisna; no trabajan. (Rainy season. Field-work suspended.)
15. Primer tiempo de sembrar tonamil.¹ (1st season of tonamil.)
16. Segundo tiempo de sembrar tonamil. (2nd season of tonamil.)
17. Tercer tiempo de sembrar tonamil. (3rd season of tonamil.)
18. Se siembra todavía y da planta. (Still planting and plants begin to grow.)

— Tiempo de sembrar: mes de febrero. (Planting season: month of February.)

At present [March 1, 1935] we are in the season of hi ʝe.

In these three towns, the Chinantec year was said to begin on February 10th, and the five nemontemi or closing days of the year to end on February 9th. However, a later investigation revealed that in the Lalana region nearby, the Chinantec year commences during the latter part of December (the precise date being variously estimated), with the nemontemi falling inside and not at the end of the year as one would expect.

Although the second table above is not to be taken as a literal translation of the calendar names, it shows the seasonal occupations. The calendar, at the present day, is employed for agricultural purposes only, the names of the months being time-bearers or indicators for the sowing and reaping of crops, etc.

The word hi1, prefixed to each month, means "period," "season," or "time" (tiempo, estacion) and the veintena or twenty day cycle is named kya³muu (gye³/муу) meaning twenty days.

Since this first expedition, a second expedition was made,² and seven

¹ Tonamil, a kind of second crop.
² This second expedition, through the good offices of Dr Alfonso Caso and Ing Pedro Sanchez, was sponsored by the Pan American Institute of Mexico City, which will publish later all the material so far obtained in the Chinantec region.
Scenes in Chinantec territory. Upper, Type of Chinantec church and village, built on levelled off promontory (Lovani); Lower, Houses with thatched palm roofs (Lovani).
Chinantec women. **Upper Left**, Showing hairdress: unmarried woman on the left has hair coiled with red wool, married woman on the right (Petlapa); **Upper Right**, Woman fetching water (Toabela); **Lower**, Women with white huipil and red skirts, called chiapaneco (Lalana).
Chinantec types. **Upper**. Group of Chinantec men with red handkerchiefs; baskets are made in Teotaltzingo (Toabela); **Lower Left**, Woman spinning cotton using a wooden spindle (Toabela); **Lower Right**, The man from whom the first calendar was obtained (Lachixola). (Photograph by J. Sturken.)
further versions of the calendar were obtained in this region, the sequence of the months being in every case the same.

Unfortunately, our search for the twenty day names constituting each month was fruitless: no trace or memory of them appears to survive, but for the first time, the precise meaning of the words in the calendar was discovered.

To only two of the month names can be attached an ancient astronomical or mythological background, namely the sixth month, hî nâ (June) and the sixteenth month, hî ta nyi’ (end of December). The former means: “At noon, when the sun is in its zenith, she stands still for a moment,” and the latter: “A thorn or spike thrusts itself into the face of the sun, preventing it from further fall.”

With the publication of these ten versions of the calendar together with the linguistic material collected, a better appreciation of the value of the Chinantec calendar compared with existing calendars and those known through historical sources may be expected.

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A SUMMARY OF JICARILLA APACHE CULTURE

By M. E. Opler

GEOGRAPHICAL LOCATION

The country which the Jicarilla Apache claim as formerly their own includes the central and eastern portions of northern New Mexico and the adjoining portion of southern Colorado. These Apache recognized the Arkansas River as their northern boundary, the Canedian River as their eastern limit, the region around the present site of Mora as the southern outpost, and a line extending north and south from where Chama now stands as their boundary to the west. That part of their territory to which they confined their actual home sites lay between the thirty-sixth and thirty-seventh degrees north latitude, extending no farther east than the present site of Springer and to Tierra Amarilla on the west.

CULTURAL POSITION

It is impossible to identify Jicarilla culture completely with any one of the areas to which North American Indian tribes are conventionally assigned; Jicarilla cultural allegiance is tripartite, rather. The material culture and the war-path and raiding complexes show a decided orientation towards the Plains. Contact with the Pueblo peoples of the upper Rio Grande has left its impress in the development of a Jicarilla corn complex and in the ritual life. But despite the interesting differentiation towards Plains and Pueblo characteristics, Jicarilla culture is in fundamental agreement with a round of beliefs and traits which the Southern Athabaskan-speaking tribes share with one another. Jicarilla culture can be best comprehended as a growth and modification of this basic Southern Athabaskan pattern in terms of Plains and Pueblo influence. A tribe which the Jicarilla closely resembles within the Southern Athabaskan-speaking group is the Navaho. In mythology, rites, supernaturals, and the practice of agriculture the correspondences between these two tribes are especially striking.

BAND, LOCAL GROUP, AND FAMILY

The Jicarilla tribe was divided into two bands. The eastern band, called by the Jicarilla gulgahén or "plains people" and known in the literature as the Llanero, ranged east of the Rio Grande and had their favorite re-

1 The field-work upon which this summary is based was carried on upon the present Jicarilla Reservation in northern New Mexico from the spring of 1934 to the spring of 1935 and was made possible by financial assistance from the University of Chicago, Columbia University, the Southwest Society, and the National Research Council. To these institutions and organizations grateful appreciation is acknowledged for their generosity and support.

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treats in the Sangre de Cristo Mountains. The second band lived west of the Rio Grande and called itself sáidindé, "sand people," but is known in the literature as the Ollero. The difference between the two bands was no more than a matter of geographic location. Informants attest that no cultural or linguistic differences between the two bands existed. The members of the two bands intermarried freely. Residence is ordinarily matrilocal among the Jicarilla. Therefore, when the parents belonged to different bands, the children became members of the band of the mother.

Each band was further divided into local groups. The Jicarilla local group was a cluster of individual families associated through blood relationships, marriage, common interests, or strong friendship, living in the same district or around a specific landmark under the control of a recognized chief or leader. At the time of American occupation there were approximately twelve such local groups, six belonging to the eastern band and the same number to the western band.

The Jicarilla family was the primary economic and social segment. It may be described as an extended domestic family with matrilocal residence, comprising a married couple, their unmarried children, their married daughters and sons-in-law, and the children of these.

**WORLD CONCEPTION**

In the beginning, the Jicarilla believe, Black Sky and Earth Woman alone existed. From the union of these two were born certain anthropomorphic supernaturals called hâ-ct'cín. These supernaturals dwelt within the body of their mother, the earth, and in the darkness of this underworld Black hâ-ct'cín was their leader. From clay images which he himself moulded, Black hâ-ct'cín created the animals and birds. Then he made the impression of his own form in the soft earth and brought this tracing to life. Thus did Ancestral Man come into being. Ancestral Man slept and dreamed of a woman. When he awoke he found Ancestral Woman at his side. The first people were the descendants of these two. They possessed the form of mankind, but their existence in the underworld was incorporeal, as in a dream. For a long time they lived without want or death.

It was in this underworld that the sun and moon were constructed by the supernaturals. No sooner were these lifted to the vault of the underworld to give light than shamans from among the people began to claim that they had made these heavenly bodies and could control them. At this the hâ-ct'cín grew vexed and allowed the sun and the moon to break through the vault of the underworld and come to the upper world.

The emergence, then, was an attempt of the people to follow and re-
cover the sources of light they had lost. The ẖa·ct'cìn clothed and decorated
twelve of the boastful shamans with yucca, spruce boughs, and the appro-
riate paint, and conferred greater power upon them. These became the ḥsâ·nất'li, one of the three varieties of supernaturals who are impersonated
in Jicarilla rites today. Six others were painted with black and white stripes.
These are the tcâ·cjini, and dancers who represent them function as the
clowns of Jicarilla rites now.

The powers of all the occupants of the underworld were utilized to
facilitate the ascent. Four mounds of earth were laid in a row, and as the
ceremony progressed these grew, merged into a single mountain, expanded
to gigantic proportions, and rose toward the hole through which the sun
and moon had escaped. The mountain did not grow quite tall enough to
allow the people to reach the upper world unaided, but the ẖa·ct'cìn con-
structed four ladders of sunbeams and upon these the ascent was con-
tinued.

Two old people, a man and a woman, came last, after the rungs of the
ladders were completely worn through. They were therefore forced to re-
main in the underworld. But before the people left the place of emergence,
this old couple warned them that they would return to the underworld.
Because of this the Jicarilla Apache travels to the underworld at death.

At the place of emergence the supernaturals gave instructions and
ceremonies to the people. The birds and animals, who spoke as humans
until now, were deprived of that attribute. But the power by means of
which they had aided in the emergence was still left them, and that is why
the Jicarilla today seeks aid from them when he is ill, and does not molest
them needlessly or even hunt them for food without proper ceremony and
prayer. Until this moment plants and rocks had the power of speech and
movement. Now they were rendered mute and immobile. But “life” and
supernatural power are still theirs; they are still capable of punishing the
impious who mutilate them needlessly or use them without the requisite
prayer.

The earth to which the people of the underworld came is believed to
have the bodily form and attributes of a woman. The story of the emer-
gence is, of course, a myth of gestation, and in the story it is unequivocally
stated that the people emerged from the underworld as man is born of
woman today.

The people traveled in widening clockwise circles from the place of
emergence. As they went, small groups, deeming some spot especially suit-
able, parted from the main body and stayed behind. The children of the
settlers invented new languages in play, and these strange dialects, begun
for amusement, became dominant and superseded the original tongue. Those who did not tarry elsewhere finally reached a region above the heart of the earth. This is the true Jicarilla country, and these people, the Jicarilla, are the sole descendants of the people of the emergence to retain the original language.

The keynote of the Jicarilla world conception is a tremendous enthusiasm for life; a conception of a personified universe with which man may identify himself. The great rocks and mountains are equated with the bony skeleton of a human being and are quite as essential to the existence of the earth as bones are to humankind. Flowing streams are considered to be "living" water in contrast to the "dead" water of stagnant pools. Not only plant life, but all things, are said to have pollen. That most important body of supernaturals, the hà:ct'cín, to whom reference has already been made, are personifications of natural objects. In theory at least, there is a hà:ct'cín of every animal, bird, substance, or natural phenomenon to represent its "power," its essential quality. In the major rite of the hà:ct'cín, only the most important hà:ct'cín, that is, the most important sources of power, such as the sun, moon, lightning, etc., are impersonated by dancers; but in the origin story of the ceremony many others are introduced, and the concept of an animate, personified universe is developed at length.

MATERIAL CULTURE: PLAINS ORIENTATION

One aspect of Jicarilla life which seems to be common knowledge and which has received constant mention is the orientation towards Plains culture—the buffalo hunting, use of travois, parfleche, tipi, etc. Unfortunately this emphasis is somewhat misleading. Such orientation is certainly present in respect to many elements of material culture, but it is very superficial and evidently recent. The case of the Jicarilla tipi will illustrate the point. The literature refers to no other type of dwelling for the Jicarilla than the tipi, yet within the life span of the oldest people of the tribe, the favorite and more common house type was a dome-shaped frame covered with a thatching of leaves or bark. Moreover, the oldest people insist that this lodge (the former house type of the Chiricahua, Mescalero, and Western Apache as well as of the Jicarilla) is a far superior home.

Again, it is true that the Jicarilla went to the plains to hunt buffalo. But no sooner were they out of reach of the mountain refuges which marked their boundaries than a great uneasiness seized them. No time was lost in finding the buffalo, securing the meat, and hastening back to their own territory. Psychologically they were anything but a Plains people. Expeditions to the plains were in the nature of an anxious press forward and a speedy retreat to familiar landmarks.
ECONOMY

Agriculture and wild plant foods. Immediately preceding the period of American occupation an agricultural complex was rapidly developing. It may come as a surprise to many to learn that the Jicarilla were raising a considerable amount of corn along the banks of the streams of the Cimarron region.

The following are traits included in the Jicarilla agricultural complex. Fields were cleared by burning the grass and brush, trees were girdled, dams built, fields flooded periodically, the water level of streams raised so that water could be diverted to ditches, and irrigation ditches were built. A protector of hide was worn over the stomach and used with the digging stick to prepare the holes for seed. Other tools included a crude wooden plow, implements for clearing ditches, and forked winnowing sticks with curved blades.

Corn was selected and saved for seed. Corn kernels were planted with turkey feathers in the fields, and prayers and tobacco offerings were made to the moon and to the turkey feathers which "protected" the fields. A circular hardened threshing-floor was prepared for peas and beans. Corn pollen, corn-meal, and corn kernels were used in ceremonies. There were corn grinding songs, a test of corn grinding for the girls in the adolescent rite, frequent reference to corn and corn parts in ceremonial songs, and the use of corn-ears in the ceremony and ritual racing which now takes place each fifteenth of September. There was also dancing by impersonators of supernaturals for the benefit of crops.

Corn-meal mush was made (ashes of cedar needles were added), and there was considerable dependence upon corn for food as indicated by the many dishes in which corn is the base or an ingredient. Moreover, there is an important myth for the origin of corn. Crops other than corn which were cultivated included pumpkins, beans, cantaloupes, peas, wheat, and tobacco. With the exception of tobacco, which men alone were allowed to tend, crops were planted, weeded, and harvested by the joint labors of the entire family.

Land was inherited, though not in accordance with strict rules, and payment in kind was made for work in the fields.

Though Jicarilla agriculture was rapidly assuming a position of importance at the time of white occupation, it must not be thought that the older sources of wild plant supply were neglected. Probably not over fifty percent of Jicarilla families raised crops at the same time, and very few families raised enough to permit a neglect of wild plant products. The

* Undoubtedly modelled after early Spanish examples.
juniper berry, mesquite bean, yucca fruit, prickly pear, acorn, piñon nut, and chokecherry were always considered indispensable staples by the Jicarilla and were supplemented by scores of other fruits, nuts, seed-bearing grasses, and greens.

Hunting and fishing. Deer, buffalo, antelope, and elk were the principal animals hunted. Mountain-sheep, porcupines, beavers, prairie dogs, squirrels, chipmunks, ground-hogs, chief hares, wood rats, rabbits, and even skunks, peccaries, horses, and burros were also utilized for food. Of bird life, turkeys, doves, grouse, quail, and snow-birds were eaten. Other animals, such as weasels, minks, wolves, wildcats, and a number of birds were not considered fit for food, but were nevertheless hunted for fur, feathers, or body parts essential to the rites.

Numerous methods were employed in the capture of the game. Whistles, designed to imitate the bleat of the fawn, were used to draw does to their death, and head nooses were strung along trails. Head masks were employed in the hunting of both deer and antelopes. Antelopes were obtained in quantities by a drive in which an entire district was encircled by men, women, and older children. As on the plains, buffalo were overtaken and shot by hunters mounted on well-trained ponies. Both arrows and lances were used in dispatching the animal. Snares were arranged around their holes to capture such small mammals as ground-hogs and chief hares. Eagles were taken by means of food tied to the top of an oval shaped blind, within which the hunter waited. Eaglets, taken from their nests, were kept alive in cages, plucked twice, and then freed.

The Jicarilla hunter took infinite care that his luck in bringing down the game was not destroyed. He was cautious in choosing those to whom he gave any of his meat, for if it were carelessly treated he would hunt thereafter in vain. Even the bones and waste were gathered carefully into a pile and carried far from camp.

From earliest childhood the Jicarilla boy was schooled in hunting. When he had attained proficiency in shooting he was ready for a ceremonial hunting expedition in company with five companions, one older and more experienced than the others. The game they secured was eaten ceremonially. A boy who did not participate in such an expedition had slight chance of becoming a skillful hunter, it was believed. At about the age of puberty, his grandfather undertook to prepare the boy for the killing of larger game. The older man gave the necessary instructions, and the boy made his first important kill. The blood from the heart of this kill was rubbed on his face and hands, and it was now considered that his apprenticeship as a hunter had been served.
Jicarilla hunting was a most elaborate and involved complex, proceeding according to fixed rules. For instance, a man who came upon a successful hunter was entitled to the hide and half the meat. If two men came upon the hunter and his kill, they raced to the animal, and the one who touched it first claimed it. When such a race ended in a tie, a wrestling match decided to whom the hide should go. Butchering and disposal of waste were likewise governed by tradition.

Certain beliefs and usages clustered around every animal. To step in the tracks of a deer when trailing it was considered dangerous and unlucky. To complete a clockwise circle about a buffalo was to court disaster, for the buffalo was then likely to charge and gore the hunter. Sympathetic magic played a prominent part in hunting customs. A certain plant which grew low to the ground was placed in the tracks of a deer that the deer, too, might be held close to the earth and so be easily overtaken. Scores of such beliefs, based on the habits of the animal sought and the properties of the objects magically manipulated permeate Jicarilla hunting customs.

In a number of phases of Jicarilla life, of which hunting is one, tasks which are the primary business of the men are thought to be influenced by the attitude and behavior of the women. In keeping with this ideology, we find that a man and his wife pray together and smoke ceremonially before the husband leaves for the hunt. After his departure the woman continues a series of ritual duties.

Not the least interesting aspect of Jicarilla hunting is the manner in which the bloody business of constantly slaughtering animal life is rationalized. Jicarilla myths teach that the animals now used for food once preyed upon mankind. Through the efforts of the culture heroes and supernaturals, the situation was reversed and these animals were threatened with extinction unless they agreed to be useful to man. They are now fulfilling that contract by giving freely of their flesh and hides. They are pleased to be taken and utilized by those who have the proper ceremonial knowledge. Animal life is part of the personified universe whose efforts are primarily concerned with the well-being and happiness of the Jicarilla tribe.

Another important food was fish. A noose with bait suspended at the center was used to snare them. In very shallow water fish were shot with arrows.

RAID

The importance of the horse in Jicarilla culture can scarcely be overestimated. It offered a mobility which made possible the buffalo hunt and enriched Jicarilla culture at innumerable points. The great source of supply was the Plains tribes, and repeated raids to the plains became a definite
part of Jicarilla economy, attended by rigorous training and an organized body of rite, belief, and story.

Raiding parties were small, seldom exceeding ten individuals. Their leaders were qualified by special knowledge, usually transmitted from grandfather to grandchild but occasionally passed to another upon the payment of four gifts (a quiver of mountain-lion skin, a mulberry bow with mountain-lion sinew string, a shield, and a spear). The ceremony was taught only in winter for two consecutive years. The teachings were a mixture of rite and practical advice (songs, prayers, camp craft, and war strategy).

A raid was announced when the heads of families, feeling the lack of sufficient horses, secured the permission of the chief of the local group to remedy their need. When the services of a raid leader had been enlisted, arrangements were made for a dance.

On the appointed evening the onlookers gathered at the dance grounds and one by one those who wished to join the expedition rose and danced. The dancing warriors were dressed in characteristic manner and had their faces painted in a stylized fashion. This dance was in the nature of a vow; one who joined the dance was committed to the venture and could not withdraw without decided loss of prestige. This dance set off the participants from all others. From that moment they busied themselves with preparations for the raid. They were pledged to continence. Interestingly enough for general psychology, the sex act is here and in other contexts of Jicarilla culture, interpreted in terms of a struggle, and if any of the men violated the obligation to sleep apart from women at this time, it would cause an encounter with the enemy.

When all was in readiness, the men left their homes for a pre-arranged meeting place, and the group started running to the east. The expedition was usually timed to begin four days before the full moon, that the travelers might have light throughout the whole journey.

At home, a woman chosen to represent each man (his wife if he was married, his mother, grandmother, or sister otherwise) obeyed many restrictions in matters of dress, food, and behavior to insure his safe return and the success of the entire party. She could not bathe during the absence of the party. Her face was painted red, her hair hung loose and flowing, her shoulders were covered by a buckskin cape such as is worn by the adolescent girl during the puberty rites. She could not rid her hair or person of lice. She could not eat anything sweet, anything tart, or any salt. Water she drank only sparingly out of a shallow clay bowl. She could not give away any food lest her husband or kinsman give away his spoils before reaching
home. She did not tie her moccasin strings but placed them loosely within the uppers of her moccasins, that her husband might not be harassed by the enemy. She could not smoke, for the men dared not smoke in enemy country for fear of detection. All her actions had to be exceptionally circumspect; the commonplaces of household work were governed by rules too numerous to list here and regulations extended to the most personal matters.

The men, when they started out, were not at first burdened with restrictions and obligations. They were dressed in old clothes. An extra pair of moccasins was tied to the belt and rested at the pit of the stomach, for moccasins in this position were thought to allay the pangs of hunger. Each man carried a lance, bow and arrows, a small bag of supplies, and a rawhide rope. The party set out on foot. They traveled rapidly until the second morning. At dawn the leader daubed his followers with white clay. With this ceremony the expedition took on its sacred and serious character. The food and behavior restrictions noted for the women came suddenly into force. The men now became restrained and wary. They could not look up, behind, or to the sides, but had to direct their gaze to the east. They spoke only when it was necessary and then in a special "raiding language" in which reference was made to objects of raiding and ceremony by circumlocution. Thus, to eat together on the raid became "to place down the wolf's paw," to spy upon the enemy camps was "to drag the wolf's tail to the east." It was the custom of the men on the raid to place their provisions in a common pile and to circle around the food and share it. At the time they were painted with white clay, the men were given wooden scratchers (shaped like a horse's hoof) and hollow tubes for drinking water, and these had to be used for the remainder of the trip.

When enemy territory was reached, the leader sought out the enemy camps, located their horses, and drove a body of these toward his men. The horses were driven a safe distance from the enemy's encampment with a modicum of noise. Each man roped a horse. Before they drove off the herd, they gathered the spears, thrust them into the ground and left them with the worn-out moccasins attached, as a gesture of contempt for the enemy.

As soon as the horses were obtained and the homeward spurt began, all special forms of speech and most of the other restrictions were dropped. A successful party entered the encampments from the south side. The man and his wife remained continent for four more days. Then both bathed and resumed normal relations.
WAR

The war complex of the Jicarilla, when compared to that of other Southern Athabaskans, shows clearly the influence of the Plains.

Expeditions of warfare were usually precipitated by the death of a Jicarilla at the hands of an enemy. Before departing, the warriors held a dance at which they appeared with whitened hair surrounded by a streak of red paint, prophetic of the scalped and bleeding enemy. Impersonators of the Jicarilla sacred clowns appeared at this time and uttered their characteristic call into the mouth of each warrior, so that, when he gave his war cry, it would strike terror to the hearts of the enemy.

The warriors departed, mounted and equipped with bow and arrows, lance, shield, knife, and war-club. There were few restrictions upon the men on the war-path compared to those demanded of raiders. Continence was the most important of these.

The traditional enemies were the Indians of the plains, and when these foes were encountered, the two sides lined up, the leaders talked in sign language, rode forward, and met in single combat. This was the signal for the two sides to engage in desperate battle. The object of the Jicarilla warriors was to inflict more casualties than they themselves suffered and to validate their triumph by the securing of scalps. Scalps were the symbol of Jicarilla superiority and the indication of the enemy's defeat and humiliation. If the Jicarilla losses exceeded those of the enemy, scalps could not be taken and any which had been secured were immediately thrown away.

It is interesting to see how the Jicarilla attitude toward the scalp combined the Plains desire for the trophy with the Southern Athabaskan dread connected with the dead. Not everyone was allowed to take scalps; that right was reserved for a leader of raid and war-path who had been ritually prepared by his grandfather. When anyone else killed a foe, he had to request the leader to remove the scalp for him. The leader removed it according to prescribed rules and prepared it for the homeward journey. He shaped a hoop of willow branch and stretched the scalp over this. The hoop and scalp were attached to a tall pole and carried upright as a standard until the encampments were reached. If the journey could not be completed in one day, the scalp was placed far to the east of the men each night, that the ghost of the enemy might not bother them.

When a victorious war party returned home, the first action was to hold a ceremony over the warriors that the ghosts of the enemy might be driven away. The scalps were considered much too dangerous still to en-
trust to the warriors: instead the scalps and all the possessions taken from the foes were put in the care of old men with the requisite ceremonial knowledge, to be sung over and cleansed. These old men, usually four in number, had complete charge of the scalp dance which was to follow. They scraped the fat from the enemy scalps, mixed it with red paint, and applied it to their faces. Then they chose a level place and, hurling the scalp of the most prominent enemy killed in the engagement across the field from west to east four times, determined where they would erect the central post. Other poles with scalps attached were arranged as an outer circle.

First those who had lost relatives in this or previous battles with the enemy were led four times around the grounds. Each mourner was handed a scalp. He threw it down and stamped upon it; or he shot an arrow at one of the scalps on a pole. Before the actual dancing, the warriors who had taken part in the fight told of their deeds. When one finished a recital, he called upon another to attest to the truth of what he had said. The second man vouched for the tale and added the story of his own exploits. After all had spoken, the dancing started. The members of the victorious war party were first to dance. The others joined in soon afterward. The participants circled the central pole, hurling threats, jeers, and insults at the enemy scalps. At this time enemy captives were made to dance and often were compelled to handle and carry scalps. Of psychological import is the belief entertained by the Jicarilla that those who were ill-natured could improve their tempers and those suffering from the pangs of guilty conscience could find relief by shouting loudly at the enemy scalps.

While the dance proceeded, one of the four old men in charge sat at each cardinal point of the dance grounds guarding a jar of water and a shallow clay bowl. Anyone who wished to drink had to receive the water from them. This ritual was symbolic of the privations the warriors had undergone in daring to journey to the waterless stretches to the east.

Social dances took place in the evening. No one was allowed to drink any water at night and no sexual intercourse was permitted for the duration of the dance and ceremony. The dance continued for four days and nights. The old men stayed at the ceremonial grounds all that time and took no sleep until the dance was over. To end the ceremony, the people lined up, each family facing the east in single file. Father, mother, male and female children stood in order. After prayers by the old men the people dispersed and all camps were moved to the east.

But the scalps and enemy possessions were retained by the old men for four days more before being returned to the warriors. Thereafter the warrior had no need to fear the scalp or the ghost of the enemy from whom
it was taken. He could decorate his clothes, his shield, or his tipi with the scalp and bring it out at subsequent dances.

There are a number of other war customs which deserve mention. When older male captives were taken, they were tied to posts and slain by women with lances. Usually these were women who had lost relatives in battle and were taking this means of retaliation. If we are to believe numerous tales and descriptions, the Plains Indians and Jicarilla tried to infuriate each other by the capture or mutilation of children. Jicarilla war songs threatened that the enemy’s children should be captives. Jicarilla mothers were specifically ordered to cut the throats of their children rather than allow them to fall into enemy hands. Jicarilla chiefs, when they faced the line of the enemy before conflict, would taunt and be taunted in turn about the impending captivity of their children. Though the Jicarilla took only scalps of enemy men and women, they also took the thumbs and ears of slain enemy children. When very young children were taken captive, however, they were treated quite decently. Their lot, in terms of manual labor, was sometimes more difficult than that of others, but ordinarily they were accepted into Apache life. If they married within the tribe, there was no discrimination whatever against their offspring.

If an enemy woman were taken captive, she could not be molested until she had been brought back and a ceremony had been performed over her. Captive women were not considered fit wives; they were sexually used and sent from camp to camp to do the heavy work. Their children by Apache men, however, were recognized as Jicarilla.

If a Jicarilla had been made captive by the enemy, though only for a day, he was considered unclean, and at his escape or recapture, a ceremony had to be performed over him to “bring him back” to his people. When scalps of slain Jicarilla were recovered, they were brought back to the encampments and there were wailed over.

When a warrior, because of grief or desperation, resolved to sacrifice his life, he divested himself of all clothing, tearing off even his loin-cloth, to signify that he had broken completely with all the ordinary conventions of life. He then threw himself into the thick of the fight and exposed himself until he received a fatal wound.

MYTHOLOGY

Besides the emergence myth which has been outlined above, and usually told in connection with it, there is the well-known tale of the culture heroes and their success in exterminating the monsters who were making the earth uninhabitable for mankind. The Jicarilla also tell a long and interesting
coyote trickster cycle and a number of cycles of animal and bird stories, including the familiar tale of the moccasin game played for day and night between the animals and the birds. There is still another cycle which has to do with the ludicrous misadventures of a "foolish people," tales which are really pointed lectures on how not to act. Indeed, to catalog the Jicarilla stories would be to mention every element and aspect of Jicarilla life. There is a story to affirm and validate any native conception.

One aspect of Jicarilla mythology which merits attention is the great resemblance in form, order of events, and spirit of the tales to Navaho counterparts. Especially is this true of certain myths which serve as origin stories for ceremonies, a matter of particular interest in view of the very real similarities between a number of Navaho and Jicarilla rites.

RITUAL LIFE

The rites may be classified into two general types, one the shamanistic or personal, and the other, the traditional or "long life" ceremony.

For the former it is believed that some "power" representative of an animal, bird, heavenly body, or natural phenomenon chooses a Jicarilla child for its own at birth. Neither the child nor anyone interested in him has any control over the nature of this "power" or any ability to substitute another in its place. When the child grows up the power appears to him in a dream or vision and bids him follow. If the Jicarilla accompanies his guide he will be subjected to terrifying experiences to test him and "make him brave." Finally he is led into the holy home of the power, past ferocious beasts which act as guards, and there the novice is trained in the songs, prayers, and ritual knowledge which constitute the ceremony. If it is a bird or animal which has offered the ceremony, henceforth that Jicarilla may not slay the source of his power. "We are all relatives now," he is told. Ceremonies obtained in this manner are used particularly for curing, though they have subsidiary functions of finding lost objects, locating the enemy, and the like. The hallmark of the shamanistic rite is the close personal bond between the shaman and his power. Through his songs and prayers he is thought to be in constant communication with his power. Of a shaman it is said, "For his power he works."

To the Jicarilla mind the dependence of the ceremony upon the rapport between the power and the individual has its definite drawbacks. Failure to follow the injunctions of the power means the loss of the ceremony. Often the power demands gruesome payment, such as the sacrifice of a close relative's life. Such power is considered vulnerable and finite. If used too frequently its efficacy diminishes. Once lost, it can never be regained,
and no other shamanistic rite can take its place, for to each man is allotted but one such ceremony. The ceremony, no matter how valuable, cannot be transmitted to another. Moreover, many never obtain such a shamanistic ceremony: some do not follow the guide, others refuse the ceremony when it is offered.³

The other type of Jicarilla ceremony and one which now enjoys ascendency at the expense of the shamanistic rite, is the traditional or “long life” ceremony. These are ceremonies which have no genesis in personal encounters with supernaturals. They have their origin and rationalization in the myths. They may be taught by the elders to younger men and there is no limit, theoretically, to the number of these rites in which a man may participate. Several of these “long life” ceremonies are distinguished for wealth of detail. One such is 'tsáné’, a four day rite, often called by white observers, the “bear dance.” This rite approximates Navaho ritual at many points. For instance, it is held within a corral constructed for the purpose, and a sand painting is drawn each day. It is a curing rite, held for those who suffer from bear or snake sickness, and its mythological sanction is to be found in the story of two girls stolen at the time of the emergence by the bear and the snake. In the rite, two of the three groups of supernaturals who aided in the emergence, the tćà·ćjinti or sacred clowns and the t'sà·nà·tł̣̊̊ make their appearance.

Another important “long life” ceremony is held at a time of widespread sickness, and in it the most important group of supernaturals, the hà·ct’cin, are impersonated. This ceremony is also held within a corral and parallels Navaho and Pueblo ceremonialism in countless details.

At puberty the Jicarilla Apache girl is the center of a four day rite, designed to insure a long and fruitful life for her. The details, ideology, and songs of this ceremony do not differ essentially from corresponding adolescence rites among other Southern Athabaskan peoples, except that among the Jicarilla a boy who impersonates one of the culture heroes is associated throughout with the pubescent girl.

One Jicarilla ceremony deserves special mention, for it undoubtedly received its impetus from a broadly similar rite of the Eastern Pueblos. It is the ceremonial relay race run September fifteenth by the Jicarilla youth. When the rite was studied, it was a considerable surprise to learn that it was partly a harvest festival, partly a contest to determine the comparative abundance of either meat or vegetable food for the coming

year, and that the details of the ceremony were carried on for three days. All of these aspects are foreign to the body of Apache ceremonialism.

The connections between this Jicarilla rite and Eastern Pueblo ritual are conspicuous. For the choosing of sides the two Jicarilla bands are utilized in place of the moieties of the Eastern Pueblos. The runners are dressed and painted in corrals which have the shape of kivas and which the Jicarilla freely compare to the Pueblo kiva. Before the race each side dances to the corral of its opponent carrying a tall standard to which two ears of corn are tied. Without trying to enumerate all the Pueblo-like elements, it may be noted that this is the only Apache ceremony which has a calendrical touch, and that in the story of how the rite was obtained, it is affirmed that the Jicarilla, Taos, San Juan, and Picuris all were given the ceremony at the same time.

The measure of the Jicarilla debt to the Pueblos for the elements of their “long life” ceremonies and their concepts of the supernaturals may be judged from a brief enumeration of traits which cluster about the Jicarilla sacred clown. The Jicarilla clown wears a horned cap of hide, striped black and white. His body is painted white with horizontal black stripes. He wears a bandoleer on which bread is strung and wears deer hoof rattles on his moccasins. He carries a bow, arrow, and spruce branches in his hand. He offers grotesque imitations of those who are impersonating the supernaturals and practices inverted conduct continually, recalling and emphasizing the restrictions of the rite by their flagrant violation. Thus the clown engages in apparent gluttony, in actual filth eating, in simulation of copulation, and in obscene speech. He participates in a burlesque deer hunt which has sexual reference, for the “deer” is offered as a bride price to one of the young women present. The clown is thought to frighten children who are unprepared for his appearance, and he is supposed to be fearsome to enemies and to those ritually unclean. The Jicarilla clown also acts as disciplinarian for adults and takes part in the policing of ceremonial grounds. All of these characteristics have been recorded for the Pueblo clown.4

KINSHIP

Terms and behavior in ego’s generation. The classification of kin in ego’s own generation illustrates some of the prominent features of the system. The Jicarilla group parallel cousins with brothers and sisters; cross-cousins alone are recognized as true cousins. With the cross-cousin of the same sex

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there is a joking relationship, in which rough play and obscenities are the rule. One takes great liberties with the mate of one’s cross-cousin of the same sex, especially in the presence of the cross-cousin. Indeed, these liberties went so far in aboriginal times as to become an institutionalized practice of wife and husband borrowing and stealing in which a mate was the legitimate prey of the cross-cousin.

No less interesting is the relationship between cross-cousins of opposite sex. This relative may be treated in one of two ways. One way is to establish a mild joking relationship, in which one accuses the other of witchcraft. The alternative is to avoid the person entirely. To become too friendly with this cousin will cause some mishap with fire and a severe burn. It is forbidden to drink or eat out of the same receptacle as this relative, to partake of food that he has tasted, or to hand something directly to him. Even his lice are remarkably poisonous.

There are separate terms for older brother, older sister, and younger sibling of either sex. Brothers and sisters are expected to act with great reserve in each other’s presence. The eldest brother, for the boys, and the eldest sister, for the girls, are in a definite position of authority and leadership.

The husbands and wives of kin who are members of ego’s generation are all called by one term, and polite form (a special third person form of the verb) is extended to them in speech and reciprocated. The use of polite form in Jicarilla kinship carries with it the obligation of reserve, respect, and mutual economic helpfulness.

The exception to this rule is the behavior toward the mate of the cross-cousin of the same sex. To this relative by affinity there is what might be called a partial joking relationship balanced by a partial treatment of reserve and respect. When a Jicarilla meets this person alone, or with anyone other than his cross-cousin, he speaks to her in the polite form and obeys all the conventions of reserve and respect which go with it. But when he sees this affinity together with his cross-cousin, the polite form is dropped and he takes the greatest liberties in speech and language.

Terms and behavior in first ascending and first descending generations. In the first ascending generation the tendency is to classify the father’s brother with the father though he can be called by a distinguishing term. Likewise, the mother’s sister is addressed as mother under ordinary conditions, but there is a distinctive term for her also. The Jicarilla has a joking relationship with his mother’s brother, whom he calls in jest “my leader.” The expression refers to the good-natured rivalry between the two, in which one is continually challenging the other to a contest in eat-
Any grandchild is called citsóyí'í by any grandparent.

The great-grandmother is called ná'kidínátcó', "grandmother twice removed."

The great-grandfather is called ná'kidínátsóyé', "grandfather twice removed."

The great-grandchild is called ná'kidínátsóyí', "grandchild twice removed."

The stepmother is called ciká'.

The stepfather is called cibéjí'.

Sometimes cidad'í' is used as a self reciprocal for the maternal uncle-nephew, niece relationship.

The term cilísé' is applicable to any cousin or sibling of the same sex; cilá' to any cousin or sibling of the opposite sex.

Note that the personal pronoun ci is omitted from terms of affinity where such terms are also used to denote relationship by blood. Thus hédač'í' (maternal uncle's wife) is, with personal pronoun affixed, the regular term for older sister.

Differences in the kinship system when a woman is speaking

Women do not use the word cilhá'á'c for cross-cousin, but sizé'dí' only.

A female calls a female sibling or cousin cilísé' and a sibling or cousin of the opposite sex cilá'.

A female calls nieces and nephews (children of brothers or male cousins) citsilke' and citsilki'í' respectively.
ing, racing, or in any test of skill or endurance for which opportunity arises. With the father's sister there is a similar joking relationship, though in a milder vein. There are separate terms for mother's brother and father's sister.

The mates of the relatives of the first ascending generation are addressed by terms also used to blood relatives. To indicate that they are relatives by affinity and not consanguinity, the personal pronoun is dropped, except in cases where a word meaning "has become" is added.

In the first descending generation children of siblings or cousins of the same sex are regarded as sons and daughters and are so called. Children of siblings or cousins of the opposite sex are nieces and nephews, and with them is established the "my leader" joking relationship already mentioned. A man calls the mate of his "son" (i.e., his own son or a son of a sibling or cousin of the same sex) "my female son," and the two hide from each other. He speaks with the polite form to the husband of his "daughter." The woman uses polite form to the wife of her "son," and hides from the husband of her "daughter." A man also calls his nephew's wife "female son," and hides from her. To the husband of his niece he uses polite form. A woman hides from her niece's husband and speaks in the polite form to her nephew's wife.

Terms and behavior in the second ascending and second descending generations. The Jicarilla use but two grandparent terms, one for grandparents of each sex; whether they are paternal or maternal grandparents is not distinguished. There is one term for grandchild, no matter of what sex or how related.

Between grandchild and grandparent there exists the most boisterous and unrestrained of all the many joking relationships which typify the Jicarilla system. License and obscenity are freely indulged. The relationship is accompanied by much teasing and practical joking as well, but its chief characteristic is allusion to sex and mating. When joking, the grandchildren and grandparents address each other by an interesting set of special terms which are really plays on the correct and formal grandparent-grandchild terms.

This joking relationship, however, is but one phase of grandparent-grandchild behavior. Quite as important is the grandparent's rôle of disciplinarian and teacher. When the child is quite young he is told that if he persists in disobedience, the grandparent will be called to punish him. This is a forceful warning, for the grandparent is known to have several painful methods of chastisement which he may exercise and which are the prerogatives of grandparents alone. The grandparent is the prime bugaboo
of the disobedient child, employing such drastic measures as castration
threats and threats to cut the child’s throat.

As the children grow older it is upon the grandparent that the greatest
responsibility for their education falls. The grandfather trains the boy;
the grandmother instructs the granddaughter in the work and arts with
which a woman should be conversant.

The joking relationship existing between grandchild and grandparent
obtains between great-grandparents and great-grandchildren too, but in
diluted form. The mate of the great-grandchild calls the great-grandparents
of his spouse simply grandfather and grandmother. There is no avoidance
or use of the polite form by either man or woman for affinities of the third
ascending or third descending generation.

Terms and behavior extended to a spouse’s relatives. Both the Jicarilla
man and woman, at marriage, use the polite form of speech to siblings and
cousins of the mate, irrespective of sex. There is no avoidance of affinities
in ego’s mate’s generation.

Of affinities in his wife’s parents’ generation the man avoids those of the
sex opposite to his own and speaks in the polite manner to those of the
same sex. The woman avoids her husband’s male relatives of this genera-
tion and speaks politely to his female relatives of the parental generation.

For the relatives by marriage in the first descending generation, the
man calls his wife’s “son” and “daughter” (i.e., the children of his wife’s
siblings and cousins of the same sex) “my son he becomes” or “my daughter
she becomes.” He calls those his wife terms niece or nephew, niece or
nephew also, omitting the personal pronoun to indicate his more distant
relationship. A woman calls the children of her husband’s male siblings or
cousins “my children they become.” The nieces and nephews of her hus-
band (the children of his siblings and cousins of the opposite sex) she calls
“younger sister,” “younger brother,” depending on sex.

Those whom his mate calls grandchildren, a Jicarilla also calls grand-
children at marriage, and those whom his mate calls grandparent, a Jicarilla
calls grandparent too. There is no avoidance of or use of polite form to
the mate’s relatives of the second ascending or second descending genera-
tion for either the man or woman.

MARITAL LIFE

Of the many concepts found in connection with marital life only a few
of the most interesting can be summarized here. The close relatives of a
dead person are considered unclean until a ceremony is performed over
them. There is never any question of allowing the ceremony to be per-
formed over any relatives of the deceased—except his mate. Before a widower may participate in the ceremony and "be forgiven" he must be freed by his wife's family, and until then he is a pariah, forced to wear a rawhide yoke, the badge of his servility, denied the right to marry, and compelled to stay apart from other people (even his own children). He might lead such an existence for some time, especially when the relatives of the mate had reason to condemn him for neglect or abuse of his partner. Sometimes the "slave" was allowed to have the purifying rite performed upon payment of a number of horses. Once he was freed, his former mate's family had no further claim on him. Often the price of freedom was that he marry a sibling or cousin of the dead mate. This was one of the circumstances that favored the sororate and levirate among the Jicarilla, for both men and women were subject to this enslavement and ceremonial uncleanliness at the death of the mate.

A man whose wife died was considered unlucky. Any woman he married after his bereavement was likely to suffer the fate of the first wife. Therefore, before a widower contracted a second marriage (i.e., an attachment which is intended to be permanent) it was necessary for him to live with another woman for a period of less than a year. This temporary arrangement was thought of as a medicine to "bring back" the man from his state of sorrow and ill-luck. Even if the couple grew fond of each other, the man would have to abandon this woman, for to do otherwise would be to endanger her life and the lives of all her issue. Only after this temporary union and a special ceremony could a man assume the obligations of a second marriage.

Polygyny was practiced, the second wife being in all possible cases a sister or cousin of the first wife. There was a strong feeling for both the sororate and levirate.

A conflict between man and woman is posited, especially in their rôles as sexual partners, which goes back to the mythical period. Consequently, the sight, smell, or touch of menstrual blood is extremely dangerous to men. It is claimed that the woman has a pouch within her partially filled with menstrual blood. The semen of the male is considered to be blood drawn from the rest of the body and stored in the testicles. At orgasm this blood is emitted and finds its way to the female pouch. The woman's blood opposes conception, while the male blood strives for it. Since the woman's blood has a decided quantitative advantage at the end of the first sexual union, no impregnation is achieved. It is not until coitus has occurred a number of times and the blood of the woman is overpowered by the male blood, that the baby begins to develop. Since pregnancy occurs whenever
enough "blood" from males accumulates in the woman, it is believed that a child can belong in part to several men. Such children who have been begotten by a number of persons have more than one center on the top of the head from which the hair spreads out in spirals.

DEATH, THE UNDERWORLD, AND THE RITES OF BURIAL

Evil, misfortune, the malice of witches, all summed up in the term t'cîd'nî, are pitted against the life of man. Sooner or later they force life from the body. This departing life has two components. One is called "breath," and it is this which leaves the corpse via the sole of the foot in the form of a small whirlwind. When a man is dying, the "breaths" or shades of his dead relatives come to him from the underworld. They offer him food, and, upon his acceptance of it, his "breath" makes its exit from his body. The "breath" accompanies the relatives to the north. The journey lasts four days and terminates at the edge of the earth. Here a wild plum tree grows, the fruit of which is offered to the traveler. Should he refuse it he returns to his body and to life.

If the "breath" eats of the fruit, it slides down a steep embankment to an underworld which is divided by a high rock wall into two parts. In one part live those who have been witches on earth. Here the plants are the poisonous and inedible varieties, the animals are those that are dangerous on earth. The only food these people can eat is lizards. Each night they hear the merriment on the other side of the wall and try to dig away the dividing barrier with stone knives. They nearly succeed in penetrating it, but in the morning the rock closes up solidly again. On the other side of the wall live those who were not witches in life, and their existence is one of pleasure and plenty.

If the "breath" is the spiritual, intangible aspect of life, what we have now to consider is the material phase of existence. Blood, marrow, saliva, mucous discharges, are all considered evidence of life, and they are not destroyed at death. These go to make the gôkô'c or ghost. For four days after death, while the "breath" is journeying to the end of the earth, the ghost remains around the body and tries to return to old haunts. Then, should the "breath" take the fruit and enter the land of the dead, the ghost turns into a coyote. The Jicarilla are extremely afraid of coyotes at any time. This ghost is always thought of as malignant and vindictive, and the relatives of the dead, since it stays around the body for four days, and since they have to handle and bury the body, are in constant danger from it.

The rites of burial reflect the dread inspired by the gôkô'c. Preparations for burial take place without delay. The body is washed, dressed in good
clothes, and the face painted red. Older relatives attend to these duties: the young are thought particularly susceptible to the disorders contracted from ghosts. The body is tied upon the favorite horse of the deceased together with as many of his personal possessions as can be carried, and the party proceeds to some lonely spot where burial is made in a crevice of the rocks. The personal possessions of the dead are buried with him or burned and thrown away, for it is believed that otherwise the ghost will return to claim his belongings. The horse which bore the body is shot near the grave. The burial party rub themselves with cedar branches and lay these in the form of a cross on the grave. Then they return by a different route from the one taken to the place of burial, and one of them traces four lines on the ground between the grave and the encampment. All those who have come in contact with the body throw away the clothes worn at the time, and all the close relatives of the deceased cut their hair. Immediately upon the return of the burial party, camp is moved to the east. During the days which follow special precautions are employed to lessen the danger of the ghost's return. As among other Southern Athabaskans, the names of the dead are never mentioned by the Jicarilla.

The members of the family of the deceased are considered contaminated by their loss; and contact with them, or their food, receptacles, and clothes is thought dangerous to others until a ceremony is performed to "restore" them at the next new moon after the death. During the interim they must stay by themselves, attend no ceremonies or social occasions, dress and eat simply, and avoid sexual intercourse. At the new moon a singer hired for the occasion performs the purifying ceremony, releasing them from the restrictions and making it possible for them to mingle with their fellows again.

New York City
INTERNAL LINGUISTIC EVIDENCE SUGGESTIVE OF THE NORTHERN ORIGIN OF THE NAVAHO

By EDWARD SAPIR

INTERNAL linguistic evidence for inferences as to cultural antecedents is not in much favor among cultural anthropologists at the present time, and this for two reasons. Such linguistic evidence is often, if not generally, tricky as to what of a factual nature can be gathered from it, for words may change their meanings radically and, furthermore, it is often difficult to tell whether community of nomenclature rests on early linguistic relationship or on linguistic borrowing attending cultural diffusion. In the second place linguistic evidence is difficult to handle, full of phonologic pitfalls, requiring a closeness of knowledge that is often out of proportion to what little can be obtained from it for tangible cultural inference. Nevertheless, at its best linguistic evidence, properly controlled, may throw an unexpected light on remote cultural perspectives. There is reason to think that as our descriptive and comparative knowledge of unwritten languages increases, their value for cultural reconstructions and other kinds of inference—not least among which is elimination of theoretically conceivable possibilities—will grow in importance. It is natural that in the Americanistic field linguistic evidence has as yet yielded but a scanty return to the historian of culture, but this need not continue to be the case indefinitely.

I shall try to show that there is tangible evidence in Navaho itself for the secondary origin of apparently fundamental elements of Navaho culture, such as agriculture, and that such evidence seems to point to an early association of the culture of these people with a more northern environment than their present one. It may be said—and with justice—that the distribution of the Athapaskan languages is such as to make this historical theory as good as certain, but dialectic distribution is external, rather than internal, linguistic evidence. It is conceivable, if not plausible, that the Athapaskan-speaking tribes were originally massed in the Southwest and gradually rayed out to the north in successive waves of migration. One might argue that the Navaho and, to a greater degree, the various Apache tribes present the non-Pueblo aspect they do, not because of their relative recency in the area of Pueblo cultural development but because, like the Walapai and other Yuman tribes of Arizona, they represent a simpler and more archaic Southwestern culture, which proved impervious, aside from a late Pueblo veneer, to the influence of the more elaborate cultures in their neighborhood. It is true that the linguistic homogeneity of the Southern Athapaskan dialects is such and the dialectic cleavages in the northern
Athapaskan area are so profound that the suggested theory fails to carry conviction either to the linguist or to the ethnologist, but here again we are dealing with external linguistic evidence. This external evidence is far more compelling than can be any evidence derived from details of dialectic structure or vocabulary, for it is more direct and sweeping. None the less, the more elusive internal linguistic evidence has its place in giving confirmation to a hypothesis based on linguistic distributions.

There is undoubtedly a large amount of relevant cultural evidence packed away in the vocabularies of Navaho and Apache. For the present I must content myself with considerations based on the study of four words or groups of words.

1. The Navaho word for "gourd" is 'ądè'. The word is used both for the plant and for the "gourd dipper, ladle." The "gourd rattle," on the other hand, is otherwise named ('â-gá-l'). But 'ândè' means not only "gourd ladle" but "dipper, ladle, spoon" in general, the gourd ladle being the ladle or spoon _par excellence_. Hence we find the earthen spoon called "mud 'ándose'" or "earth 'ández'," while the modern tablespoon is called "metal (<flint) 'ández'." Now the term 'ández' (in form a possessed noun -dè' with indefinite possessive prefix '-a- "somebody's" or "something's") means not only "gourd," "gourd ladle," and "ladle, spoon" in general, a natural family of words, but also "horn" or rather "somebody's, some animal's horn" (dé "horn" as absolute; 'ández' "an animal's horn," parallel to bi-dè' "his [animal's] horn"). In no other Athapaskan dialect does 'ández' or its dialectic equivalent mean "gourd" or "gourd ladle," while, so far as I can discover, it is only in Apache that it means not only "horn" but also "ladle" in general. In Chiricahua Apache we have possessed -dè' "horn (of animal)" and 'ídè' "cup, dish, dipper"; in Mescalero Apache -dè' "horn (of animal)" and 'ídè' "cup, dish, dipper." In both Navaho and Apache 'ández', 'ídè', 'ídè', in its meaning of "gourd ladle" or "dipper," keeps its indefinite possessive prefix 'a-, 'i-, when itself possessed, e.g., Nav. bê'-ídè' (as-similated from *bi'-ández') "his gourd ladle," Chiricahua Apache bi'-ídè' "his dipper," Mescalero Apache bi'-ídè'. This does not in the least prove

1 See, e.g., Franciscan Fathers, A Vocabulary of the Navaho Language (2 vols., St Michaels, Arizona, 1912), Vol. 1, p. 99, sub "gourd;" Vol. 2, p. 13, sub ądè', where it is defined as _Cucurbita_.

2 See Franciscan Fathers, An Ethnologic Dictionary of the Navaho Language (St Michaels, Arizona, 1910).

3 _Ibid._, p. 401.

4 Vocabulary, Vol. 1, p. 186, sub "spoon."

5 My Chiricahua and Mescalero Apache forms are quoted from manuscript material kindly put at my disposal by Dr Harry Hoijer.
that Navaho 'à-dè' "one's horn" and 'àdè' "gourd ladle" are unrelated words, for we have other examples in Southern Athapaskan of double possessives of type "his-one's ..."; e.g., Navaho bì-tà' "his (i.e., bird's) feather," 'à-tà' "a (bird's) feather," but bì-ëtì'ì' "his-one's-feather," i.e., "his (secondarily owned) feather, his plume (used in hair decoration)."

All this suggests that Navaho 'àdè' "gourd ladle" originally meant "ladle" in general and that this word in turn originally meant "an animal's horn," reinterpreted as "horn spoon," very much as our musical instrument, the "horn," originally a "ram's horn" used for blowing, is now a brass instrument with no obvious relation to an animal's horn. The semantic history of 'àdè' would, then, be: (1) an animal's horn; (2) ladle made of horn; (3) any ladle; (4) gourd ladle; (5) the gourd, Cucurbita, of which ladles are made. Stage 1 would be proto-Athapaskan; 2, a dialectic Northern and Pacific, and presumably early Southern, development based on the widespread use of horn for spoons; 3, a Southern Athapaskan transfer of meaning due to the fact that spoons were no longer made of horn; and 4 and 5, a specific Navaho (in part perhaps also Apache) development. Inasmuch as stage 2 no longer has validity in Navaho, the meanings of the word group into two disconnected sets (1; 3–5), so that 'àdè' is now felt to be two distinct and unrelated words, the more so as it is tabooed among the Navaho to use the horn of the deer for the making of spoons. My interpreter Albert Sandoval once volunteered surprise that identically the same Navaho word meant both "a horn" and "gourd, gourd ladle."

If we turn to other than Southern Athapaskan dialects, we find that the absolute *dè "horn," the possessed -dè' "horn of . . .," and the form with indefinite possessive prefix *tì-dè' "an animal's horn" are found in both of the two other Athapaskan areas. Corresponding to Navaho dé we have, e.g., Kutchin ŋi "horn" and Hupa -dè- (in compounds); corresponding to Navaho -dè' we have Carrier -dè, Chipewyan -dè (Li) (Chipewyan high tone = Athapaskan [Navaho, Apache, Sarsi, Kutchin] low tone), Hare -dè, Loucheux -¿i, Kutchin ŋi, Beaver -dè, Sarsi -dà, Hupa -dè, Kato -dè, and Mattole -dè; while Navaho 'à-dè' has an exact correspondent in Chipewyan -é, Hare é-dè, Loucheux e-¿i, Kutchin é-f¿i', Bâtard Loucheux e-dè, and Hupa ŋi-dè, all meaning "an animal's horn." The early use of horn for spoons, which can only be inferred for Southern Athapaskan, is linguistically reflected in Hupa ¿ide-kin', literally "a horn's handle," whence "spoon," and in Hare éde-è a "cuiller en corne" (Petitot: è a "plate, bowl") and Sheep Indian (esbatahot'ine) ede-ka "corne aplatie" (Petitot), whence "spoon." Obviously, to the Navaho mind 'àdè' in its meaning of "gourd" must be referred to the beginning of things, for the
term is used in ritual and mythology, for example in the compound term Gourd Children, but the feeling of the Navaho is of no more importance in the historic problem than their conviction that if', always meant "horse" (though we can easily prove from comparative evidence that its original meaning was "dog") and that their ancestors became acquainted with the horse not too long after the Emergence, as indicated by the origin legend for the creation of the horse in the four cardinal points out of the four ritualistically proper materials.

Our linguistic analysis, in short, points unmistakably to two things of historical interest: that the gourd is not an original element of Southern Athapaskan culture; and that horn spoons, not directly given by present-day Navaho culture, must be assumed to have been known to the remoter Athapaskan-speaking ancestors of the Navaho or, at the least, to early Southern Athapaskan culture. These inferences go well with a theory of immigration of the Navaho and Apache from the north (or east) into the Southwest. Even if one goes no further than to infer the absence of the gourd and the presence of horn spoons in an early phase of the culture of the Navaho-Apache tribes, the illumination brought by a close analysis of Navaho 'àdè' and its Apache cognates is useful for the reconstruction of the period antedating the massive influence of the Pueblos on the Navaho and the Apache tribes.

2. The Navaho verb for "seed lies" is -sàs, a perfective neuter, e.g., sísàs "the seed lies," ńsàs "the seed lies in a row." The original meaning of these forms is obviously not specifically "the seed lies" but, more generally, "the mass of finely divided particles (e.g., grain, sand) lies." A corresponding active verb, nà:sàs, means, for instance, not merely "I scatter the seed," but also "I let the mass (of grain, sand) spill (e.g., out of a bag); I sprinkle it (e.g., sand, water)." I can find no cognate for these verbs in the material available to me from other Athapaskan dialects, and the inference—as so often in analogous Athapaskan cases of apparently isolated verbs—is that we probably have here a dialectic denominative formation, i.e., a secondary set of verbs based on a noun.

Now it is perfectly clear from Navaho phonology that all verb stems beginning in s (after vowels) are contracted products of a "classifier" -l- and either z or y; in other words, -sàs must go back to either -l-zàs or -l-yàs. The perfective neuter *sí:l-zàs or *sí:l-yàs is analogous in form to such a perfective neuter verb as sílçò:z8 "the fabric lies." But what is the

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8 An Ethnologic Dictionary, pp. 351, 353.
9 See, e.g., Vocabulary, sub "broadcast (in sowing)."
8 c = ts.
underlying zâs or yâs? Quite obviously, “snow,” Navaho zâs, yâs (these two forms, of which the latter is the more archaic, constitute one of the few cases of dialectic difference within Navaho). Hence the verb sîsâs must have meant, originally, “it lies like (flakes of) snow,” whence “the seed lies”; the derived active verb nà:sâs originally meant “I scatter it about (so that it lies) like snow,” whence “I sow the seed broadcast.”

As in the preceding case, while the present cultural term is not widespread in Athapaskan but is confined to Navaho (or Southern Athapaskan), it is not difficult to establish a close connection with a universal Athapaskan term of differing cultural connotation. Athapaskan *yâxs9 “snow” is found in the majority of Athapaskan dialects: Ingalk yîð, Babine yîs (Jeness), Carrier yás, Chipeywan yâd (Li), Slave zâh, Hare, Dogrib zâh, Loucheux zîow, Kutchin zâh, Kaska zâs (Jeness), Beaver yas, Sarsi zâs, Kwahicoqua yaxs, Hupa yahs,10 Kato yas, Mattole yas, Jicarilla Apache zas, Mescalero, Chiricahua zâs. The original meaning of the Athapaskan word is not “snow” in general but specifically “snow lying on the ground;”11 another common Athapaskan term, represented by Navaho čîl, means “snowdrift” or “falling snow.” This restricted meaning, “snow lying on the ground,” is clearly the prototype of the present Navaho term for “the seed lies.” To summarize, a non-agricultural term (“snow lying on the ground”) takes on a transferred and more general meaning in a classificatory verb (“the finely divided particles lie [snow-like] on the ground”) and, in a secondary, agricultural environment, advances to the technical meaning of “the seed lies.” No other sequence of meanings fits the linguistic facts.

3. The Navaho word for “corn” is nà:dấ-. The second element, -dấ-, occurs in a number of compound nouns referring to plants in which it tends to be translated “corn” by Navaho interpreters, e.g., hà:sčé:-dá́- “buckthorn,” approximately “god-corn” (hà:sčé:- is a familiar Navaho god name and, in slightly abbreviated form, is the first element in the native term for the Talking God); čî:́dá́- “buckthorn,” perhaps “bitter corn” (cf. dî:čî́- “it is bitter”); mà:l:dá́- “cedar-berries,” literally “coyote-corn;”

9 My reason for reconstructing to high-toned *yâxs rather than low-toned *yâxs is too technical to give here.

10 The Hupa word means not “snow” but “white frost (on trees).”

gàhcòhdá’ “winterfat,” literally “jackrabbit-corn.” The reason why, in compounds such as these, -dá’ is translated “corn” rather than “food,” which is obviously more logical in such terms as “coyote-food” and “jackrabbit-food,” is probably the use of the abbreviated -dá’ for “corn” in possessed forms (e.g., šìdá’ “my corn”) instead of the fuller nà:dá’ of the absolutive. But it is quite easy to prove that -dá’ is not, in any true sense, abbreviated from nà:dá’ “corn” but, on the contrary, is an old term for “food” which lingers, somewhat disguised, in such compounds as have been quoted and in possessed forms for “corn” (šìdá’ “my food” par excellence, whence “my corn”). This interpretation, not clear to the Navaho himself because the word in actual use for “food” is čí:yá’n and he therefore feels that the primary meaning of -dá’ is, or should be, “corn,” is at once made plausible from within Navaho when we compare -dá’ with the medio-passive imperfective neuter verb -dá “to be eatable” (e.g., yídá “it is eatable”), itself closely related to the durative transitive verb -yá “to eat it” (from which čí:yá’n above is independently derived). It looks, therefore, as if -dá’ originally meant “what is eatable,” i.e., “food,” secondarily “corn” in possessed forms.

The nà- of nà:dá’ is quite obscure to the Navaho. It seems to follow no obvious analogy and cannot be equated with the common nà- “about, here and there” of continuative verbs. One might venture nà:dá’, originally “corn is here and there,” whence “planted corn, standing corn,” finally generalized to “corn.” This is to be taken no more seriously, however, than an attempt to see our common word tide in the -tide of eventide, whereas every historical student of English knows that this compounded -tide is a survival of an old word tide synonymous with time and cognate with Danish tid and German Zeit. Our problem cannot be considered completely solved until we have done more than plausibly surmise that -dá’ originally meant “food” and have found a linguistically unforced explanation of nà-. The former requirement is met by a consideration of Athapaskan cognates, which reconstruct to *dán-ê (itself reduced from *dê-hân-ê “that which is eaten, food,” relative form in *-ê of *dê-hân, whence *-dán, “to be eaten, to be eatable”), possessed form *-dán-ê’, *-dán-ê’ “food of . . .:” Chipewyan dâñe (Li), possessed -dâñé, Sarsi dâñi, Mescalero Apache dán, possessed -dán and -dá’, Chiricahua Apache dán, possessed -dán (also -dá’ in nà:dá’ “corn,” perhaps borrowed from Navaho), Hupa possessed -da’n’ in -da’n’ sa’a’n “food of . . . is lying” = “. . . is saving with food,” Mattole possessed -da’ne” “possession, property” (presumably a meaning enlarged from “food”). These forms enable us to understand the exact status of Navaho -dá’. It is not the reflex of the primary *dânê
“food” but of its possessed form *-dánè’ “food of ...”; the former (exemplified by Chipewyan dáné, Sarsi dáñ, and Apache dán [read dáñ, for an old Southern Athapaskan *dán, monosyllabic, would have yielded Navaho, Apache *dán, while an old Athapaskan *dán would have given Navaho, Apache *dá]) would have resulted in Navaho *dání, *dáñ. The Apache possessed forms in -dán are merely generalized from the absolutive dáñ, the variant Mescalero -dá’i “food of ...” being the true reflex of Athapaskan *dán-è’ and an exact cognate of Navaho -dá’ “corn.” We see, therefore, that on strictly linguistic grounds such Navaho forms as má’ti-dá’i mean, not “coyote-food,” but “coyote’s food.” This makes it doubly impossible to interpret ná’-dá’ as “corn here and there,” which form, if it ever existed, would have had to yield *ná’-dáñ in Navaho. We are driven to infer that ná’-dá’ originally meant “food of ná’-,” whatever ná’- may be.

Once we see that ná’- must have referred to certain beings, human or animal, whose food was corn, we advance rapidly to a satisfactory linguistic solution. Many Athapaskan dialects have reflexes of an old word for “enemy, aliens,” occurring in two forms (*ná’-, ná’- in compounds, and, with indefinite possessive prefix, *ği-(dè-)-ná’-, *ği-na’- in compounds). These words are frequently used to refer to specific neighboring tribes. Examples of *ği-na’-, *ği-dè-na’- (*dè- is collective) and compounded *ği-na’- are: Carrier ə-d-na, Chipewyan tè-ná (Li) “enemy, Cree Indian,” Slave e-na-kie “Eskimo,” Hare e-h-da “enemy,” e-na-ke “Eskimo,” Dogrib e-h-da, Loucheux e-ne “enemy, Eskimo,” Bâtard Loucheux a-ra-ke “Eskimo,” Kutchin čè-k’óí (contracted from *ča-nè-) “Eskimo,” Hupa ǯi-na’ “Yurok Indian,” Navaho ’ā-ná’, Mescalero, Chiricahua Apache ’i-ndá’. (The -kie, -ke, -k’óí of some of these forms, analogous to Navaho -ké, is a plural animate suffix.) The old compounded form without indefinite prefix, *ná’-, is illustrated in Chipewyan na-tń-i “enemy” (Petitot) (literally, “the one who acts as an enemy”) and, presumably, in Kato na-čká “orphan” (from “alien” + “child, little”). In Navaho this ná’- is found in compound nouns, particularly such as refer to foreign peoples, e.g., ná’lání “Comanche Indians” (from “enemy-many-the”), ná’-šléží “Zuñi Indians” (contracted from ná’-yśléží “enemy” + “the ones who are blackened”), ná’-gálí (apparently made over, in accordance with the Navaho tribal name pattern, from má’-gálí “Mescalero”),12 ná’-tń-hó “Laguna Indians” (apparently also “Isleta Indians”)? The last of these tribal names is interpreted as “enemies at the water” by the Franciscan Fathers13 but a

12 Ibid., Vol. 2, p. 135, sub náqohó; better “at the river,” for this name is based on tń-h, possessed -tń-h, “river which does not dry up,” rather than on tń “water.”
more natural interpretation is to take the name as a relative in -ī (assimilated to -ō) from nà-tō-h “enemy-river,” presumably an old name for the San Jose (and Rio Grande?), in contrast to the two normal interpretations of tō-h, namely San Juan River and Little Colorado River, the two rivers in or near the old Navaho habitat which never completely dry up. This is confirmed by the place name nà-tō-h (h) sīkāi given by the Franciscan Fathers for Grant, New Mexico (nātço sākāl),14 literally “the enemy river has its legs distended,” “(where) the San Jose turns crotch-wise.” The point is of some importance linguistically as indicating that Navaho compounds in nā- “enemy” not only mean “. . . enemies” but also “enemy . . .” In other words, both nà-tō-h and nà-dā’ are archaic Navaho words which qualify basic nouns (“river” and “food”) by referring them to the enemy, in this case the Pueblo Indians.

The Navaho word for “corn,” nà-dā’, in summary, which can be analyzed with great probability into an older “food of the enemy,” “Pueblo food,” implies that there was a time when the Navaho, an agricultural people in historic times, were still thinking of corn as an alien food. Later on, when they had adopted corn as a staple and had built so much of their myth and ritual around it that it was inconceivable to them that there could be anything alien about it, they could not possibly feel the nā- of their word for “corn” as akin to the -nà- of ànà “enemy” and the nā- of tribal names. The sentiments clustering about the two terms had become irreconcilable.

4. There is a curious verb stem in Navaho which seems to be used only in certain quite specialized verbs; this stem has the forms: imperfective -kē-h (probably error for -kē-h), perfective -kī, progressive and future -kē-l, usitative and iterative -kē-h, optative -kē-l. It is used in an idiomatic verb referring to sleeplessness, e.g., iterative bīl sīcánkē-h “sleeplessness always bothers me,” perfective bīl sīcānkī “I have been sleepless.” The form of the verb is such (bīl “sleep” is subject; -cā “away from” is preceded by the indirect pronominal object) as to suggest that the verb stem refers to a specific type of movement. My interpreter, Albert Sandoval, had no notion what the underlying metaphor was but said he felt, somehow, that there was a reference to gliding movement in it: “sleep glides (slips) away from me.” There is no linguistic support for this feeling, which is hardly more than an ad hoc interpretation to fit the linguistic form. This obscure verb, as Sandoval pointed out, must have the same stem, in its progressive form (-kē-l), as the sacred name of the owl, cāhālxē-yīl nā-kēl “darkness with-it the-one-who-comes-gliding(?)-back, the one who comes gliding (?) back

14 Ibid., Vol. 1, p. 226.
with darkness." The image of gliding is not so apposite here. The simple progressive form, which would be *yiké’-l "it glides (?) along," is not in use in Navaho. There is nothing to be done with these isolated forms except to see in them survivals of an old set of verbs of movement which perhaps still occur in other Athapaskan dialects.

Turning to Chiricahua Apache, we find the verb stem: imperfective momentaneous -kè· (continuative -kê), perfective -kí, progressive and future -ké’-l, usitative and iterative -ké, optative (evidently transferred from imperfective) momentaneous -kè· (continuative -kê). Its meaning is given as "several run, trot," which is by no means easy to reconcile with the hypothetical "glide" of the Navaho words. If the Navaho and Chiricahua Apache words are historically related, as is indicated by their strict formal parallelism, it must be because each dialect has developed specialized meanings that diverge from a third term. Now the distribution of the meanings of the Northern and Pacific Athapaskan verb stems which are demonstrably cognate to the Southern Athapaskan stems is such as to leave little doubt of what this third term must have been. The following table of stem forms gives a summary of dialectic meanings and of phonetic equivalents for four selected stems of the set:

<table>
<thead>
<tr>
<th>Athapaskan</th>
<th>Momentaneous imperfective</th>
<th>Perfective</th>
<th>Progressive</th>
<th>Continuative imperfective</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Ingalki “to travel by canoe”</td>
<td>*-kè’-x</td>
<td>*-kén</td>
<td>*-ké’-l</td>
<td>*-kè</td>
</tr>
<tr>
<td>2. Loucheux (ditto)</td>
<td>-kaix</td>
<td>-kan</td>
<td>-kal</td>
<td></td>
</tr>
<tr>
<td>3. Kutchen (ditto)</td>
<td>-kòi</td>
<td>-kê</td>
<td>-kê</td>
<td></td>
</tr>
<tr>
<td>4. Carrier (ditto)</td>
<td>-kè</td>
<td>-kè</td>
<td>-kè</td>
<td></td>
</tr>
<tr>
<td>5. Beaver (ditto)</td>
<td>-kè</td>
<td>-kè</td>
<td>-kè</td>
<td></td>
</tr>
<tr>
<td>6. Hare (ditto)</td>
<td>(read -kè)</td>
<td>-kè</td>
<td>-kè</td>
<td></td>
</tr>
<tr>
<td>7. Chipewyan (ditto)</td>
<td>-kòih</td>
<td>-kè</td>
<td>-kè</td>
<td></td>
</tr>
<tr>
<td>8. Sarsi “to travel by canoe; to go for trade”</td>
<td>-kàh</td>
<td>-kè</td>
<td>-kè</td>
<td></td>
</tr>
<tr>
<td>9. Ts’ets’aut “to travel by canoe”</td>
<td>-ki</td>
<td>-kè</td>
<td>-kè</td>
<td></td>
</tr>
<tr>
<td>10. Chasta Costa (ditto)</td>
<td>-xìw</td>
<td>-xiŋ</td>
<td>-xìl</td>
<td>-xe’</td>
</tr>
</tbody>
</table>
12. Mattole "to travel by canoe" -k̕i:x -k̕iŋ -k̕i:l
   (relative form)
13. Kato "several bathe" -k̕e'
   (transferred from cont. pf., it., and opt.?)
14. Chiricahua Apache "several run, trot" -k̕e -k̕i -k̕e:l -k̕e
15. Navaho (only as survival in obscure forms) -k̕e:h -k̕i -k̕e:l
   (read -k̕e:h?)

The history of the meaning of these verb stems is now reasonably clear. The primary meaning of the Athapaskan verb stems may have been "several objects (or persons) move in the water, float" (see Hupa and Kato above), whence "the group travels on the water, to travel by canoe (as one of a canoe-party)." Both meanings are preserved in Hupa. The latter meaning, however, may well have been the primary one. The specific meaning of a group traveling by water seems, under changed environmental conditions, to have taken on a new meaning in Chiricahua Apache ("several run, trot"), though the old plural or collective implication is still preserved. In Sarsi the meaning of "to travel by canoe" is now felt to be rather archaic and to belong to myth and story. The natural meaning today is "to go to trade, to go (by foot or horse-back) in order to shop;" this is developed from "to go by boat (or canoe) in order to trade at a Hudson's Bay Co. trading post," itself specialized from the common Athapaskan meaning "to travel by canoe." Here too the gradual passage to a typical Plains life, with little or no use of water craft, has brought about a redefinition of a familiar set of words. The Navaho words seem to stem from an old meaning "to travel by canoe," naturally entirely effaced from tribal memory. A generalized meaning "to float," applying to singular as well as plural subject, cannot be assumed for early Navaho because there is no evidence anywhere in Athapaskan for a reflex of *-k̕e-nx in the sense of "one person floats" and because all Athapaskan dialects are peculiarly sensitive to the difference between singular and plural forms of verb stems referring to characteristic types of movement. "I become sleepy," in other words, seems originally to have meant "Sleep paddles away from me;" the Owl was ritualistically described as "he who brings Darkness back in his canoe." Such locutions seem to stem from a cultural setting in which travel by canoe was so much a matter of course that it could be transferred to the supernatural world.
The Navaho ná·ké'l "he comes 'gliding' home" (of which ná·kél in the sacred name of the owl is the relative form) is contracted from an old Athapaskan progressive *ná·γé-(dē·)-ké·nél, of which there are exact reflexes in many of the other dialects, e.g., Sarsi ná·γé-kál "he's coming back on a boat, he's returning from shopping;" Beaver na·γa·kil (read -kel?) "he is paddling back;" Carrier na·s·kel (contracted from *ná·γé·ś·ké·nél) "I am again navigating, I am returning by boat;" Ingalik nö·γö·šö·kal "he paddles again."

The evidence collected in this paper may now be summarized. (1) It is assumed that there is important external linguistic evidence, distributional in character, to provide a prima facie probability of the northern origin of the Navaho and Apache. All the Southern Athapaskan dialects (Navaho, Western Apache, Mescalero and Chiricahua Apache, Jicarilla Apache, Lipan, and Kiowa Apache) obviously form a close-knit dialectic unity which contrasts with the more complex dialectic ramifications of Pacific and Northern Athapaskan. The geographical center of gravity of these languages, in short, lies in the north. (2) If we could find internal linguistic evidence in Navaho, of cultural implications, tending, as it were, to free Navaho and Navaho culture from their present Southwestern environment, the initial probability of a northern provenience would be strengthened. Such supplementary strengthening of an inherently probable hypothesis is suggested by the linguistic analysis of four Navaho words having cultural connotations. The cultural inferences that may be derived from this analysis are: that the gourd was not originally an element of Southern Athapaskan culture; that spoons in this culture were originally made of horn; that broadcast sowing of seed was foreign to the culture; that maize, a staple in historic times, was at one time felt to be an alien food—in other words, that the Southwestern agricultural complex was originally lacking; and that a glimpse, faint but not to be lightly argued away, may be had of a time when the Navaho, or Southern Athapaskans collectively, made use of canoes. (3) All of these inferences deepen, in a historical sense, the cultural gap between the Navaho and the Pueblos. This gap is already given, in a descriptive sense, though in lesser degree, by the modern ethnologic evidence. The first four of the cultural inferences we have listed are theoretically compatible with a non-Pueblo Southwestern cultural setting and, equally, with a

more northern setting. The last of these inferences, if valid, points more positively to a northern setting.

"Northern origin" does not in the least imply a direct line of movement from north to south across the Great Basin. Such a line of migration is most improbable. It is far more likely that the movement of these peoples proceeded via the western plains. If this is correct, an analysis of Southern Athapaskan culture would aim to reveal four strata: a fundamental northern layer, comparable to the culture of the tribes of the Mackenzie basin; an early western Plains adaptation, more archaic in its outlines than the specialized culture of the Plains as now defined by ethnologists; a first Southwestern influence, tending to assimilate these tribes to the relatively simple non-Pueblo culture of the Southwest; and a second, distinctively Pueblo, Southwestern influence. To these must, naturally, be added a good deal of Navaho specialization on the basis of the Pueblo influence. The disentangling of these various layers is work for the future and, in any event, is hardly likely to be ever more than fragmentary. Meanwhile, the geographical sequence: Chipewyan, Sarsi, Kiowa Apache, Jicarilla Apache, Navaho, may stand as a suggestion of the reality of the historical problem, though, no doubt, the Plains character of Sarsi and Kiowa Apache culture is in each instance of a much later type than the hypothetical Plains influence to be worked out for Navaho cultural antecedents.

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NAVAHO DIAGNOSTICIANS

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IN the highly specialized medical practice of the Navaho Indians, when the cause of disease cannot be determined by obvious symptoms, diagnosis is accomplished by divination. This is carried out by individuals who may or may not be hatá’íí (medicine men) as well—more often not. Tozzer² and Hill⁴ have presented brief discussions of one method (motion-in-the hand). A more extensive study of Navaho divination and its practitioners was the subject of a report by the late Dr William Morgan,³ but since in my observations I have found points at variance with his report, and have gathered additional data, a further record was deemed useful. Although the practitioners of divination may use their art to determine the locus of lost or stolen articles or animals and to identify unknown workers of witchcraft—as the Navaho say, "to find out things they do not know"—they are most commonly called to act in connection with sickness; and so the term "diagnostician" as used by Dr Morgan seems fitting. In fact some of these practitioners function in case of sickness only.

In ethnological discussions one too often finds the statement that so-and-so was "wrong" or "misinformed" because he had reported things at variance with what may have been found at another time or place by another investigator. It has been my experience, and also that of certain other students, that the usages of the large and widely scattered Navaho tribe now vary markedly, so far as both individuals and geographic localities are concerned. A somewhat detailed account of the nature of the informants and interpreters and of the locality where information was gathered should therefore be given in any report on the Navaho.

Information for this report was obtained from six sources (personal names are withheld for obvious reasons): an English speaking medicine man of middle age, able to give several small chants, very well informed as to medical practice in gen-

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¹ It was possible to gather the information for this paper during the pursuit of a problem in Navaho medical ethnobotany, which is being carried out with the financial assistance of the Chemical Foundation of New York. Grateful acknowledgment is therefore made to this Foundation.

² The orthography of the Navaho words is that of Dr Gladys A. Reichard, to whom I am deeply indebted for much assistance.


eral, extremely co-operative and without guile, who also acted as interpreter for three other sources; an educated young man whose father is an active practitioner of motion-in-the-hand; two middle-aged practitioners of motion-in-the-hand; an elderly "man who knows the [therapeutic] plants," also well informed concerning medical practice; and an old medicine man, now partially blind, able to give numerous small chants and to assist with the larger ones, very well informed as to practice and legends in general, and a practitioner of star-gazing for many years. The last four informants do not speak English. These men live in the southeastern corner of the Eastern Navaho Jurisdiction, in the northeastern corner of McKinley County, New Mexico; specifically, near Mariana Lake, Pinedale, Perea, San Antonio, Smith Lake, and Casamera Lake. In addition I witnessed several performances of motion-in-the-hand including one given for myself.

There are three types of divination recognized in this area of the reservation: ņidlnih or motion-in-the-hand (also sometimes called tiňlé-i, Gila monster, because this animal is the spirit of motion-in-the-hand), so' nil'i or star-gazing, and ist'sâ': or listening. Associated with star-gazing are djóhona-ńíł'i or sun-gazing and moon-gazing. The Navaho have no word for divination in general other than bił 'ihosiní-h, "that which he knows." Motion-in-the-hand predominates in most of the localities investigated, although it was said that star-gazing is more common in the vicinity of Mariana Lake. Listening was common a long time ago, but today there are very few listeners: in some places the practice has essentially disappeared. Although most diagnosticians know only one kind of divination, it is possible for one man to know and practice all three; and star-gazers are often acquainted with sun-gazing and moon-gazing as well. Women commonly practice motion-in-the-hand, but not star-gazing, although it was said that "if there is a smart woman she can go out with the star-gazer to help." A few women used to do listening. The kind of divination selected by the client seems to be merely a matter of availability, for all kinds are of equal standing, and since motion-in-the-hand predominates this is usually tried first. If the patient is not benefitted by the recommended treatment a star-gazer may then be called, although another trial of motion-in-the-hand may be resorted to. Although it was said that if a patient is "wise enough" he will employ a diagnostican early in his illness before he gets a medicine man, the usual practice is to call upon one after remedies or the services of a medicine man have been tried and have failed to produce the desired progress. Sometimes a medicine man is not satisfied with the results of his efforts, and will recommend to the family that they call in a diagnostican to discover what is wrong. The function of the diagnostican is to discover not only the cause of the illness but also to recommend the
treatment to be used (sometimes he recommends actual therapeutic measures, although usually he simply tells what kind of chant should be sung over the patient), and to recommend a practitioner who can apply that treatment. This sometimes results in a change of medicine men during an illness. The Navaho believe that there may be two untoward influences at work in the case of certain ailments: the obvious cause, such as an accident causing broken bones, and an unknown influence which might attack the patient in his weakened condition and which is waiting for just such an opportunity because of some previously transgressed taboo. Therefore a medicine man may be employed to treat the obvious symptoms; then, after he has worked a while, a diagnostician may be called to discover the second influence and to recommend another medicine man who can deal with it.

MOTION-IN-THE-HAND

It has been said that before the diagnostician begins to work he discusses the patient’s condition with the family or with other bystanders. All my informants insisted that the diagnostician need not know anything about the case before beginning, and that he always goes to work without preliminary gathering of information. They seemed surprised when I suggested such a thing, saying that he “does not need to” since the information is supposed to come through supernatural means. On each occasion where I saw a man-with-motion-in-the-hand work, including one performance for myself, he started the ritual without preliminary discussion. In each instance, however, he had been around enough to gather casually about as much information as he could obtain by further discussion.

After a fee has been stipulated, which is usually small—such as a ring, bracelet, moccasins, fifty cents, or a dollar—the man-with-motion-in-the-hand washes his hands and forearms and, with one arm bared to the elbow, sits cross-legged near the patient. In my case he first placed corn pollen upon me in ceremonial order from right foot to top of the head, but this is ordinarily omitted. Then he sprinkles pollen from his right elbow along the radial margin of the forearm, around the hypothenar eminence, and along the palmar surface of the thumb to its tip; from the tip of each finger along the palmar surface to the center of the palm, beginning with the middle finger; and, finally, in the center of the palm. A variant of this procedure is to draw the line from the elbow to the thumb in the form of lightning with four angles, with the apex of the first angle near the elbow, directed toward the ulnar margin of the forearm. Another is to draw a line of pollen from the hypothenar eminence to the ulnar edge of the palm, after placing pollen on the fingers and before finally depositing some in the center of the palm.
Most diagnosticians use the right hand; at least they always start with the right hand. One informant said that a few might use the left hand if they are used to it. Sometimes they begin with the right hand, later clap the hands together, and then use the left hand, letting the right hand rest.

Ordinary corn pollen is used; but if a Gila monster can be obtained it is placed on a buckskin, pollen is sprinkled over it, and then gathered again. This constitutes “live pollen” and is considered more effective.

Sometimes when a sand painting in which four tińlé-i or Gila monsters are depicted (black, blue, white, and yellow) has been made during a five day wind chant, a little sand from each Gila monster is picked up and saved. A man-with-motion-in-the-hand may later obtain this from the medicine man who gave the chant, and sprinkle it on his arm in place of pollen. When this is done, pollen is not used in addition. One informant said that the powder of hadahó’niye’ or striped stone fetishes, ground on a rock, could also be used in place of pollen. Sometimes if the diagnosticians are wavering between two decisions he may decide to try it over again immediately. In this case, three or four minutes after completing one performance, he rubs ashes on his hands without another application of pollen, and begins again.

Contrary to what has been reported before, all informants emphatically stated that except for the material sprinkled on the hand no equipment, such as fetishes, prayer sticks, etc., is ever used by the man-with-motion-in-the-hand. Such was the case in the performances which I witnessed.

The diagnosticians next says a prayer in a low voice, which is usually short, although longer ones may be used. This prayer is said to be always the same, and is directed to the Gila monster spirits, asking for information concerning the disease and the proper methods of curing it. One informant said that the prayers might vary somewhat but always end the same way. According to the origin legend, the Gila monster, tińlé-i, which “knows everything and sees everything,” first showed the people how to do motion-in-the-hand. It was said that motion-in-the-hand is known to have come from the Gila monster, because if you watch this lizard when he walks you will see that his forefoot shakes when he raises it from the ground. Ceremonially there are five kinds of Gila monster—black, blue, yellow, white, and spotted—and these are represented in sand paintings in certain chants. They are diyin diné’i or holy people. The prayer is said to all five. It is my opinion that the variously colored tińlé-i are not actual lizards, and that the only one which exists as an animal is the tińlé-i ikij or spotted Gila monster. Some women do not know the prayer; in that case, and in the case of beginners, a medicine man may pray for them. A brief translation of the Gila monster prayers and songs was given by Hill.
Immediately following the prayer a song is begun. Although some informants said that special songs which are always the same should be used, there is apparently some latitude in the choice. The standard ones are tinf-i biyi-n or Gila monster songs (it was said that there are fifteen or sixteen songs to the Gila monster and that about five are usually sung); but mountain songs, buffalo songs, or hojódjí or songs of blessing may be used. The number of songs completed depends on the length of time necessary to perform the rite. In two performances which I witnessed the rite following the prayer lasted about six or eight minutes and only two songs were sung. If the diagnostician does not know the songs, anyone present who does know them may sing for him. It happens much more frequently that the man-with-motion-in-the-hand does not know how to sing, and has to have others help him, than that he has to have a prayer said for him. I know two practitioners of many years standing who still do not know the songs although they do know the prayers.

During the prayer and thereafter the diagnostician sits with eyes closed and face averted, and as soon as the singing begins his extended hand usually begins to shake. Although it was said that the motion of the hand may begin any time, even during the prayer (in which case the prayer is discontinued and a song begun), it more often than not begins at the start of the song. It was also stated that if the motion-in-the-hand is a new acquisition it starts easily, but if it is old there may be a little difficulty in starting it. While the hand is moving the diagnostician thinks of various diseases or causes of disease. When something happens which tells him that he is thinking of the correct one, he then thinks of various chants which might cure the disease; then, of what medicine man might be the best one to give the chant; then perhaps of plant medicines or other therapeutic measures which might be used. After all the desired information has been divined the shaking stops of itself, the singing is discontinued, and the diagnostician opens his eyes and tells those assembled what he has discovered.

It is difficult to describe the motion of the hand and arm. It varies from a fine tremor with the extremity held in one position to rather violent motions of the whole arm through large arcs. The various types of motion can best be recorded in the following description of how the diagnostician arrives at the correct diagnosis and prescription. It must first be understood that it is claimed that the motion is involuntary—it starts and stops without volition—and that since the diagnostician closes his eyes and turns his face away from the patient, he does not see what his arm is doing. One informant said that if you do not believe this the man-with-motion-in-the-hand will let you hold the shaking hand, and then the other hand will begin
to shake; and that if you hold both his hands then his whole body will shake.

A common event is for the forefinger to draw a line in the sand of the floor, while the diagnostician is thinking of a cause of disease. If it is not the correct cause the hand rapidly erases the line with a brushing-away motion, and another line is drawn while thinking of another cause; and so on until a line is drawn which is allowed to remain, and the hand performs a rapid pointing or patting motion toward the line. This type of motion tells the diagnostician that he is now thinking of the correct cause, so he ceases to think of causes of disease and begins to think of various chants to cure the disease. Disease is located in the body of the patient by feeling, grasping, or patting various parts of his body, and again a rapid patting motion of the hand on a particular part tells that the disease is located in that part. An informant said that the patient "does not have to tell where the pain is, but you can feel it when you are doing it [motion-in-the-hand], and when you quit and ask him if that is the place where he has pain, he will say 'yes'." The proper medicine-man to treat the patient is selected by thinking of various men who know the selected chant, while the hand points in the directions in which they live. A persistent pointing motion in one direction tells that the man to call is over there.

One method used to discover the cause of sickness and the chant to cure it is interesting in that it is strikingly similar to automatic writing. A picture is drawn in the sand by the moving forefinger; then the diagnostician opens his eyes, looks at it and sees that it is symbolic of some chant or is like a fragment of a sand painting used in some chant. He then knows that it is this chant which is causing the trouble.

To understand this, one of the Navaho theories of disease must be understood. If a Navaho woman is pregnant (five, six, or seven months or more) and she or her husband goes to a ndá'ý or war-dance and sees there the rattle-stick or the scalp, or the man who "kills" the scalp, then the unborn baby may later suffer from sickness associated with the ndá'ý and have to have the same chant, i.e., the ndá'ý, to cure it. The malign influence may not attack him until he is as much as sixty years old, or it may begin to operate when he has suffered from some accident and is thus in a weakened condition. The same applies to other chants, to seeing dead animals, especially those struck by lightning, or to eating the flesh of animals which have been exposed to lightning. The lightning influence is especially malign, and an osni'djí hatał or lightning chant is needed to avert it. The malign influence from any particular chant is removed by having the same chant sung over one. All this applies to similar happenings during one's life as well as to
prenatal influence. Following is a list of such symbols or symbolic actions.

A symbol of the rattle stick used in the ndá’i or war dance; beating on the ground with the hand as if beating a basket, or a circle representing the basket which is beaten as a drum in the mountain chant; putting the hand in the fireplace and picking up ashes or charcoal which is used to blacken the patient in the hotcọdjí hatá’l or evil chasing chant; feeling the body of the patient to locate broken bones or strains which are cured by the i’nádjí hatá’l or life chant; making a motion with the hand as if shaking the rattle used in the i’nádjí hatá’l; a zig-zag symbol of lightning for the lightning chant; an angle pointing upwards representing a mountain for the mountain chant; a crude figure of a man, “a wind person,” for the wind chant; an elongated figure of one of the yé’éi’h as represented in the sand paintings of the night chant; two vertical rows of three dots each like part of the painting on the body of the patient in the goláyáí hatá’l or Chiricahua Apache wind chant; a drawing of a snake, or of an eagle, or pointing to a mountain meaning a bear, or pointing to the sky meaning lightning, or pointing to the sun or drawing a circle representing the sun, for malign influences from these sources; or a crude figure of a lizard like tílélé’i, the Gila monster, represented in a sand painting in a wind chant.

The automatic or involuntary character of these drawings or actions was attested to by all informants. They said that the diagnostician does not know what his hand is going to do or what picture it is making until he later looks at it, and if he looks at it before the picture or act is completed the hand immediately ceases its motion. In my own case the proper treatment for a lame knee (which was to dig a small hole in the ground, place hot rocks in the bottom, cover them with cedar leaves, and then to sweat the knee in this hole) was disclosed by the moving hand of the diagnostician rubbing and patting the ground and finally digging a little hole in the ground with the forefinger, then several rapidly alternating pattings and seizures of my knee, and patting and pointing motions to the ground. The motion finally stopped with his hand resting on my knee.

Sensations accompanying the motion of the hand were described by several informants. They were variously characterized as like “needles in the arm,” “an electric shock,” “hitting the crazy-bone,” “a dash of cold water,” or “as if the arm were asleep.” One man said that such sensations occur in beginners, who interpret it as “the spirit going into the arm.” The sensation moves from the arm to the heart and “moves the heart.” After one has been practicing for a long time the sensations may become painful, especially if one is old or sick. Another informant said that if the sensation occurs in the elbow the patient will die, but if it is felt in the back of the
shoulder he will recover; another said that the sensations are of no signifi-
cance so far as diagnosis is concerned.

Motion-in-the-hand cannot be inherited and cannot be learned. It
comes to one suddenly, like a gift, and is usually acquired at a chant where
there is some doubt about the diagnosis. Anyone present who is sitting and
watching the service may suddenly begin to shake. Then he goes to the
patient and tries to make a diagnosis. If he is successful he then knows that
he has motion-in-the-hand and can practice thereafter. Following this he
may go to a medicine man who knows the motion-in-the-hand chant, but
in the winter only; and for a fee he learns from him the prayers and songs.
These may be learned from anyone who knows them; not necessarily from
another diagnostician. Before the prayers or songs are learned the novice
may practice by having someone else pray and sing for him. There is, then,
no period of apprenticeship for motion-in-the-hand. The art may also be
acquired at a motion-in-the-hand chant, ōdínidí'í hata'í (this is identified
with one of the wind chants), where a sand painting containing Gila mon-
sters may be made or where Gila monster songs are sung. When someone
hears this animal’s songs he may begin to shake.

A typical experience follows. Six years ago a man had many headaches and was
having a chant sung over him in an attempt to cure them. Another man who was
present suddenly felt a pain in his right arm, which traveled from his elbow to his
finger tips, and his hand began to shake. He therefore attempted diagnosis and dis-
covered that the patient had sung the songs of blessing for too many houses
(ho'yan) without first having a similar service for his own home, and that to cure
his headaches he should have this ceremony. This diagnosis seemed satisfactory
and the man who made it has been practicing since that time.

Most informants said that the man-with-motion-in-the-hand can diag-
nose for himself, “if he has strength enough to do it without hurting him-
self.” One diagnostician, however, told me that he had tried it when he had
been sick and that he was unsuccessful. It made him dizzy whenever he
tried it for himself and he had to employ another diagnostician to do the
work.

There are no seasonal or other limitations of the practice of motion-in-
the-hand. It may be done throughout the year at any time and place; but
the songs should be learned in the winter, and the legends concerning it
should be told only in the winter.

It does not follow that because a man is a diagnostician he has anything
else to do with the practice of healing. However, like any other layman, if
he knows anything about the treatment of sickness—such as where to
gather plant medicines, how to administer medicines or other therapy, how
to perform certain chants—he may apply this knowledge himself, or assist a medicine-man, or teach others to do so. Because of the nature of his work the diagnostican has often gathered considerable medical knowledge and he often uses it when called upon. There are some, however, who know nothing about actual medical practice.

STAR-GAZING

Star-gazing differs in some important respects from motion-in-the-hand. It can and must be learned, and anyone who wishes may learn it from a practitioner. It was said that an intelligent man can learn it in a day, although most beginners take longer. The complete ritual of star-gazing is somewhat more complicated than that of motion-in-the-hand, even involving the making of a sand painting, although a briefer ritual without the sand painting is often used. This may be because there is need for immediate diagnosis, because the patient cannot afford the complete ritual, or because the diagnostican does not know how to make the sand painting; for he may practice the briefer ritual without knowing the complete one. The fee is somewhat larger than that for motion-in-the-hand, being a blanket, a horse, ten head of sheep, or ten or fifteen dollars, for a small service. Otherwise the general considerations concerning diagnosis by motion-in-the-hand apply to star-gazing.

In the complete ritual the diagnostican first makes a sand painting in the dwelling (ho-yen), about two feet in diameter. It represents a white star with four points toward the cardinal directions. Between the points of the star are four heaps of sand representing mountains, the southeast mountain being white, the southwest blue, the northwest yellow, and the northeast black. Around the whole, with an opening to the east, is a zig-zag line representing lightning. Then the diagnostican makes ready the dried and powdered lenses from the eyes of the five night-birds with keen sight who acted as lookouts in the legend of how star-gazing was first made known to the people. He dips the tip of his finger in this material and then draws it along his lower eyelids. It is similarly applied to the patient, to the one man who will go out with the star-gazer to assist him, and to anyone else present who is "smart" and may be able to assist by seeing something. The eyes of the five birds mentioned are the main ones, but eyes of other birds may also be used if available. Then the fire is covered and from now on the people who remain inside do not move or make any noise, but they concentrate and try to see something in addition to that which is seen by the diagnostican, sometimes gazing at a star through the smoke-hole.
The star-gazer takes one person with him and leaves the house. Outside he prays the star-prayer (sò' dizin) to the star-spirit, asking the star to show him the cause of the sickness. Then he begins to sing star-songs (hotsò' biyï'n) and while singing gazes fixedly at a star or at the light of a star reflected in a "glass rock" or quartz crystal which he holds in his hand. Soon, it was said, the star begins to "throw out a string of light and at the end of this the star-gazer sees the cause of the sickness of the patient, like a motion picture." If these strings of light are white or yellow the patient will recover; if red, the illness is serious or dangerous. If the white light falls on the house and makes it as light as day around it, the patient will get well. If the house is seen burning or in darkness he will die. If a certain medicine man is the proper one to cure the sickness the star will throw a flash of light in the direction of his home, or on his body if he be present. Places far away may be seen. After the diagnostician has obtained enough information in this way he returns to the house and tells what he has seen. If anyone else has seen anything, his experience is also considered.

Sun-gazing and moon-gazing are done the same way. A star-gazer can diagnose for himself. If he has not yet learned the prayers or songs, someone else who knows them may pray or sing for him while he looks at the stars. It was said that the same prayers, songs, and procedure are always used. One old star-gazer who is now nearly blind says that he can still practice by taking two men with him "to do the looking for him" while he prays and sings, and then making a diagnosis from their report.

LISTENING

Very little material concerning listening was gathered because of the fact, mentioned above, that it has been practically forgotten in the region studied. It is very like star-gazing in many respects, except that the diagnosis is made from something heard rather than from something seen. The origin-legend of listening was said to be the same as that for star-gazing. Listening must be learned, and an intelligent man can learn it in one night. The procedure is like that of star-gazing, but without sand paintings or fetishes. The dried and powdered ear-drum of a badger is used in place of the lenses of bird’s eyes; the listener dips a finger tip in the powder and then places it in each ear. Then he takes one man with him, goes out of the house, prays to "listening," saying "I want to hear, etc.,” and then begins to sing (star songs may be used) and to listen. The cause of the sickness is determined from the characteristics of something heard, such as the rattling of a rattlesnake, the roar of a bear, or thunder. If someone is heard crying, the patient will die.
LEGENDS

There are legends concerning the origins of the various types of divination, but they may be told only in the winter or at certain chants in the summer. This fact may account for the statement that diagnosticians possess no legends concerning their work. A practicing diagnostician need not know these legends; in fact they are more often known by old medicine men or others well versed in the lore of the tribe. I obtained two recountings of short legends concerning the origin of motion-in-the-hand. One narrator said that there are no longer ones, but several others told me that there are longer ones, and it is my opinion that such is the case. Another informant told me a short legend with much trepidation, saying that he did not have the turquoise and shell beads which are obtained at a chant to protect one from lightning, and that he might be struck by lightning if he told such things in the summer. I also obtained a fairly long legend concerning the origin of star-gazing and listening.

6 Morgan, op. cit.
7 Leland C. Wyman, Origin Legends of Navaho Divinatory Rites (Journal of American Folk-Lore, in press).
MAYAN HIEROGLYPHS: Glyph Gs of
THE SUPPLEMENTARY SERIES

By HERMANN BEYER

IN his brief, but very important, paper on the various forms of Glyph G of the series generally following immediately after the day-date of an Initial Series, J. Eric Thompson says: "There is no case of a Glyph G occupying the eighth position." Dr Teeple, who incorporated the results of Thompson's investigation in his comprehensive work on Maya chronology, repeats the statement in slightly different words. A more careful scrutiny of the hieroglyphic material, however, reveals the fact that several specimens of this sign exist. That it escaped recognition is, in the first place, due to the circumstance that this form of Glyph G often is combined with Glyph F into a composite hieroglyph. In one instance a faulty drawing, and in another one an erroneous interpretation of the Initial Series, have impeded its recognition.

In the two examples from Palenque (fig. 1, a, b), the symbol of Glyph G for the eighth day forms the central part of a composite hieroglyph which has to be analyzed as a variant of Glyph F, like c, on which the symbol is superimposed: covering most of the central element, a knot or tie. The peculiar symbol for the eighth day is, in my mind, the tail-end of a mythical serpent. The two Initial Series for a and b each end in 4 uinals—that is, 80 days, which, divided by 9, leave a remainder of 8.

The partly-destroyed Glyph G-F of d has its central part and suffix, as well as the whole arrangement, like a and b. The Initial Series ending terms 13.0 confirm this interpretation, as they yield the remainder 8 when divided by 9. For this case Morley's drawing (fig. 1, e) is incorrect, excepting the first detail of the suffix, the forepart of the main-sign, and the suffix, which are in agreement with the other specimens. A photograph of Stela 10 of Copan in Mr Blom's Index of Maya Ruins shows clearly the outlines

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2 "... Nine forms in order, except one, the form for 8, no example of which is surely known" (John E. Teeple, Maya Astronomy, Publications, Carnegie Institution of Washington, No. 403, 1930, p. 43). "We have no example of No. 8 ... " (p. 44).

3 For our purposes here it is sufficient to be able to recognize its odd shape and to distinguish it from the other eight forms of Glyph G. In another paper I shall treat the question of its original meaning and give more exact drawings for some of them. Here I have reproduced mostly Morley's illustrations, although they are slightly inexact. The symbol is employed also as part of one form of Glyph X of the Supplementary Series, as suffix of the month-sign Cumku and in some other cases.

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of the symbol for Glyph $G_3$, Maudslay’s drawing reproduced in Fig. 1, $d$
being much better than Morley’s arbitrary head-symbol.

In Fig. 1, $f$ the symbol for day 8 covers the entire middle part of the
compound, but the displaced tie now is employed as suffix. The superfix
is that of Glyph F of the Supplementary Series. The Initial Series of the
monument (Lintel 21 at Yaxchilan) ends in 2.4 Maya notation, that is, 44
in our decadic system. Forty-four contains $4 \times 9$ and a remainder of 8.

Thus we have four cases where Glyphs $G_3$ and F are fused into one com-
 pound character. But there are also two examples known to me where $G_3$
forms a separate unit adjacent to Glyph F.

![Fig. 1. a, Glyph $G_3$-F (Palenque, Temple of the Cross, A10); b, Glyph $G_3$-F (Palenque,
Temple of the Foliated Cross, B9); c, Glyph F (Piedras Negras, Stela 25, A10); d, Glyph
$G_3$-F, Maudslay’s version (Copan, Stela 10, A4b); e, Glyph $G_3$-F, Morley’s version (Copan,
Stela 10, A4b); f, Glyph $G_3$-F (Yaxchilan, Lintel 21, B4); g, Glyph $G_3$ (Copan, Stela 12, A6a);
h, Glyph $G_3$ (Piedras Negras, Stela 14, left side, G2a).](image)

In Fig. 1, $g$, Glyph G of the Supplementary Series from Stela 12 at
Copan is reproduced after Morley. He reads the Initial Series as 9.10.15.0.0,
6 Ahau 13 Mac,\(^4\) completing the fragmentary superfix of the month-glyph
into that for Mac. This fragment, however, can be reconstructed as Zac
just as well, and in this case the ending terms agree with Glyph $G_3$. Sup-
posing 9.10.2.13.0, 6 Ahau 13 Zac the Initial Series, the remainder 8 is

\(^4\) Sylvanus G. Morley, The Inscriptions at Copan (Publications, Carnegie Institution of
reached, while Morley’s date renders 0. The head-numerals in the photograph reproduced by Morley and his drawing are indistinct, but at least they do not contradict the reading proposed here.

A fairly clear example of G₈ not incorporated into Glyph F occurs also in the inscription of Stela 14 from Piedras Negras (fig. 1, h), now in the University Museum of Philadelphia, where I had an opportunity lately to inspect it. Although the head-numerals of the Initial Series and the month-glyph are indistinct, the reading 9.18.0.3.1, published by Teeple,⁵ seems to be correct; at least the lunar count of the Supplementary Series agrees with it. In this case we have, then, to assume an error of the Maya chronicler or of the sculptor, since the remainder is 7 instead of 8.

It is regrettable, certainly, that of the two examples for the detached Glyph G₈ one appears in a disputable series and the other probably is a blunder of the ancient chronicler. I think, however, that the main point, the establishment of the characteristic symbol for the eighth “acompañado” is warranted by the other, reliable, data. With the new symbol the series of the Nine Lords is now complete, and we see that practically all Initial and Supplementary Series are arranged in the same manner. In other words, consistency now is proved for cases which formerly seemed to be exceptions.

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THE LINGUISTIC EXPRESSION OF EMOTION

By JULES HENRY

STUDENTS of philosophy, psychology, and language have frequently urged the relationship between the formal categories of language and the modes of thought of the people speaking them. An attempt to establish such a relationship should, however, take account of the fact that the categories of any language are limited in number, and that although they are limited, the infinite variety of experience must be crammed into them. For this reason the grouping of a particular body of phenomena in a given category may not necessarily be significant as an index of a people's view of the natural world. Although Kaingang expresses color to some degree by means of the same mechanisms it uses to express behavior,¹ this does not mean that the Kaingang Indians of Brazil view color as an activity of the natural world. An analysis of the structure of Kaingang shows, rather, that all conditions of things must be expressed either in the category of definition or in the category of activity. The fact, therefore, that color falls into the active category is without significance for a study of Kaingang world-view.

In many languages direct relations between the formal categories and cultural life and thought are extremely difficult to trace because of the extremely formal character of the linguistic categories we find. A good example of a language whose categories have a direct and immediate bearing upon every-day life and interests is Quechua,² where

... the main preoccupation ... is to avoid any mistake in the nuance of the phrase. This is most apparent in the verb formation, where, with the help of the verbal suffixes an extraordinary amount of variation of the same verb can be attained. And though Runasimi (i.e. Quechua) is, grammatically speaking, a very simple language, it is nevertheless extremely rich and precise. . . . [These verbal suffixes] . . . vary the intensity, place, duration, purpose, and so on, of the action, and may be combined practically without limit. . . .³

A few examples of these suffixes are: -raya, to doubt the execution of an action; -yu, immediate action, short, but calm and not sudden; -çaku, to execute the action in a ridiculous manner.⁴

Although the categories of a language and its general content may make

³ Ibid., pp. 157, 163.
⁴ Ibid., p. 164.
no provision for certain groups of ideas and feelings it does not follow that
the people speaking these languages cannot and do not express them every
day of their lives. The categories and lexicography of Quechua provide
for feelings and ideas that are expressed with great facility and subtlety
in other languages by means of intonation, gesture, and facial expression.
Kaingang is a striking example of a language, which, although devoid of
formal devices for the expression of degree or intensity, gives constant and
vivid expression to these concepts. By means of changes in pitch and
force of articulation, through modification of sounds by unconscious con-
traction of the pharynx and unconscious changes of the vowels; by means
of changes in facial expression and bodily position, Kaingang achieves a
richness of color and flexibility, which could never be inferred from the
manifest content of the language. This is no mere statement of my sub-
jective feeling, for Kaingang rhetoric reaches extremes of uninhibited
exuberance never dreamt of in English.

The Kaingang do not know the story of "The Magic Flight." When
I told it to them they became very fond of it, and one evening I had the
pleasure of hearing an Indian tell it by the campfire. In order to illustrate
how slowly the pursuer was forced to travel when his quarry caused a
forest to spring up behind him, the story-teller's voice rose until it became
high-pitched and almost feminine, and he articulated the sentences very
slowly. It seemed to me that the story-teller's style was unusually good,
and I found it very funny. But his Indian audience sat there, smiling a little
but not laughing heartily as I did. This was because the devices of the
story-teller were commonplace rhetorical forms of the language. The
Kaingang always raise their voices when they are describing some long
drawn-out activity, and their voices even take on what might be to us a
complaining tone. When I described a hunting trip to my students at
Columbia they asked me what I was saying when I used the complaining
tone. It was the usual tone to describe the slow climbing of a hill.

In telling their bloody feud tales the Kaingang delight in recounting
the how and the where of all the wounds received and all the blows given.
They cannot say "He struck him very hard," because there is no gram-
matical device for expressing "very hard," but when they want to convey
this idea they utter "strike" with remarkable force. The initial aspirated t
becomes violently explosive and the terminal nasally exploded g becomes
unusually explosive and resonant. When I first heard these high-pitched,
explosive articulations I thought my informant was being humorous, but
his intense excited face showed me that he thought the events he was de-
scribing anything but funny.
In the cases I have described the face assumes a curious expression, which I can imitate but which I could not adequately describe. For a study of these features it is necessary to have a recording device and a camera. The changes in facial expression are just as formalized, just as much part of the communication in many instances as are the formal categories of the language. The same is true of intonation and gesture. Idea, intonation, facial expression, and gesture are a meaningful unit.

Relationships between emotional life and linguistic forms are much clearer and easier to find in syntax than they are in formal categories. It is a commonplace that syntactical changes in sentences may fundamentally alter meanings. This proposition is, indeed, so obvious that in studies of primitive languages its implications have frequently been overlooked. Nevertheless a white man speaking a primitive language cannot but be struck by the tremendous importance of the proper use of various syntactical forms. In Kaingang the grammatical categories are, to a large extent, purely formal, but it is much easier to learn to manage them than it is to learn to manipulate well the subtler details of syntax. Upon one's knowledge of how to manipulate sentence structure depends one's ability to slide out of a "tight corner" at the right time in a situation of conflict, to "shilly-shally" with proper delicacy, and to be elaborately vague at the right moment. A good example of this in Kaingang is the use of indirect speech forms.

In Kaingang it is possible to make sentences with neither subject nor object, so that one can talk about things and people without mentioning them at all. I can say that a tapir was shot or that my clothes were stolen without once mentioning who shot the tapir or what was shot, and without mentioning what was stolen or who stole it. This characteristic of the language enables people to talk about disagreeable things without mentioning what or whom they are talking about. The following examples are taken from texts and represent a few of the many instances of this kind that occur.

Ndukho becomes insane and tries to kill his wife by dropping his lance down on her from a tree. She, however, suspects him. When she hears the lance falling she slips around to the other side of the tree and says, "Why was a lance thrown at my head?" Her husband answers, "My lance fell from my hand." By failing to mention who threw the lance the wife avoids directly accusing her husband.

In an origin myth Chu vai meets his enemy, Nggooyecho, and says to him, "Now there will be fighting." This is the very approximate translation of the Kaingang sentence, which has neither subject nor object. In this difficult situation the protagonist avoids plumbing the depths of the issue by falling back on the impersonal mechanism his language provides him.
Kuven's wife, Kucho, was angry with him because he was sleeping with her co-
spouse, Waichugn, instead of with her. So she made fun of him by saying, "He is in
the habit of practicing cunni linga." This so enraged him that he could not sleep.
Next morning when he arose he said, "It was said that I practice cunnilingus, and
I was thinking about it all night long." Whether he used the form without the sub-
ject to avoid making himself ridiculous or to avoid bringing Kucho's anger against
him again, he took refuge in the obvious mechanism his language offered.

These instances from texts would, perhaps, not be sufficient in them-
selves to demonstate the point were it not for the fact that the Kaingang
are indirect even when they do mention subjects and objects. They allude
to subjects that are only remotely connected with the true subject, for
often the true subject is too delicate or sensitive a point. It is difficult
to follow quarrels in Kaingang society, for very often the principals will
not even mention the thing they are quarreling about.

Wanyeki's wife, Angglo, would have nothing to do with him, but went around
with Kanyahwe surreptitiously. Once Kanyahwe went to work for the Agent for a while
and Angglo followed him while her husband remained at the Indian settlement.
The next night I heard a terrific quarrel between Wanyeki and his wife, both of
them shouting at the tops of their voices. Wanyeki was berating her for "working
elsewhere"!

In a story Vuktha takes away Thuli's wife. Thuli does not know that his wife
has decided to leave him. He comes back to camp with some honey and says to her,
"Mix some of this honey with water for me and give it to them [his children]." She
replies, "Continue to live without the thing you talked about [i.e., continue to
live without having the honey mixed by me]. This man here [Vuktha] has said he is
married to me." When Thuli hears this he becomes furious. He turns to her father
and says to him, "That being the case, be angry. I have waited after saying 'Give
out some of the honey for me' until I have become angry." In this little extract
from a much longer text we can see how all the events, from the divorce of Thuli to
the hatching of the plot to kill Vuktha ("be angry" is a request for murder) are
brought about purely through the use of indirect speech forms.

Beside formal categories, syntax, and the rhetorical forms of intona-
tion, gesture, rate and force of articulation, there are other features of
language which seem not to be wholly concerned either with categories,
syntax or rhetoric, nor yet with all three combined. These features I
should like to call symbolic. In many languages there are words which have
implications, and hence emotional effects, whose sources cannot be dis-
covered simply from a knowledge of the definitions of the words themselves.
Such words have emotional aura, which are by no means evident from
their definitions. Examples of this in English are "liberty," "equality,"
and "democracy." These words, because of their cultural history and
particularly because of their associations with the history of the United States have a tremendous effect upon any American audience hearing them. It must be admitted that this effect is out of all proportion to the actual meaning of these words. Similar phenomena occur on the primitive level, but their roots may often lie so deep as to be undiscoverable except through a thorough knowledge of the people. In Kaingang a good example of such a word would be to ōnù, "to be angry." In order to make its symbolic content clear, however, it is necessary to first describe an important aspect of Kaingang personality.

In Kaingang society fear very often changes to anger. A frightened person may either fly from the cause of his fear or turn upon it in a towering rage and destroy it. I have heard them describe an encounter with a snake, an animal which they fear even more than the tiger: "He [the man] became angry [with the snake] and killed it." In the feud situation in which two groups live together in mortal terror of each other the tension results in murder and an emotional display that is described and felt as anger.

The following is an extract from a story recorded in text:

Kakthi was visited by a tiger. Her dog barked at the noise of the tiger's approach. The tiger stroked Kakthi's feet and awakened her. She spoke to the tiger and it answered her, saying, "When I came your dog barked and I became frightened. I became angry. Then I came at night." Then the old lady said, "Why do you come?" "For no reason. I come to see you. I am going away. I'll probably go that way. When I go up the mountain you will hear my cry and become afraid. If you send your dog after me I'll become afraid of him and I'll be angry and go away."

The above text is a clear expression of the fear-anger equation among the Kaingang. The following is equally clear:

When Patkle died Kangdadm built a pyre for him and tried to burn him, but Patkle fell off the pyre when only his hands and feet were consumed, thus showing that he was supernaturally dangerous and a terrible threat to everyone. Because of this, the story runs, "they [his relatives] became angry and went away. Four days later I [the informant] went and put his bones in the ground. They feared him and therefore they went away. He is vai [supernaturally dangerous] to them and they may die. He showed them that they were going to die right there. That is why he fell in the direction of the place where they were going to die. Then they became angry. 'He is supernaturally dangerous (vai) to us,' they said.'"

The Kaingang are very much afraid of the souls of the dead, because the dead are supposed to carry off the living to keep them company in the other world. Vomble said to me, "When a man's wife comes to him he will die. He will go with her soul. He becomes angry."
A story relates how Kangglagn was visited by a soul. When the soul spoke to her and told her that it had come to take her away, she fainted. Her companion, Nggemu, shook her, and when at last Kangglagn recovered Nggemu asked her whether she had seen a soul. Kangglagn said she had. "Then Nggemu became angry at the soul because the soul might come again," and if it came again it would be with only one object, to take someone away with it, because that is why souls visit mortals.

Teneyu was visited by her brother Ndili's soul. It said to her, "'I have come to call you and your husband.' Teneyu became angry with him. She said, 'No! You are a different thing and yet you come to me! They killed you and I wandered around suffering, but you come to call me!'" The soul then delivered a long speech to her and finally went away. "She [Teneyu] was all green. She stood there without saying anything. He [her husband] said to her, 'Why are you green?' But she said nothing. She sat down holding on to a post. Then he asked her if she was sick, if she had a pain in the belly. But she did not talk. Wanengglo [her husband] asked her whether she had seen a soul. 'Probably she saw a soul.' But she did not say anything."

These examples are excellent illustrations of the close relation between anger and fear in the Kaingang personality. Had the tiger heard the dog barking behind him he would have known that he was being pursued by those threatening his existence. When Patkle did not burn, his relatives took it as an omen of supernatural danger. Nggemu and Teneyu were afraid that the soul would take them away, i.e. kill them. The reaction in all of these cases was fear-anger, one emotion with two facets.

If we turn now to an analysis of the expression to ñu we will be able to understand certain peculiarities of its use and the emotional response elicited by such use. The element to is a postposition meaning "direction toward" and ñu is the element that expresses anger. In this case ñu is treated as a verb. The element ñu also occurs alone, however, and means "dangerous." Thus, a sentence with the expression to ñu means "(He is) angry," whereas a sentence with ñu alone means "(He is) dangerous." Thus to ñu has in it something of directed danger, and ñu has in it something of undirected anger. Hence, the statement "I am angry with you" suggests that "I am dangerous toward you." In line with their use of indirect speech in difficult situations, conspirators in a murder plot do not say, "Let us kill them," but say instead, "Let us be angry with them." When Thuli asked his father-in-law to "be angry" he was asking him to commit murder.

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4 She turned greenish-yellow, a typical symptom of extreme fear.

6 I have called to a post-position because it must follow directly the noun to which it refers. It is separable from ñu. See A Kaingang Text, p. 192.
With this background we can understand why telling a Kaingang you are angry with him, or even using the intonation associated with anger will make him angry with you. If you say to A, "I am angry with you," his reaction is not contrition or repentance, or any kind of "negative self-feeling," but rage. This happens because even though he may know that you do not intend bodily harm, there is an aura of danger about anger, and danger creates fear, which in turn begets anger. Thus the expression to flu, "to be angry," which objective analysis shows to mean "directed danger" has a symbolic content which is discoverable only through an understanding of the Kaingang personality.

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A TRINCHERA NEAR QUITOVAQUITA, SONORA

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STONE walls on many of the hills of Papagueria¹ have excited the interest of workers in that area for a number of years. The wall to be described below is of interest because it indicates a westward extension of the known range of the builders of these structures.

One mile southeast of Quitoavaquita, a Papago town on the Arizona-Sonora border, the Sonoyta River enters a range of granitic hills, locally known as the Cerritos de Agua Dulce.² At the point where the river enters the hills is a small isolated knoll separated from the main cerrito by an abandoned river channel about twenty feet above the present channel. On the east side of this knoll is a wall about 300 feet long, three feet high and two feet thick. Between this wall and the slope of the hill, material resembling sheetflood wash has accumulated or been placed. No other structures of any sort were found on or near this knoll.

The major portion of this wall has fallen, although it is firmly built. Earthquakes, which are, and have been for many years, common in this area, are probably responsible for the falling. Only a few hundred feet upstream, the bedrock in the river bed has been faulted so that the east (upstream) side is elevated four feet. This faulting occurred recently enough so that the broken edge of the uplifted portion is still sharp, but not within the last few years, as the scarp has been eroded away in the unconsolidated sediments close by.

This wall closely resembles the walls on the various “Cerros de Trincheras” described by McGee,³ Lumholtz,⁴ Sauer and Brand,⁵ and others, although it is west of the areas from which trincheras have previously been reported. Inquiries at Quitoavaquita produced the information that this wall was built before the coming of “el Doctor Lumbo” (Lumholtz), and before that of the “good padre” (possibly Kino). Further questions

¹ Papagueria is roughly bounded by the Colorado and Gila Rivers on the west and north, 110 degrees on the east, the Rio de Sonora on the south, and the Gulf of California on the southwest.
² Where the Sonoyta River crosses 113 degrees. For location see W. T. Hornaday, Campfires on Desert and Lava (New York, 1908), map p. 23; Carl Lumholtz, New Trails in Mexico (New York, 1912), map in pocket.
brought only "No comprendo" from the Papagos. At Sonoyta, Alberto Celaya, the Comisario, who assisted Lumholtz during his explorations in Papagueria, knew of the structure, but knew nothing of its origin. No description of this structure has been found in the literature of the area, which goes back to 1698.

Fig. 1. Southern part of trincheria southeast of Quitovaquita, Sonora. View from the east.

These walls have been described as fortifications by McGee and some of the early Spanish writers, but this wall, like many of the others known in Sonora, is poorly placed strategically. Three sides, the north, west and south, are open to attack. Sauer and Brand give evidence that some of the terraces described by them were inhabited, possibly by valley people who were driven from their homes. No evidence of habitation was found at the Quitovaquita trincheria. The structure was certainly not an irrigation dam, for it could not impound water, and there is no water to impound
at this place except during the summer rains, and very little then. It was not a corral, for it is open at the ends, and could not confine any animal more active than a turtle. Lumholtz suggests that the trincheras were religious structures. Considering the location of some of these structures, and their apparent uselessness for any other purpose, this suggestion has its merits. Small pieces of red mericanite lava, found at the base of this wall, suggest a connection with Pinacate, twenty miles distant, where this lava is plentiful. Itoi, one of the major Papago gods, is said to have made his home in a cave at Pinacate, and to have burned away part of the peaks. No mericanite was found between the Quitovaquita trincheras and Pinacate, but in view of the lack of thorough exploration of the area, it cannot safely be said that there is no such lava nearer than Pinacate.

This wall is the most westerly of the trincheras in the Sonoyta valley and probably represents an outpost of the pre-Papago peoples who built them. Until a more thorough study of the outlying trincheras is made, and their relation to the structures further inland is determined, no age, other than pre-Papago, can be assigned to this structure.

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7 Others at Sonoyta, on Sierra de Cubabi southeast of Sonoyta, and at La Nariz.
RECENT ARCHAEOLOGICAL INVESTIGATIONS IN THE SOVIET UNION

By HENRY FIELD AND EUGENE PROSTOV

INTRODUCTION

THE archaeologist who attempts to add chapters or even paragraphs to the history of mankind is faced ultimately with the coördination of his material into the composite whole. During the past decade a wealth of new archaeological data has come to light on every continent. Asia guards carefully many human secrets which can only be unravelled through ambition, patience, and hardship. The discoveries at Chou-kou-tien, Mohenjodaro, Persepolis, Kish, and many other sites are available to every student. On the other hand, the results of archaeological research in the Soviet Union remain almost unknown due to language barriers and to the difficulty of obtaining Soviet publications. There have been constant rumors that considerable archaeological work was in progress in many parts of the U.S.S.R. As a specialist on the physical anthropology of the peoples of southwestern Asia and on the prehistory of that region during the past ten years, I have often felt the need of information concerning recent anthropological and archaeological research in the Caucasus region and in Soviet Turkestan. It had long been my wish to visit museums in the Soviet Union in order to study the collections and to meet the anthropologists and archaeologists, who are doing valuable research work. In September, 1934, 1 accompanied by Richard A. Martin, I entered the Soviet Union at Baku. 2

Six weeks were spent in the U.S.S.R. during our journey through Tiflis, Ordzhonikidze, Rostov, Kiev, Moscow, and Leningrad. The present article is based on collections studied and personal interviews with archaeologists, who have summarized the results of expeditions sent out under the auspices of various institutes. 3 Additional data in the form of archaeological reports, maps, and monographs have arrived from Moscow, Leningrad, Kiev, and Tiflis. Periodicals also are now being received by Field Museum library as exchange publications. The material herewith incorpo-

1 As leader of the Field Museum Anthropological Expedition to the Near East. From April to September I had measured 1,500 men in Iraq and 250 men in Iran. Anthropometric data were also obtained on 50 Yezidis in Tiflis, and, at Ordzhonikidze, on 106 men and 50 women from Northern Ossetia.

2 Through the cooperation of the Department of State in Washington, Ambassador William C. Bullitt, and VOKS our expedition equipment was allowed free entry into the U.S.S.R.

3 Among others, reports were received from R. F. Barton of IAE and A. A. Jessen of GAIMK.

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rated mainly concerns archaeological researches conducted during 1934–1935. The general arrangement of the information has been treated geographically under the headings Trans-Caucasia, Ukraine, Crimea, North Caucasus, European Russia, Turkestan, and Siberia. Maps showing the archaeological sites in Trans-Caucasia, Ukraine, and Turkestan were drawn by Richard A. Martin. Dr A. Sushko of the University of Chicago assisted in the compilation of the Ukrainian section. A detailed archaeological map of the Ukraine was furnished by VUAN. The sites on the maps published here were located with the assistance of Eugene Prostov, who also selected passages from numerous original sources and supervised the transliterations.

This article supplements the report by E. Golomshtok which summarizes the results of anthropological research in the U.S.S.R. up to the end of 1932. In addition to this material the following publications in western languages may be mentioned: “Eurasia Septentrionalis Antiqua” and articles by B. Bogaievski, N. Matorin, and D. Zelenin in a special issue of VOKS (Socialist Construction in U.S.S.R., Vol. 4, No. 4, 1933), devoted to ethnography, folklore, and archaeology in the U.S.S.R.

The following Russian abbreviations have been used:

GAIMK  Gosudarstvennaia Akademiia Istorii Material’noi Kul’tury (State Academy for the History of Material Culture, Leningrad)
IAE  Institut Antropologii i Etnografii (Institute of Anthropology and Ethnography of the State Academy of Sciences, Leningrad)
Sovetskaia Etnografiia (Soviet Ethnography: a bi-monthly journal published in Leningrad by the State Academy of Sciences in cooperation with the Russian [RSFSR] Commissariat of Education in Moscow)
VOKS Vsesoiuznoe Obschestvo Kul’turnyh Snoshenii (All-Union Society for Cultural Relations with Foreign Countries)
VUAN Vseukrain’ska Akademiia Nauk (All-Ukrainian Academy of Sciences, Kiev)
ZOKS Zakavkazskoe Obschestvo Kul’turnyh Snoshenii (Trans-Caucasian Society for Cultural Relations with Foreign Countries)

TRANS-CAUCASIA

Recent discoveries in this area are arranged under two main headings, namely the Paleolithic and Neolithic periods and the Copper and Bronze Ages. Few references to Paleolithic or Neolithic discoveries in Armenia or Azerbaijan are available.

A valuable summary of prehistory in Trans-Caucasia has been published by S. N. Zamiatinin of IAE.

A. The Paleolithic and Neolithic Periods. Information concerning the human culture of the Quaternary period in Trans-Caucasia, according to Zamiatinin, was very limited until the past decade. Although the exploration of caves in Georgia was begun in 1914–1918 by R. R. Schmidt and S. A. Krukowsky, the results of these investigations remained unpublished and some of the materials themselves were lost.

During 1926–1928 the first systematic excavations were undertaken under the direction of G. K. Nioradze in the cave of Devis Khvreli near Shorapani railroad station.

Zamiatinin has made a detailed study of the Caucasian Paleolithic cultures, as well as an examination of the Krukowsky collection in the Museum of Georgia, Tiflis, and the Schmidt collection in Leningrad. He disagrees with the Upper Aurignacian dating given by Nioradze for the Devis Khvreli site and by Schmidt for the Virchow cave.

According to his observations, the Upper Paleolithic caves of Imerethia may be divided into three consecutive chronological groups.

The earliest division is represented by the few objects from the preliminary soundings of Krukowsky (1918) in the two caves in the vicinity of Chiaturi, Khergulis Klde, near the village of Vachevi, and of Taro Klde near Shukruti. These sites yielded characteristic Upper Paleolithic implements as well as a large percentage of late Mousterian forms prepared from broad triangular flakes, sharp-pointed instruments, and scrapers distinguished for their perfection of retouch (fig. 1, a, b).

The second group, which is most fully represented, is characterized by the cave of Devis Khvreli and the Virchow cave near the Motsamet monastery in the vicinity of Kutais, the latter explored in 1914 by Schmidt. The types of flint implements from these sites have been published by Nioradze. One must observe the profusion of the nuclear type of imple-
ment, the massive scrapers, the polyhedral burins, etc., giving to the inventory an archaic appearance. There are also numerous smaller imple-

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**FIG. 1.** a, Stone implements from Khergulis Klde near Vachevi (Chiaturi region). ½ natural size (after Zamiatnin). b, Stone and bone implements from Taro Klde near Shukruti (Chiaturi region). ½ natural size (after Zamiatnin).
ments, occasionally of geometric form, which closely connect this group with the subsequent, youngest group (fig. 2, a).

The third division is represented only by the cave of Gvardzhilas Klde'
FIG. 3. Archaeological sites in Trans-Caucasia.
near the village of Rgani. This site, which yielded many specimens, was completely excavated by S. A. Krukowsky during 1916–1917. Here predominated implements prepared from narrow and thin, elongated laminae as well as a number of smaller implements and a few geometric microliths. The numerous bone implements (fig. 2, b) have characteristics of later periods.

Regarding the flint implements from these Upper Paleolithic sites in Georgia, the main characteristic lies in their extreme similarity as well as in the sequence of the transition from one chronological group into the other. The distinctiveness which typifies the chronological subdivisions of western European Paleolithic periods is lacking in Georgia.

This peculiarity of the Georgian paleolithic caves connects them with contemporary discoveries in the Mediterranean Basin (e.g., Capsian stations in North Africa, the early Natufian sites of Syria and Palestine, and the “Grimaldi-facies” of the Upper Paleolithic sites in the Apennine Peninsula).

There is, therefore, little possibility of an homology between the Imerethian caves and the French sites solely on the basis of the similarity of disassociated types of implements. It is possible to predict that no typologically Solutrean sites will be found in Trans-Caucasia. The finds up to now give a sufficiently complete picture of the Upper Paleolithic of Georgia, without suggesting the possibility of any essential lacunæ.⁸

The three groups described above can be chronologically juxtaposed as follows: Group I, the Aurignacian period; Group II (apparently the longest), the Solutrean and Early Magdalenian; Group III, the Late Magdalenian and the Early Azilian.

Zamiatinin conducted preliminary explorations of Paleolithic sites in Trans-Caucasia from September to November in 1934.

Explorations in the vicinity of Kutais and Chiaturi were sponsored and financed by IAE. The Black Sea coastal region was investigated by the Abkhazian archaeological expedition, jointly sponsored by the Trans-Caucasian Branch of the Academy of Sciences and the Central Executive Committee of Abkhazia; the expedition was led by I. I. Meshchaninov, and included an archaeologist, a geologist, and the assistant director of the Kutais Museum.

The work in Imerethia was initiated by an examination of the sites previously explored by R. R. Schmidt and S. A. Krukowsky. Near Kutais,

⁸ Zamiatinin believes that these observations are true of the Upper Paleolithic of the Crimea; the “Aurignacian” and “Azilian” sites of the Crimean Peninsula do not represent as wide a chronological rift as it may seem.
Zamiatnin examined the Uvarov and Virchow caves close to the Motsamet monastery on the left bank of the Tskhali-Tsiteli.

In the vicinity of Chiaturi, Zamiatnin examined numerous caves containing Upper Paleolithic cultures near Rgani, Vachevi, and Shukruti. 9

The Upper Cretaceous limestone bluffs, forming the gorges of the Kvirila River and its tributaries, contain many caves and rock-shelters having ancient deposits, and indicate the extensive population of this region during the second half of the Quaternary period.

The most interesting finds were in the rock-shelters of Mgvimevi. About one kilometer to the north of Chiaturi stands the perpendicular rock on which the fifteenth century Mgvimevi monastery was situated. One of the churches of the monastery is built over the mouth of a vast, natural cave. Above the talus are located numerous rock-shelters and grottoes. In a few cases no deposits were found on their steep, stony floors; in others the use of the shelters as cattle-sheds is destroying any possible material. In four of the seven caves and rock-shelters Paleolithic remains were found.

The greatest number of finds were made in the two large rock-shelters. Although there was some destruction of the objects and the stony floor of the shelter was denuded to a considerable extent, in places thin layers of ancient deposits were partly disrupted by the cattle, exposing a considerable number of flint flakes, knife-shaped, elongated laminae and implements. The Mgvimevi finds are typologically similar to those from Devis Khvreli.

Interesting signs have been incised on the face of the rock along the edge of rock-shelter No. 5. These marks were undoubtedly made by Paleolithic man. The great antiquity of these engravings is proved by the fact that some of them are covered with a stalagmitic crust which, lower on the slope, is much thicker and encloses Paleolithic implements and fragments of extinct fauna. The patterns consist of intersecting lines of varying depth carved on the surface of the rock, forming designs of a linear-geometric character.

It is curious to note that similar engravings, also Upper Paleolithic, have been found in the Romanelli grotto, 10 in the south of the Apennine Peninsula, and also near Tebessa, Province of Constantine, Algeria. 11

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9 Rgani, seven kilometers northwest of Chiaturi; Vachevi and Shukruti, nine kilometers west-northwest and two kilometers south-southeast of Chiaturi, respectively. See map, fig. 3; fig. 1, a, b; fig. 2, b.

10 E. Regalia and P. E. Stasi, Grotta Romanelli (Castro, Terra d’Otranto): Seconda nota (Archivio per l’Antrop. e la Etnol., 1905, pp. 113-72, table 1).

This supports the observation on the similarity between Paleolithic finds in Georgia and in the Mediterranean previously made by Zamiatnin, based on his typological analysis of the flint implements.

The Paleolithic engravings at Mgvimevi are the first to be found in the U.S.S.R.

At the end of grotto No. 7 Zamiatnin came upon three massive flint implements, two burins, and one scraper. No other remains were discovered in either the grotto or the corridor. This recalls isolated implements uncovered in French caves containing Paleolithic engravings associated with no other cultural remains.

Other Upper Paleolithic remains have been brought to light in a grotto near Darkveti, six kilometers northeast of Chiaturi. Traces of a ruined encampment, somewhat later than the remains, probably contemporaneous with the Gvardzhilas Klde cave, were found in the gorge of a small stream flowing into the Kvirila River in a cave near Tsirkhvali. Another site was discovered on the outskirts of the town of Chiaturi.

Zamiatnin also searched for Paleolithic sites along the Black Sea coast of the Caucasus. He found Lower Paleolithic implements in the ancient alluvium of the Eastern Gumnista River in the vicinity of Yashtukh Mountain near Sukhum. The site is four kilometers north of the town on the road to Mikhailovka at its entrance to the gorge between the Byrts and the Yashtukh Mountains.

The first discoveries came to light in the soil excavated from a ditch along the road cutting across the ancient deposits; afterward their place of origin was located in a gravel bed overlain by a stratum of diluvial, argillaceous soil changing into the top soil. The gravel bed containing the stone implements had become, under the influence of the ferro-manganese combinations, a solid mass of conglomerate.

During the excavations many worked flints were also uncovered in the argillaceous layer and also on the surface.

The finds began at the entrance of the gorge, and continued south for approximately a kilometer and a half, always embedded under identical circumstances, never presenting the characteristics of a cultural stratum, and occurring only sporadically.

According to the geological observations of his collaborator, A. S. Soloviev, the finds are associated with the third terrace of the Eastern Gumnista River, connected in its turn with the third terrace of the sea. The encampment must have been inhabited during the Riss-Würm interglacial period and it was denuded at the beginning of the Würm glaciation.

The artifacts found near Yashtukh Mountain appear extremely archaic.
The majority are very massive, wide and short flakes of an irregularly triangular shape, with a very large striking platform occupying a considerable portion of the lower, flat side of the implement. The striking platform rarely displays preliminary flaking. In general the flints were utilized while still in this stage, without any further retouching, and have only marginal fractures, which may have been caused by usage. A number of discoidal nuclei were also collected.

Among the objects of finished form were sharp-pointed implements and massive scrapers, several cleavers and discoidal implements worked on both sides (bifaces), presenting a well-flaked nucleus.

Another characteristic of the flint-flaking technique of Yashtukh must be mentioned: comparatively few artifacts are worked by means of the Mousterian retouch. Sometimes crude flaking characterizes the secondary working which is typical of the biface implements of the Lower Paleolithic period.

This method is absolutely unknown in the Mousterian encampment near Ilskaia on the Kuban River, explored by Zamiatnin in 1925–1928. At Ilskaia even the biface implements were shaped by means of retouching.

The flint implements most nearly analogous to those from Yashtukh are from Lower Paleolithic sites containing Levallois and Clactonian types in northern France, Belgium, and England, recently described in detail by Breuil.

Thus, at the Lower Paleolithic site near Mount Yashtukh occur the oldest Paleolithic implements yet discovered in the U.S.S.R.

That here we do not have a purely local facies of archaic appearance, explicable by the character of the raw materials or some similar consideration, is seen from another curious find near Ochemchiri, eighty kilometers farther south. Here were discovered typical Mousterian implements, more similar to the Ilskaia finds than those of Yashtukh, but located like the latter except that they were found, not in the gravel bed, but in the lower argillaceous level.

Implements similar to those from Yashtukh were also discovered at Kiurdere near Psyrtskhi (formerly Novyi Afon) on the Black Sea. The stratigraphy was similar to that of the Yashtukh site.

During 1935 Zamiatnin and five assistants continued these researches. They were joined later by a geological expedition under V. I. Gromov.

In 1925 the skeleton of a Neolithic man was found at a depth of eight meters near the ancient fortress of Tsitsernakaberd in the Khorunbulak Gorge of the Zanga River near Erivan.\(^\text{12}\) This tall, dolichocephalic individual

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\(^{12}\) B. N. Vishnevski, Palaeoantropologicheskaia nakhodka bliz Erivani[A Paleanthropological Find from Erivan] (Sovetskaia Etnografia, 1934, No. 5, pp. 40–47).
was associated with fifteen small flint and obsidian implements and four crude pots. The stratigraphic details of the find were not observed during the discovery, but could not have been of the Quaternary period, as originally claimed. Nansen compared the skeleton with those from Brünn and Combe-Capelle. Schultz believed that this individual belonged to the Mediterranean race. According to Vishnevski these conclusions were premature, both on the basis of the anthropometric investigations and subsequent craniological researches in Armenia, which have shown the predominance of the complexes of the northern race in the Bronze and Iron Age sites of Armenia.

B. The Copper and Bronze Ages. Investigations regarding the early metallurgy of the Caucasus region were conducted during 1933–1934 by GAIMK. Analyses of copper and bronze specimens were made by the Institute for Historical Technology of the Academy.

As a result, the regions of ancient copper mining and the centers of metallurgy in the Bronze Age and the spheres of their influence have been established. It is probable that there existed a local source of tin supply for the bronze industry.

GEORGIA

1. *Abkhazia, northwest of Georgia*. During 1934 an expedition from IAE, with Meshchaninov as leader, investigated the eastern shores of the Black Sea. Near Eshiri several dolmens were found, each of which contained about fifty skulls and objects belonging to the second millennium B.C. In the same village tombs of the late Bronze Age were excavated. These burials contained bronzes of the well-known Koban type including axes, lance,


16 The Museum of Georgia, located at 13 Rustaveli Prospect, Tiflis, contains departments of Anthropology, Botany, Geology, and Zoology. The archaeological section is under G. K. Nioradze, Paleolithic specialist. The Aurignacian and Magdalenian objects from Devis Khvreti and the Magdalenian specimens from Gvardzhilas Klde are on exhibition. There are also Bronze Age objects under the care of B. Kutin. Peter Usakov specializes in Hittite research. Professor Ziefeld is in charge of Near Eastern archaeology. The ethnographical section contains two large rooms, one devoted to Svanetia, the other to Khevsureti.

Expeditions are financed by special State funds appropriated for excavations, and when large building operations are in progress an archaeologist is assigned to see that no historical monuments or objects are destroyed.
heads, and other objects belonging to the first millennium of our era. The expedition continued these excavations during the latter part of 1935.

2. Leningori (Akhalgori), north central Georgia. A monograph by the late J. I. Smirnov describing the tomb of Akhalgori has been published by the Museum of Georgia in Russian and German editions. This tomb was discovered in 1908 in the valley of the Ksan, a tributary of the Kura (Cyrus). The numerous golden ornaments and silver vessels belong to the Achae-
menian period.

3. Sasirethi, Province of Gori, eighty kilometers northwest of Tiflis. Bronze Age objects, some for ritual use, were found here in 1930.

4. Rion River, western Georgia. The expedition of the Bureau for the Protection of the Monuments of Georgia (A. Amironashvili, leader) continued the excavation of a dwelling mound of Reka near Poti (the ancient Phasis) on the lower course of the Rion River. This is the first early site excavated under scientific direction within the territory of Colchis. The exact age has not yet been determined but it appears to be earlier than the late Bronze Age. These excavations were continued during the latter part of 1935.

5. Kakhetia Region, northeast Georgia. (a) At Ikalto the joint expedition of the Museum of Georgia and Tiflis University during 1934 excavated a cemetery of the late Bronze Age, which is approximately at the beginning of the first millennium B.C. (b) Anaga, on the Alazan Canal, province of Kakhetia, yielded non-paved tombs containing Bronze Age sabers, probably belonging to the Koban Samtrevo culture (thirteenth century B.C.).

6. Gostibe, Province of Kharthli (right bank of Kura, twenty kilometers from the Kaspi railroad station, central eastern Georgia). A crypt, disclosed by an inundation, contained objects attributed to the Bronze Age.

7. Zemoavchali, north Georgia. A Bronze Age burial, probably of the thirteenth century B.C., containing bronze weapons and ornaments, and pottery, had been excavated by Niordadze in Zemoavchali near Mtskheta in 1927. Skeletal material from the burial showed signs of artificial cranial deformation.

8. Upper Terek River. During 1934 the Terek Expedition of the GAIMK

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17 J. I. Smirnov, Der Schatz von Achalguri (Tiflis, 1934).
20 Makalathia, op. cit., pp. 102-103.
(A. P. Kruglov, leader) explored the upper course of the Terek River to Balta on the Georgian Military Highway. The excavations were conducted in the ancient burial grounds near the villages of Chmi and Lars. Burials of the second half of the first millennium B.C., containing various bronze objects and bone and tooth pendants, were discovered near Chmi. The bone used was that of domestic animals.

"Tamara's Castle" in the gorge of Darial and a number of medieval watchtowers on the shores of the Terek, forming a single defensive system, have been explored. There were also several types of fortifications, none of them later than the seventeenth century.

A number of sanctuaries venerated until recent times and containing various ritual offerings were investigated, and several stone vaults containing tribal and family burials.

ARMENIA

1. In 1934 near Leninakan (formerly Alexandropol) a site discovered during building construction was studied by the Institute of the History and Culture of Armenia in Erivan. Houses and tombs were found as well as a mold for casting bronze axes of the type associated with the Trans-Caucasian Late Bronze Age.

2. At Bash-Garni, southeast of Erivan, Buniatian continued the restoration of a Roman temple, erected in the year A.D. 52.

AZERBAIDZHAN

Tombs of the Early Iron Age containing numerous bronze objects including weapons, ornaments, horse harness, etc., were excavated by the Trans-Caucasian division of the Academy of Sciences of the U.S.S.R. during 1934 near Delanlar in the district of Dzhebrail.

Under the auspices of the Azerbaidzhan Academy of Sciences N. I. Anserov is making an anthropometric survey of the peoples of Leninakan and other districts in Azerbaidzhan. Alisker Alekperov is preparing a report on tribal marks of the Caucasus, which are said to resemble Magyar religious symbols. S. A. Kovalevski specializes on the history of the Scythians, Cimmerians, and the Geloni.

The archaeological section of the Azerbaidzhan State Museum (Azgos Musei, 6 ul. Malygin, Baku) includes pottery, bronze, and iron objects from Yaloila-Tapa in the Zakatal district and painted pottery, iron, and bronze from stone tombs and sepulchers in various districts. Black vessels and bronze objects from the tumuli at Khadzhlakh (Agdam) and at

23 For a Neolithic reference, see p. 269.
Fig. 4. Archaeological sites in the Ukraine.
Chovdar and Khach-Bulog (Gandzhar) are on exhibition. The collections of Baiern excavated in 1879 near Delizhan and objects from the Gandzha district are shown. A sixteenth century Persian coin collection as well as gold coins found in the Lenkoran district and attributed to the Byzantine period are among important exhibits. The ethnographic section shows racial types as well as the life of the nomadic and semi-nomadic cattle breeders in exhibits of settlements, covered wagons (kibitkas), utensils, and clothing.

The section devoted to Oriental Art includes Daghestan and Azerbaizhan pottery as well as a fine collection of Persian and Rayy pottery of the eighth to the fourteenth centuries. Rugs from the Kuba and Kazakh districts are on exhibition. There are also examples of metal ware, miniatures, and calligraphy.

An archaeological expedition is excavating near Julfa but no reports were available.

There are about two hundred Ossetian skulls in the Medical Museum of the Azerbaizhan Academy of Sciences. Some of these skulls from the village of Khudat near Kuba show extreme artificial cranial deformation.

UKRAINE

It was not until the last decade that members of the VUAN\(^{22}\) in Kiev concentrated part of their efforts on the problems of prehistory in Ukrainia. Guided by the older archaeologists the younger generation discovered and excavated twenty-five new Paleolithic stations (map, fig. 4). In order to work out the stratigraphy of the various sites geologists were invited to cooperate with the archaeologists. Under the direction of F. A. Kozubovsky the following list and description of Paleolithic stations has been prepared by VUAN:

**Podolia District**

1. Sokil (Sokol), on the river Dniester
2. Vrublyntsi (Vrublintsy), on the river Dniester
3. Kitaigorod, I and II, on the river Ternava
4. Demshyn (Demshin), on the river Dniester

\(^{22}\) The All-Ukrainian Academy of Sciences (VUAN), 14 Blvd. Shevchenko, Kiev, is conducting archaeological researches throughout the Ukraine. Members of the staff include the following: F. A. Kozubovsky, director; K. U. Korshak, Paleolithic specialist; S. S. Magura, Neolithic specialist; H. V. Morgilevsky, specialist on the history of architecture. The famous collections from Tripolie are being studied in the Academy, where there is a special laboratory for the examination by chemical and physical methods of pottery and metals by means of microphotography, resistance to pressure, temperature of firing, etc. Glass, frescoes, wood, and flint are also subjected to analysis.
5. Kolachkivtsi (Kolachkovtsy), I and II, on the river Studenytsia (Studenitsa)
6. Near Studenytsia (Studenitsa) village
7. Bakota, on the river Dniester
8. Stara Ushytsia (Staraya Ushitsa), on the river Ushytsia (Ushitsa)
9. Kuzheleva, on the river Ushytsia (Ushitsa)
10. Kalus, on the river Dniester
11. Semenky (Semenki), on the river Bug

Volkynia District
12. Horodok (Gorodok), on the river Horyń (Goryn)
13. Dovhynychi (Dolginichi), near Ovruch on the river Horyń (Goryn)
14. Korosteń, near Iskorost on the river Uzh
15. Near Kolodyazhne (Kolodiazhnoe), on the river Sluch

Kiev District
16. Kiev I, St. Cyril's Street
17. Kiev II, Protasiv (Protasov) Yar
18. Near Selyshche (Selishche) village on the river Dnieper, near Kaniv (Kanev)

Chernigov District
19. Mizyfi, on the river Desna
20. Pushkari, on the river Desna
21. Novhorod Siverskyi (Novgorod-Seversk), on the river Desna

Poltava District
22. Hontsi (Gontsy), on the river Udai
23. Zhuravka, on the river Udai

Kharkov District
24. Shchuriv Rih (Shchurov Rog), on the river Donets

Donbas District
25. Near Luhansk (Lugansk)

Dnepropetrovsk District (formerly Ekaterinoslav)
26. Kryvyi Rih (Krivoi Rog), on the river Ingulets
27. Kodak village

Dneproges District
28. Dubova Balka (1931)
29. Kaistrova Balka (four stations in 1931)
30. Osokorivka (Osokorovka) (1931)
31. Miorka (1932)
32. Yamburg (1934)

The general opinion concerning the antiquity of man in the Ukraine is that the majority of the Paleolithic stations were occupied in Aurignacian
times. Comparative studies of recent publications dealing with material from eastern Europe appear to confirm this statement.

_Podolia District_. During 1927–28 excavations revealed a number of Aurignacian sites with implements and fragments of bones of small animals belonging to the final phase of the Quaternary period in eastern Europe. These important archaeological stations are located near the villages of Bakota, Studenytsia (Studenitsa), Vrublyntsi (Vrublintsy), Staraya Ushitsa (Staray Ushitsa), Ozaryntsi (Ozarintsy), Kalus, Kolachkivtsi (Kolachkivtsy), and Kuzheleva in the Kamenets-Podolsk district.

_Volhynia District_. The most typical Paleolithic station found in the Volhynia district is that of Dovhynychi (Dolginichi), located on a moraine deposit. A quantity of bone material indicate a mixed swamp and steppe fauna followed by _Elephas primigenius_, _Rhinoceros tichorhinus_, _Rangifer tarandus_, _Canis lupus_, _Equus caballus_, _Sus scrofa ferus_, and _Citellus sylvaticus_. Stratigraphical and paleontological data combined with a technological study of the industry ascribe this station to the Upper Aurignacian period.

S. Hamtchenko and I. Levytskyi discovered a Paleolithic station near the village of Kolodyazhne (Kolodiazhnoe) on the river Sluch. They excavated through nine meters of deposit which yielded worked bone objects attributed to the Upper Paleolithic period.

_Kiev District_. During 1934 the Ukrainian Expedition of GAIMK (S. S. Magura and T. S. Passek, leaders) excavated jointly with the Institute for the History of Material Culture of VUAN. In the region of Kiev they continued to study the Tripolie culture attributed to the beginning of the Bronze Age. Ten sites have been explored. Two dwelling complexes ("Tripolie squares") have been excavated. In them were discovered remains of three fireplaces, many pieces of clay plastering, permitting the reconstruction of the form of the living quarters, several large vessels for storing and preparing food, several hand mills, fragments of feminine clay figurines, bones of domestic animals, and shells of edible bivalves.

A few dune stations have been explored. Cultural remains of a station later than the Tripolie culture have been excavated near Ukrainka.

_Cherningiv District_. In 1932 M. Rudynskyi discovered Paleolithic stations near the village of Pushkari and in the city of Novhorod Siverskyi (Novgorod Seversk) on the Desna River. A deposit rich in flint tools and animal remains was unearthed about eighteen to twenty centimeters below the humus. In his report to VUAN Rudynskyi states: "the site near Pushkari shows a clearly defined pre-Solutrian layer of Aurignacian industry which may be ranked with the contemporary classical sites of central and western Europe."
Poltava District. In the vicinity of Zhuravka a Paleolithic station has been discovered on the lower terrace, sometimes assigned to Würm II, on the left bank of the Udal River. This station yielded few animal remains with the exception of Marmota bobax. Some bones of Elephas primigenius were found about three hundred meters from the archaeological site, where type implements included backed blades (lames à dos rabattu), as well as Châtelperonn and Gravette points.

Donbas District. Data dealing with Paleolithic discoveries near the city of Luhansk (Lugansk) in the Donbas (Donets) region are as yet incomplete, although a preliminary report has been made by S. Loktiushov. He found also some Upper Paleolithic stations, which he, Gorodtsov, and Sawicki attribute to the Aurignacian period. The fauna include Rhinoceros tichorhinus, Rangifer tarandus, Equus caballus, and Bison priscus. The most important object was a spear point made of mammoth bone.

In 1924 P. Esimenko found an Upper Mousterian station at the mouth of the Derkul River, which flows into the river Donets.

Dnepropetrovsk District. In 1934 an expedition sent out by IAE from the Dnepropetrovsk (formerly Ekaterinoslav) Museum found near Kodak village at a depth of twenty-five meters remains of Elephas trogontherii and other fauna, together with some stone implements. This stratum apparently lay beneath the moraine of the great Dnieper ice sheet of the Mindel-Riss period.

Dneproges District. In the Dneproges district special research has been conducted in the general area of the Dnieper rapids. Eight Paleolithic and about eighty Neolithic and Megalithic stations have been discovered. T. T. Tesla found two Paleolithic sites near Dnepropetrovsk. J. Levytskyi brought to light a new group of Paleolithic stations near Yamburg.

On the left bank of the Dnieper river the Paleolithic stations reveal cultural strata separated by sterile bands. At Dubova Balka, where there are eight cultural levels, the first stratum containing artifacts is at a depth of 4.5 meters and virgin soil is reached at a total depth of 7.5 meters. According to Riznychenko the cultural remains discovered in the lower strata, consisting of a sandy loess deposit, may be attributed to the close of the Würm I period and the first half of Würm II. The animal remains include Bison priscus, Equus sp., Lupus lupus and Lepus sp.

The archaeological sites on the left bank of the Dnieper River suggest that the prehistoric hunters who lived there in Upper Aurignacian times

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24 No other information at present available.
25 He is reported to be preparing a detailed publication on a Paleolithic skeleton from Vovnikh near Dnepropetrovsk.
belonged to a single primitive society and returned to these sites periodically. Excavations revealed flint flakes, scrapers of various types, Gravette points, and bone tools, including needles and ornaments made from sawn, grooved, tubulated bones, a pendant consisting of a perforated animal tooth and some drilled shells of Cardium, Buccinum, and Netrum fluviatilis.

From a detailed study of the typology, fauna, and geology Levyttskyi reports that at the archaeological site near the village of Osokorivka II the cultural strata can be divided into the following three groups:

1. Lower strata, Nos. 1–3, are Aurignacian.
2. Stratum No. 4 is Magdalenian.
3. Stratum No. 5 is Epipaleolithic.

NORTH CAUCASUS

1. During 1934 and 1935 IAE sent an expedition to Elizavetovskaia, Kuban district. This expedition (V. A. Gorodsov, leader) excavated an extensive necropolis of the second and third centuries B.C.

2. The Manych expedition, from GAIMK (V. V. Holmsten, leader) worked during 1934 near the Manych canal dam, near Khutor Veselogo, Mechet district, Azov-Black Sea region. Many ancient settlements, with adjoining tumuli, were explored. The majority belonged to the Bronze Age but in some the strata were overlain by medieval deposits.

The remains of a dwelling were discovered during the exploration of an early Scythian settlement.

3. The Baksan expedition from GAIMK (A. A. Jessen, leader) during 1934 explored the middle course of the Baksan River, Kabarda-Balkar autonomous region. A number of Bronze Age tumuli were explored and two excavated. One burial was in a box formed of limestone slabs and another a simple interment. Stone implements and pottery dated the burials in the third or at the beginning of the second millennium B.C. Other sites of the same period were also explored, with a few settlements pertaining to the first millennium A.D., belonging probably to the Alans and forming a single system of fortifications. A settlement assigned to the second half of the first millennium yielded pottery, mill stones, clay platters and pans for baking bread, and other cultural remains.

Ancient Kabardinian burials, not later than the fourteenth century, were excavated: a hoard of iron proves the existence of ancient forges.

CRIMEA

The Institute of Anthropology and Ethnology of the Academy of Sciences of Leningrad (IAE) sent the following two expeditions to this region:
1. At Yalta during 1934 and 1935 O. N. Bader with three assistants excavated a Microlithic station which yielded a large collection of Tardenoisian specimens.

2. During 1934 and 1935 near the village of Shankova, S. N. Bibikok with three assistants excavated some caves containing implements of Azilian type.

3. During 1934 the Evpatoria expedition, from GAIMK (P. N. Schultz, leader), explored the western part of the Crimea. Tumuli and a pre-Scythian settlement were investigated. The Scythian settlement near Kara-Tobe was excavated. Several groups of dwellings built of chopped straw and stone were discovered, the most interesting being a yurt-like dwelling of the first century A.D. To this was attached a grain pit and a stone room in the form of a niche. This was a nomad's dwelling adapted to the conditions of a settled life. In the settlement were found many pieces of local and imported Greek and Roman pottery. Tumuli near Chokrak contained two stone effigies of Polovtsi warriors.

4. During 1934 the Crimean Expedition from GAIMK (V. I. Ravdonikas, leader) made a special study of the ancient water supplies of the Crimea. This expedition was conducted in cooperation with the State Historical Museum and the water supply organizations of the government.

A catchment basin in the creek of Bilderan near Cherkes-Kermen, Bakhchisarai, was studied. In the region of Feodosia crushed rock embankments were explored, originally considered to be artificial condensers built by the Genoese. The expedition also explored ancient water supplies of the Feodosia region and collected a number of ancient mains, which were taken to the Museum.

A catacomb of the Goths' burial ground in Eski-Kermen, the exploration of which had been systematically conducted since 1928, and the cave complex of Chelter were investigated. The excavations have yielded considerable archaeological and anthropological materials.

5. During 1934 the Kerch expedition from GAIMK (L. M. Slavin, leader) made important archaeological investigations of the ancient classical colonies of Bosporus. Two sites threatened by building activities were explored, the settlement and necropolis at Dia and the settlement at Myrmikia. Defensive walls and towers of the Hellenistic period were found by the excavations in Dia, together with the remains of early Roman fisheries, containing cisterns for the salting of fish, a large platform strewn with the remains of fish, and also several wells. In another section of Dia

26 According to Pravda (October, 1935) implements of Azilian type were reported from near this village. A Neanderthal site was also found at Bychki on the river Kacha.
were a male and female effigy, each on a stone slab, of the Cimmerian-Tauric period, preceding the Greek colonization of this region, i.e. up to the sixth century before the Christian era. The stylized effigies (babas) were covered by layers of the classical Hellenistic and Roman periods. Several fish-salting cisterns were discovered in the Hellenistic stratum together with fishing implements, remains of private dwellings, and a street pavement. Other cisterns and dwellings were found in the Roman stratum.

Eighteen burials belonging to the classical period, containing clay, metal, and glass objects were excavated in the necropolis of Dia. A street gutter of the fourth century B.C. was exposed in the settlement of Myrmikia, as well as a large winery of the Hellenistic and Roman periods. The winery consisted of a large, cemented, level platform, five by four meters, used for pressing grapes, two stone presses, and two cemented cisterns for the storage of wine. Remains of Hellenistic and Roman streets were unearthed nearby. Imported Greek ceramics of the sixth century B.C. were discovered which enabled a correction to be made in the original dating (fourth century) of the founding of Myrmikia.

Other explorations were conducted in the city of Kerch, yielding important materials for the topographical study of Panticapea.

**EUROPEAN RUSSIA**

The results obtained by seventeen expeditions from GAIMG are described by P. I. Boriskovsky.27

1. The White Sea-Baltic Sea Expedition (B. F. Zemliakov, leader) explored the shores of Lake Onega. A Neolithic site on Mt. Medvezhia of the early part of the second millennium B.C. was excavated. Remains of ancient fires, implements of polished schist and flint, and fragments of ornamented vessels were found.

Another Neolithic site near the village of Voi-Navolok yielded polished schist axes, chisels, flint arrowheads and scrapers, drilled weights, and many pottery fragments. A second site in the same neighborhood belonging to the end of the Neolithic period yielded flat-bottomed vessels. All three stations belong to an ancient colony of fishermen.

Five tumuli excavated near the cemetery of Chelmug contained Finnish burials of the tenth to eleventh century.

2. The Svir Expedition (V. I. Ravdonikas, leader) worked near the Svir hydro-electric plant ("Svirstroi II") along the Svir River and the

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eastern shore of Lake Onega. Three Neolithic sites were discovered. Finds included stone implements and pottery fragments.

A fishing and hunting settlement of the third millennium B.C. on the Svir, near the village of Voznesenie, was excavated. Nearly a thousand stone implements and more than six thousand fragments of pottery were found. Stone implements included polished pickaxes, axes, wedges, chisels, flint arrow and spear heads, knives, gravers, etc. Many unfinished tools demonstrate every stage from crude flaking to polished implement. Some of the fragmentary vessels measured fifty centimeters in diameter at the neck, and were completely covered with comb and pit-ornament.

Petroglyphs on the eastern shore of Lake Onega were studied. These represent the earliest examples of monumental plastic art in eastern Europe.

3. The Novgorod Expedition (M. K. Karger, leader) excavated the so-called Rurik’s gorodische three kilometers from Novgorod. The upper stratum, pertaining to the twelfth to fourteenth centuries, contained many fragments of wheel-made pottery, objects of iron, implements for fishing, molds, and other objects. Below this, the stratum attributed to the ninth to tenth centuries yielded pottery, manufactured without the help of the potter’s wheel, and crude iron implements. The lowest level contained ornamented pottery and bone implements, as well as bones of fishes and animals, attributed to an Upper Neolithic settlement of fishermen-hunters.

Monastery ruins of the twelfth to fourteenth centuries near Novgorod were also explored. Excavations in the southern transept of the Church of the Savior of Kovalev disclosed four magnificent stone sarcophagi with fourteenth century burials.

4. The Volga Expedition (P. N. Tretiakov, leader) worked between Rybinsk and Yaroslavl, near the Sredvolgstroi hydro-electric station. Rectangular dwellings with central hearths were discovered in the three settlements of the first millennium A.D. on the Volga. Remains of copper smelting were found, including crucibles, molds, ingots, slag, sickles, fishhooks, arrowheads, fishing weights, etc. Bones of domestic animals bear witness to cattle raising. Interesting results were obtained from a site near the village of Berezniaki, where five houses were excavated, together with a burial edifice and the remains of an ancient stockade constructed with thick oaken beams.

A number of excavated tumuli contained German coins of the eleventh to twelfth centuries. The graves of women held various ornaments including beads, jewelry, etc., while those of men yielded iron knives and bone combs.
5. The Vaulovo Expedition (D. A. Krainov, leader) was organized in cooperation with the State Historical Museum. A burial ground of the Fatianovo culture, belonging to the Early Bronze Age, was excavated near Vaulovo in the Tutaev district of the Ivanov region. Seven burials, two of them each with two bodies, enclosed flexed skeletons in constructions made of logs. One grave presented a ritual burial of a goat. The grave furniture included ornamented and plain vessels, perforated stone hammer-axes, flint wedges and knives, bone awls, bronze axes, a bronze awl with a wooden handle, and other objects. Of particular interest was a crude stone axe, possibly a hoe. The great number of bronze objects sheds a new light on the Fatianovo culture.

6. The Bogoliubov (Ivanov region) excavations, conducted by N. N. Voronin, disclosed the remains of fortifications and a castle of the famous Prince Andrei Bogolubski. The plan of the castle offered the nearest Russian parallel to those of western Europe.

7. The Moscow-Volgostroi Expedition (O. N. Bader, leader) worked along the Volga-Moscow canal. A Bronze Age burial ground near Protasovo yielded Fatianovo type perforated, polished stone axes, flint scrapers and knives, wedge-shaped flint axes, a large polissoir. Bomb-shaped ornamental vessels were discovered in five burials attributed to the beginning of the second millennium before our era.

A later burial ground and several tumuli were excavated near Nikolskoe, the former of the fifteenth to seventeenth centuries, the latter of the eleventh to thirteenth centuries. Anthropometric studies of the skeletal remains may shed light on the change of physical type of the inhabitants of the neighborhood of Moscow which took place between the twelfth and eighteenth centuries.

A feudal gorodishche at Dimitrov contained strata of eleventh to fourteenth, seventeenth, and eighteenth centuries. An eleventh century blacksmith’s shop and a grain pit were among the discoveries.

Four distinct cultural layers, eleventh to eighteenth centuries, were discovered in the kremľ (citadel) of Kalinin (formerly Tver). The remains of an old tannery, blacksmith’s forge, potter’s shed, etc., were brought to light.

Many other archaeological monuments of various periods were found during the construction of the Volga-Moscow Canal.

8. The First Volga-Don Expedition explored the middle course of the Don from Migulinskaia to Serafimovich.

A number of sites of the Upper Neolithic period have been discovered yielding many microliths.
Several tumuli and other Bronze Age sites, a Roman Sarmatian settlement, a number of tumuli of the nomads of the ninth to twelfth centuries, and several monuments of the Golden Horde, thirteenth to fourteenth centuries, have been located.

9. The Second Volga-Don Expedition explored the Don from Serafimovich to Golubinskaia. Many Bronze Age sites were discovered yielding stone implements, fragments of vessels, and bones of animals. Several tumuli were also excavated.

Medieval settlements of the eighth and ninth centuries were also uncovered.

10. The Third Volga-Don Expedition (M. A. Miller, leader) explored the Don from Golubinskaia to Kamenski, from Kalach to Krasnoarmeisk, and along the Donskaia-Tsaritsa River. About one hundred tumuli were found, together with remains of settlements of various periods. Several of the tumuli contained flexed burials in catacombs and interments belonging to the Bronze Age, as well as burials of Sarmatian type, second to fourth centuries A.D., and burials provisionally classified as Avar, fifth to seventh centuries A.D. A settlement of the Golden Horde was also excavated.

11. The Sarkel Expedition (M. I. Artamonov, leader) explored the Khazar city of Sarkel on the Don near Tsimlianskaia, contracted in the year 835 by Byzantine craftsmen.

A number of brick structures, important for the study of building technique of that period, were brought to light. Many marble fragments, including part of a column fifty centimeters in length, indicate the presence of a temple or a palace built by the Byzantines. Numerous bones of domestic animals suggest that meat was the most important item of diet, while fish bones point to the practice of fishing. Conical and barrel-shaped grain pits two and a half meters deep, with the remains of carbonized grain as well as numerous hand-mills, testify to the practice of agriculture.

Some remains of handicraft, tools for the manufacture of ornaments, bone and stone articles, and pottery objects bear witness to commercial relations with Byzantium and the Moslem East.

The city burial ground yielded typical nomad burials containing horses.

12. The Oka Expedition (P. I. Boriskovski, leader) explored from Murom to Riazan. Traces of Paleolithic culture, including worked flints and bones of prehistoric animals, occurred at Troitse-Pelenits near Yasakovo, Spasski region.

The Epipaleolithic station of Elin Bor near Murom yielded flint implements of geometric forms with scrapers, laminæ showing signs of retouching, arrowheads of the so-called Sviderski type, etc.
13. The Kama Expedition (N. A. Prokoshev, leader) continued the work begun in 1932 near the Perm hydro-electric station. The Levshin dune station, belonging to the end of the third millennium B.C., was completely excavated, yielding fragments of ornamented pottery, flint implements, net weights of flat pebbles, and a copper awl, all imbedded in the hearths of this same cultural stratum. The copper awl is unique and, with the copper, laminated knife found by A. V. Schmidt, offers a basis for dating the site. The remains of a dugout type of dwelling, pottery, stone implements, including polished axes, adzes, points, arrowheads, scrapers, etc., were uncovered in the lower stratum of a dune site attributed to the middle of the second millennium B.C. on Lake Griaznoe.

A previously known site near Turbinovo was found to have two strata: the upper, a settlement of the first centuries of our era; the lower, a Bronze Age burial ground of the end of the second millennium B.C. In the latter were found flint implements, a copper (or bronze) hatchet, celts, adzes, knives, awls, and also bracelets made of white alloy and nephrite objects, the last suggesting a very early commercial intercourse with Siberia.

Galkinskoe settlement, at the mouth of the Chusovaia, belongs to the “Anan’in” culture (several centuries B.C.). Here were found pottery, stone, bronze, bone, and iron implements. Especially interesting are a hoe fashioned from moose horn and a hand mill.

14. The Ufa Expedition (P. A. Dmitriev, leader) worked near the Ufa-Ishimbai’ev railroad of the Bashkir A.S.S.R. Nineteen archaeological sites were discovered, and some of them excavated.

The Balanbash settlement, belonging to the beginning of the first millennium B.C., yielded a bronze hook used for suspending a kettle over the fire, bone spindles, a table-like object of unknown usage, ornamented pottery, and bones of domestic animals.

The Demskaja settlement is of the early “Anan’in” culture of the same period: here were found numerous flint implements, hand-mills, a copper knife, a copper lamina with rivets, pottery fragments, and bones of domestic animals.

The site near Kuganak, of a slightly later stage of the “Anan’in” culture, also contained bones and pottery.

Other tumuli and interments contributed archaeological materials of all periods of Bashkir history from the middle of the second millennium B.C. to the eighteenth century A.D.

15. The Pogar district of the Western Province (Oblast) was explored by K. M. Polikarpovich. Two Upper Paleolithic sites containing the bones of mammoth, arctic fox, and other Quaternary fauna, together with remains
of hearths, flint chips, and implements, were discovered near Yudinovo on the bank of the Sudost.  

16. The Oskol Expedition conducted explorations during construction of the Staryi Oskol-Kursk railroad. A late Bronze Age settlement was excavated near Lukianovka, including part of a large (fifteen meter wide) dugout dwelling, in which were found pottery, tools, and animal bones. Of especial interest were implements made of mandibles of large animals, deer or moose horn combs, and half of a stone mold for manufacturing ornaments. A tumulus of the same period contained flexed burials with pottery and bones of domestic animals. Several dune stations of the late Bronze Age were also explored.

17. The Kostenki Expedition (P. P. Efimenko, leader) continued excavations begun in 1923 of Upper Paleolithic sites in the villages of Kostenki and Borshevo near Voronezh.

In 1934 GAIMK cooperated with IAE and the Soviet section of the International Association for the Study of the Quaternary Period in Europe. Two ancient dugouts were disclosed in Kostenki I. Beside flint implements and bones of domestic animals, stores of minerals used possibly for dyeing were found. Fragmentary female figurines of marl, also those of animals, probably connected with totemic beliefs, came to light. A mammoth, the head of a cave lion, a bear, and a camel could be identified. This is the first discovery of a camel effigy reported from a Paleolithic site.

TURKESTAN

Historians have always shown marked interest in the archaeology of this important region which lay on one of the great trans-Asiatic routes of migration. Furthermore, the remarkable discoveries by Pumpelly at Anau have stimulated the desire for further information from this little known region. It is therefore of considerable interest to describe in outline the results of Soviet archaeologists in Turkestan. The location of the sites is shown on the map, Figure 5.

1. Uzbek S.S.R. The Ferghana Expedition from GAIMK examined the ancient irrigation system of Kyzyl Yar steppe. Excavations were conducted at the gorodishche (site) Shaari-Khaiber near Khakul Abad. Two cultural layers were explored: the lower was characterized by pottery manufactured without the use of the potter’s wheel and sometimes covered with painted decorations reminiscent of Anau I pottery; the upper layer revealed

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28 In 1935 an expedition from IAE under the leadership of K. M. Polikarpovich, accompanied by two assistants, was to examine two Upper Paleolithic sites on the Sudost River in the former government of Orel.
Fig. 5. Archaeological sites in Turkestan.
a feudal fortification of the sixth to eighth centuries A.D. In the Naryn valley two lower layers (third to fourth centuries A.D.) of a tepe were explored. A number of these tumuli were investigated in the gorge of Kamnyr Ravat, containing cultural remains of primitive society through the feudal era, and organically connected with the ancient systems of irrigation.

The irrigation of the Isfar Valley and adjoining settlements was studied. Excavations in three burial grounds brought forth urn burials in large ossuary jars, accompanied by many objects and well preserved wooden articles.

The report on the 1933 campaign appears in Vol. 110 of the Izvestiiia (Communications) of the Academy. An article dealing with the ancient irrigation of this region has also appeared.29

2. Zaravshan Valley, Tadshik S.S.R. During 1933 in the ruins of a castle on Mount Mugh near Khairabad in the upper Zaravshan Valley a large number of Sogdian manuscripts of the eighth century of our era were discovered. This report29 contains a catalog of the eighty manuscripts (69 Sogdian, 8 Chinese, 2 Arabic, and one undetermined) and the texts of the Arabic and Chinese manuscripts.

3. Angren Valley, Uzbek S.S.R. In 1934 the Tadzhik-Pamir Expedition of the IAE (M. Masson, leader) made an archaeological survey of the Angren Valley, which lies some fifty to one hundred kilometers to the south and southeast of Tashkent. This was an important mining and metallurgical region in early Islamic times (ninth and tenth centuries). These mines are now being re-exploited. The cities of Tunket and Khanka as well as about ten other tepes, ancient roads, mounds, slagheaps, etc., were discovered.31

4. Chirchik, south of Tashkent. In 1934 an expedition from the State Museum for Ethnography in Leningrad, led by G. Grigoriev, began an archaeological survey of the Chirchik valley. A large number of pre-Islamic sites were found. These tepes appear to belong to the first millennium before the Christian era, when this region was inhabited by the Sakas of the Achaemenian texts.

In 1935 the expedition began to excavate the tepe of Kaunchi on the right of the Chirchik River near the Tashkent-Samarkand railway. The tepe, which covers an area of fifty acres, is the largest in this region. Two


31 In the communication from A. A. Jessen, dated September 15, 1935.
cultural levels can be discerned. The lower stratum is mainly characterized by bone with some bronze implements, the upper by a large number of iron implements and pieces of slag. The wealth of material excavated will contribute many details regarding the cattle-breeding, agriculture, industrial arts, and the religion of the ancient population. The upper stratum is entirely pre-Islamic.

5. Khorezm (Khiva Oasis), Turkmen S.S.R. In 1934 an expedition from GAIMK (M. Voievodsky, leader) explored the southern part of Khorezm in the delta of the Amu Darya (Oxus). This area lies to the south of the Aral Sea.

Ruins of the city of Zamakhshar attributed to the eighth to fourteenth centuries were investigated. Remains of a pottery workshop, allowing a detailed study of the entire manufacturing process employed, were unearthed. The city was abandoned in the fourteenth century A.D.

6. Baghir, Turkmen S.S.R. At Baghir, west of Ashkhabad, capital of the Turkmen S.S.R. and located on the Trans-Caspian railway near the Iranian boundary, excavations were conducted by the Scientific Institute of Turkmenistan on the site of the ancient Parthian capital of Nessa, during 1934 and 1935. A large Parthian building, a house of the Sasanian period, a water conduit, pottery, statuary fragments, and other objects were discovered. In the summer of 1930 an expedition uncovered two large superimposed buildings. Anau is thirteen kilometers south of Ashkhabad.

SIBERIA

1. The Altai Expedition\(^\text{22}\) explored several interesting kurgans on the left bank of the Ursula River in the Oirat Autonomous Area. These were first reported in 1932. Some were crowned with three or four concentric circles of stone, others had on the summit square cubicles formed of slabs. The largest kurgan was forty meters in diameter and four smaller mounds in the same group were joined in a chain. The tumuli with concentric circles disclosed that the burials, 2.6 to 3.0 meters deep, had been robbed at an early date. A few of the objects found in the corridor made by the robbers were pottery similar to that of the Minusinsk Tatar culture, blue and white paste beads, etc. One stone kurgan, on an upper terrace, did not contain burials. Scattered sheep bones suggested its original ritualistic function. Another kurgan yielded bone horse trappings and the burial of a woman, containing remains of a red silk pall with bronze ornaments, toilet articles,

and jewelry closely resembling the well dated finds\textsuperscript{33} in Noin Ulin, northern Mongolia, as well as the late Tagar tumuli of the Minusinsk, Achinsk, and Krasnoyarsk regions on the Yenisei. The latter contained coins of the Wu Chu Khan dynasty, first issued 118 B.C.\textsuperscript{34}

The finds are important because of the association with the flourishing commercial development of the Northern Hun Union in the first century before our era.

2. In 1935 an expedition from IAE under Okladnikov conducted excavations, principally of Neolithic graves, near Khabarovsk on the Amur River.

These researches form part of a much larger expedition under the leadership of V. G. Bogaraz, chief of the Siberian Department of the IAE, and Ian P. Koshkin, Director of the Institute of the Peoples of the North. They are accompanied by several ethnologists who are studying the tribes of the Amur region. Work will continue over a period of several years. One of the main objectives of these expeditions is to throw light on the migrations of peoples to the American continent.

The Academy of Sciences of the U.S.S.R. has made a preliminary appropriation of 100,000 rubles for these expeditions.

3. The Angara Expedition of GAIMK (G. P. Sosnovski and B. E. Petri, leaders), in coöperation with IAE and the Soviet section of the International Association for the Study of the Quaternary Period in Europe, conducted explorations of the shores of Lake Baikal and the upper courses of the Angara and Belaia Rivers in eastern Siberia, at the site of the projected hydro-electric stations on the Angara.

The expedition continued the excavations of the well known Upper Paleolithic site of Malta near the Belaia River. Great accumulations of mammoth, Siberian rhinoceros, reindeer, and other bones of animals hunted by the inhabitants of the settlement were discovered, together with great numbers of stone and bone implements. Especially interesting is a flint, leaf-shaped point supporting the previous dating of the settlement as Sol-


utrian, a well made, perforated bone needle, fragments of stone bracelets, bone slabs ornamented with wave-like lines, fifteen bone pendants or beads, and a bird figurine. A breastwork of bone and dirt, serving apparently as the base of a primitive dwelling, was discovered in one of the sections of the settlement.

Twenty-three archaeological sites, mainly Neolithic settlements of hunters or fishers of the second or third millennium B.C., were discovered in the course of exploration of the Angara River from its sources to the city of Irkutsk.

SUMMARY

This article incorporates a brief summary of the results obtained at approximately two hundred archaeological sites by fifty-seven Soviet expeditions. Grateful acknowledgment must be made to the Directors of IAE, GAIMK, VOKS, VUAN, ZOKS, and others who have contributed information herein contained.

From this material the student of anthropology may avail himself of these recent\textsuperscript{35} archaeological data and should further detailed information be desired he may correspond either with the institute under whose auspices the research work was conducted or with the leader of the expedition.

The authors hope to publish from time to time similar reports on anthropological, archaeological, and ethnological expeditions within the boundaries of the Soviet Union.

\textbf{Field Museum of Natural History}

\textbf{Chicago}

ROLAND Burrage Dixon, son of Louis Seaver Dixon and Ellen R. Burrage, was born in Worcester, November 6, 1875, and died at his home in Harvard, Massachusetts, on December 19, 1934. Prepared at Hopkinson's School, he entered Harvard and was graduated in the class of 1897. Following his graduation he was appointed an Assistant in Anthropology at the Peabody Museum and during the summer was engaged in archaeological field work in Ohio. After receiving the Master's Degree in 1898, he became a member of the famous Jesup North Pacific expedition of the American Museum of Natural History, New York, and did ethnological work among the Indians of British Columbia and Alaska. It was in 1899 that, under the auspices of the American Museum of Natural History, he spent the first of six seasons among the Indians of California. He
soon became the acknowledged authority on the ethnology of this region. He received the Doctor's degree from Harvard in 1900 with a thesis on the language of the Maidu Indians of California.

Following the year of his Doctorate, he spent a winter in research in Germany with a trip through northern Mongolia and Siberia.

In 1901 he became an Instructor in Anthropology at Harvard University, the next year becoming a member of the Faculty. He was an Assistant Professor of Anthropology from 1906 to 1915 and since 1915 was a Professor. In 1904 he became Librarian of the Peabody Museum, in 1909 Secretary, and in 1912 Curator of Ethnology. All of these offices he held at the time of his death. By virtue of his interests in bibliography and cataloguing, he placed the library of the Museum in the first place among all anthropological libraries of the country.

Dixon's ethnological research included work in New Zealand, Tasmania, Australia and Fiji, together with a year spent in the western Himalayas, Assam and Upper Burma, the Malay Peninsula and Java, with a short trip to China and Japan. He also visited the Philippines, Mexico, and Yucatan.

In spite of these extensive travels and his work in California, Dixon was not primarily interested in field research. Filled with love for exactness and urged on by a firm belief in the necessity of the most exhaustive study of source material, he learned Russian and the Scandinavian languages in addition to the more usual French, Italian, and German. Almost literally he knew everything that had been written on the primitive peoples of Asia, Oceania, and of North and South America. He was undoubtedly one of the most erudite ethnographers of all time.

This great mass of anthropological knowledge he gave freely to his students. He was especially successful with those graduate students who worked directly under him. He inspired scholarship. In his judgments of the scientific work of his students and of his colleagues and himself, Dixon exhibited an almost inhuman objectivity. His attitude was one of unsympathetic impartiality, of ruthless condemnation, or of detached approval. These judgments because of their impersonal and principally intellectual character possessed unusual validity.

His first course in Harvard College, given in 1898–99, was on primitive religions. His most noted courses, some of which he had given for over thirty years, covered exhaustively the ethnography of North and South America, Asia, and the entire Oceanic area, including Australia.

In the realization of his ideal of a thorough study of the source material of a subject, he sought out all the physical measurements which have ever
been published on the peoples of the world. In his book, "The Racial History of Mankind," which resulted from this research, he fitted each people studied into a framework based on a statistical study of three basic anthropometrical measurements. Racial histories have been written by tracing single physical traits throughout the world but this was a pioneer attempt to use a combination of traits, a procedure which has been widely followed since this book was written. Although his special technique met extensive criticism, he was the first anthropologist to show by scientific data the composite character of the American Indians as being primarily Mongolian but with admixtures which can be affiliated with early white and negroid strains. Recent archaeological investigations have borne out this thesis.

Another of his interests was the study of the migrations of peoples through the diffusions of ideas, based on material objects. His book, "The Building of Cultures," embodies an exhaustive search of similarities in objects, however widely scattered, in order to ascertain the early movements of peoples over the world. His third major work was a collection of the mythology of Oceania. His many contributions to anthropological literature contain articles on linguistics, folk-lore, primitive art, early Polynesian voyages, and religion.

He was a member of several learned societies in the United States, Great Britain, and France.

In 1918 Dixon became a member of the House Commission, called the "Inquiry," and collected reports on the political conditions in Central Asia. In December of the next year he sailed for France with the American Commission to Negotiate Peace and remained in Paris until May.

Apart from his academic duties, Dixon was essentially an out-of-doors man. With pack train he spent several summers in the inaccessible parts of the Olympics, the Cascades and the Sierras of the Pacific, and he knew every secluded camping site in the White Mountains. In travelling and camping in wild regions with the few friends whom he admitted to this intimacy, he was the most delightful companion imaginable. He was painstakingly skilful in the technique of wilderness living. His dislike for the restrictions of urban life caused him in 1915 to build a house at Harvard, Massachusetts, where, surrounded by his own woods and his gardens, he lived a life of isolation but of contentment.

Dixon was a man of great reserve and of few intimacies. He shrank from personal contacts except upon superficial and conventional bases. Very few persons were admitted to the secret of his personality, although he often showed himself a delightful host and a charming companion. It required continuous and close association with him to break through the
surface of his polite and protective geniality. Moreover the rare fissures through his reserve closed almost instantaneously. He adhered with great tenacity to a plan of life and a scheme of scientific research which he had laid out in his youth. Undeterred by opposition, unbending and rigid in his ideas, he steadfastly maintained his aims and ideals of a scholastic life. His intimate knowledge of ethnography and the rarity and breadth of his interests made him a unique and outstanding figure in American anthropology.¹

A. M. T.

Roland Burrage Dixon occupied a unique place in American anthropology. His professional work was of a type of its own. It may be described as derived from a natural history interest, transmuted to successful adaptation to cultural material. His thirst for knowledge was insatiable, the drive to organize and interpret masses of fact grew with the years. Like every born naturalist, he was an accurate observer. But even more than for his field studies, he won repute as a painstaking, accurate scholar of wide range. Dealing with clearly-grasped problems, many of his contributions are of important bearing on points of theory; but he was not primarily a theoretician. His fundamental approach was geographic-historical; not descriptive ordinarily, except of new data acquired by himself, but in the sense of dealing with special problems—often very knotty ones—of an essentially historical bearing. Questions of the meaning of puzzling and complex distributions against a historical background he attacked with particular ardor and success. In this field his work bears a close spiritual kinship to that of Laufer, whose loss anthropology had to suffer almost simultaneously.

Dixon’s knowledge of geography was immense, and concerned with far more than culture distributions. He knew nature and natural environments—some from personal observation in several continents, all from intensive reading. He appears to have published only one strictly geographical paper, in the first year of his authorship. But his interest never waned, and it provided a soil from which his distinctive culture-historical studies drew fertile nourishment. Almost alone among their major contemporaries he and Swanton maintained a sane and constructive interest in tribal and ethnic migrations—a factor of indisputable effect on culture constellations, but unduly neglected by the great majority of us once we had succeeded in grasping the factor of diffusion and other processes operative wholly on the cultural level. Perhaps we shrank from the frankly historical aspect of

¹ This was presented on February 12, 1935 as a Minute of the Faculty of Arts and Sciences of Harvard University.
populational movements. Many of us may have dreaded even more the severe preoccupation with printed documents. Whatever the causes, we have for a generation mainly posed our problems as if this factor could be permanently neglected, except for the static distribution given by a summary language map; and it is to the credit of Dixon’s breadth and balance that he always tried to recognize this element in its full but not exaggerated strength. In this matter as in all others he remained uninfluenced by the professional swings and fashions of the day; he planned his work as he did his life and adhered to his course.

His range of production was enormous. He published in the fields of descriptive ethnography, historical ethnology, archaeology, linguistics, folk-lore; and this not as a specialist in one area, but with a master knowledge of Oceania equal to that of America, and a control of all continents. In the highly difficult sphere of the problem of Oceanic-American relations treated with scholarship at once wide and sound, Nordenskiöld was perhaps his only compeer.

The greatest body of Dixon’s field work was concerned with the Indians of California. Outstanding here is his “Northern Maidu” of 1905, the first modern and intensive ethnographic monograph on any Californian people. Only second in importance is “The Shasta,” two years later. He published about two dozen briefer papers on this area. His California linguistic contributions have been somewhat neglected and probably underestimated of recent years. Dixon was not a trained philologist, nor rigorously grounded in phonetics. His work therefore at times lacked the highest precision of acoustic and structural form. But wherever the ground has been gone over again, it appears that his materials are sound in all essentials of both content and form: he grasped and portrayed the fundamental features correctly.

The California field work ended about 1907, and some five years later Dixon’s publications on the region came to an end, except for sporadic papers. His work entered a new phase, resulting in a series of books: “Indian Population” under the Thirteenth Census, 1915; “Oceanic Mythology,” 1916; “The Racial History of Man,” 1923; “The Building of Cultures,” 1928. The first three of these are interpretations of broad surveys; the last deals more directly than any other work of Dixon’s with processes and theory. It failed of being a great book because Dixon’s natural approach was phenomenological, and he set himself a task arduous to his temperament when he decided upon an abstract frame and goal. Probably this is why his bibliography shows a lacuna of five years preceding this volume, whereas from 1902 until his death there is no other year without record of
publication. At the same time “The Building of Cultures” is marked by discriminating analysis and sound judgment throughout, and at times by discerning insight, as in the distinction made between applicability of the age-area inference formula to different traits and to comparable parts or phases of the same trait—a methodological point here apparently expressed with clarity for the first time. In the frequent passages in which the facts relevant to some complex like the outrigger, blowgun, or alphabet are analyzed, often in detail, and in the testing of particular reconstruction hypotheses against the data, Dixon is in the full swing of his stride.

“The Racial History of Man,” though it escaped much heavy attack, has generally been considered Dixon’s weakest performance, and was regretted by many of his friends. In spite of later corroboration of particular points, the findings as a whole are a failure because of inadequate method. The objective was sound enough and much needed: to establish a classification and history of human populations in wholly objective racial terms. This is also the purpose of Czekanowski and the Polish school of anthropology. It recognizes that most populations are racially mixed, and that the problem is to segregate them into definable racial units or components. This is obviously an enormous task at which a group can labor for a generation, and in which all possible traits must be given consideration, even though information on them is very irregular in quantity. Dixon however undertook the work single-handed, and set himself a time-limit. He therefore confined himself to three traits on which comparable data were most abundant the world over, and distinguished an $x$ and $y$ counterpart within each, such as brachycephalic and dolichocephalic. The eight resulting combinations he assumed as basic races, computed their proportions in the various populations, and tried to fit the results to various facts of history, geography, and race description. So simple a procedure might have had valuable suggestive results if these had been limited to tentative findings and the pointing out of new problems. But Dixon’s reliance on the objectivity of his method induced him to follow it even when it led to fantastic results evidently due to factors which he had omitted from consideration; and the use of his wide range of knowledge in attempts to prop some of his less probable findings, only made matters worse. He often referred to the work half-jokingly as his magnum opus. Less vastly conceived, more gradually and less rigidly executed, it would probably have been more fruitful and important.

Dixon’s finest and richest vein of scholarship he developed in special papers on problems involving geographic, cultural, and historical comparisons. He began to produce these during the book-writing period, but they
came more abundantly since. Aspects of American Archaeology, Methods
of Firemaking, Words for Tobacco, Archaeological Discoveries in the
Philippines, The Sweet Potato in Polynesia, Contacts across the Southern
Pacific, Tobacco Chewing, Long Voyages of the Polynesians—mostly in
the AMERICAN ANTHROPOLOGIST between 1913 and 1933, and listed in full
in the accompanying bibliography—form a unique series in which learning,
acumen, and close demonstration are combined to a high degree. With
these must be included a series of reviews in the ANTHROPOLOGIST—also
cited—into which Dixon put some of the best of his scholarship. It was in
the treatment of special topics or problems followed through a wide setting
of space and time that his native genius found its most complete expression.

A naturalist translated into a scholar in the field of culture history seems
to sum up what Dixon above all was in Anthropology. Along this line he
labored on, reserved, courteous, unsparing of himself and unswerved, with-
out immediate disciples. His particular virtues are rare and difficult to
communicate: there is obviously no one who can fill his unique place in
the ranks of the profession.

A. L. K.

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    pp. 266–76).
    Some Coyote Stories from the Maidu Indians of California (Journal of American Folk-
1902 Basketry Designs of the Indians of Northern California. The Huntington California
    Maidu Myths. The Huntington California Expedition (Bulletin, American Museum of
    [A. L. Kroeber, joint author.]
    System and Sequence in Maidu Mythology (Journal of American Folk-Lore, Vol. 16,
    pp. 32–36).
    23–27).
    The Northern Maidu. The Huntington California Expedition (Bulletin, American
    The Shasta-Achomawi. A New Linguistic Stock, with Four New Dialects (American
1906 California Folk-Lore. "Water Monsters in Northern California" (Journal of American
The Pronominal Dual in the Languages of California (Boas Anniversary Volume, New York, pp. 80–84).


1912 The Independence of the Culture of the American Indian. Address of the Vice-President and Chairman of Section H, A.A.A.S. [1911] (Science, n.s., Vol. 35, pp. 46–55).


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1923 The Racial History of Man (New York, 583 pp.).


1928 The Building of Cultures (New York. 312 pp.).

Harvard University
Cambridge, Mass.

University of California
Berkeley, Calif.
REPORT

PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION FOR THE YEAR ENDING DECEMBER, 1935

The American Anthropological Association held its thirty-fourth annual meeting at Phillips Academy, Andover, Massachusetts, on December 27–29, 1935, in conjunction with the American Folk-Lore Society and the Society for American Archaeology.

COUNCIL MEETING, DEC. 27, 4:30 P.M.

President Robert H. Lowie in the chair. The minutes of the Pittsburgh meeting, 1934, were not read, but were approved as printed in the AMERICAN ANTHROPOLOGIST, Vol. 37, No. 2: 327–38.

REPORT OF SECRETARY

The President appointed the following committees during the year:

Nominating Committee: J. R. Swanton (chairman), F. H. H. Roberts, Jr., F. G. Speck.
Program Committee: A. M. Tozzer (chairman), A. V. Kidder, L. Spier, J. M. Cooper.

By appointment of the President, Neil M. Judd and Isabel T. Kelly represented the American Anthropological Association at the Seventh American Scientific Congress held in Mexico City, September 8–17, 1935.

The membership of the Association as of December 1, 1935 is as follows:

<table>
<thead>
<tr>
<th>Category</th>
<th>Number</th>
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<tr>
<td>Life</td>
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<td>Regular</td>
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<tr>
<td>Exchanges</td>
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<tr>
<td>Deceased during 1935</td>
<td>4</td>
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<tr>
<td>Dropped</td>
<td>33</td>
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<tr>
<td>Resigned</td>
<td>36</td>
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<tr>
<td>Admitted</td>
<td>108</td>
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</table>

The Association has lost by death during the year four members: (AAA) Walter Hough, Mrs Harold Ickes; (AES) F. S. Dellenbaugh, H. F. Cleland. During the year two of our former presidents died: Walter Hough, president 1923–24, and Marshall H. Saville, president 1927–28.

The Secretary attended the meeting of the American Council of Learned Societies in Boston, Mass., January 31, 1935.

A rough tabulation of the correspondence work of the Secretary’s office for the last five years shows an average of between 350 and 400 letters sent out annually from his office.

Respectfully submitted,

John M. Cooper, Secretary

It was voted that the Secretary’s report be accepted.

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REPORT OF TREASURER

The present bank balances in the four funds of the Association stand as follows:

- Regular Fund $4,285.05
- Permanent Fund 3,379.24
- Index Fund 1,187.70
- Memoirs Fund 22.68

This makes a total of $8,874.67, of which $8,513.90 is drawing interest in four savings accounts (New Haven Savings Bank, Connecticut Savings Bank of New Haven, Second National Bank of New Haven, and First National Bank and Trust Co., New Haven). The $4,285.05 of the Regular Fund is divided between a checking account with a balance of $360.77 and a savings account of $3,924.28.

The financial condition of the Association is thoroughly sound. During the past four years the Regular Fund has increased as a result of annual surpluses by $1,291.98. For the first time in several years the fiscal year closes with no outstanding bills. It is therefore recommended that the sum of $1,291.98 be transferred either to the Permanent Fund or to the Memoir Fund according to the wishes of the Association.

The attempt has been made in this year's report to clarify certain details both by means of arrangement and by footnotes.

**Regular Fund**

**Gross Receipts**

<table>
<thead>
<tr>
<th>Balance on hand December 1, 1934</th>
<th>$4,663.53</th>
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<tr>
<td>Membership dues:</td>
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<tr>
<td>American Anthropological Association:</td>
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<tr>
<td>1932-33</td>
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<td>1934</td>
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<td>1935</td>
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<td>1936</td>
<td>289.80</td>
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<td>$3,550.64</td>
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<tr>
<td>American Ethnological Society</td>
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<td>5,944.91</td>
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<td>$10,608.44</td>
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* Unpaid memberships in the American Ethnological Society: 8 members, $40.00.
† Unpaid memberships in the Central States Branch: 3 members, $15.00.

**Gross Disbursements**

- American Anthropologist: $3,760.56
- George Banta Publishing Company
  - Printing and Illustrating: $338.34
  - Distribution: 338.34
Storage, Insurance .......................... 81.26
Reprints .................................... 387.01 $4,567.17

Reprint Series ................................ 122.55
Editor’s expenses ............................ 1,076.94
Treasurer’s expenses ......................... 491.34
Secretary’s expenses ......................... 65.39 $6,323.39

Cash on hand, November 30, 1935 ........... 4,285.05

Resources

Cash on hand, November 30, 1935 .......... $4,285.05
Due from dues:
1935: American Anthropological Association $216.00
American Ethnological Society ............ 40.00
Central States Branch ....................... 15.00 $271.00

Due from sales ................................ 44.01
Due from reimbursements (reprints, etc.).. 71.49 386.50

$4,671.55

Liabilities

Membership dues for 1936 already paid .......... $289.80
Net excess resources over liabilities* ....... 4,381.75 $4,671.55

* For the first time in several years this item contains no unpaid bills.

PERMANENT FUND

Receipts

Balance (savings account and bonds), Dec. 1, 1934 .. $3,558.00
Interest on savings ................................ 100.58
Interest on three bonds ......................... 11.75 112.33 $3,670.33

Investments

Liberty Bonds (three) .......................... $291.09
Cash in savings account, November 30, 1935 .... 3,379.24 $3,670.33

INDEX FUND

Receipts

Balance, December 1, 1934 ..................... $1,164.31
Interest on savings ........................... 23.39 $1,187.70

Investments

Cash in savings account, November 30, 1935 .... $1,187.70
MEMOIRS FUND

Receipts

Balance, December 1, 1934 ........................................  $1,087.25
Interest on savings ..................................................  $17.01
Gift of Mrs Elsie C. Parsons (royalty) .........................  4.50  21.51  $1,108.76

Disbursements

Memoir Number 42 (Kroeber et al.)*
George Banta Publishing Company ................................  $1,086.08
Cash in savings account, November 30, 1935 ..................  22.68  $1,108.76

* The remainder of the cost of Memoir Number 42 ($320.16) was paid by the Laboratory of Anthropology, Santa Fé. The publication was made possible by a gift of $1,000.00 from the National Academy of Sciences plus an accumulated interest of $86.08.

The Association also published during 1935 Memoir Number 43 (White) which cost $902.15 and was paid for by Mrs Elsie C. Parsons, Memoir Number 44 (Beaglehole) which cost $232.32 and was paid for from the Regular Fund as budgeted, and Memoir Number 45 (Aginsky) which cost $449.23 and was paid for by Dr Bernard Aginsky ($299.23) and Dr Franz Boas ($150.00). The total cost of Memoirs published by the Association in 1935 was $2,989.94 plus the cost of distribution for Memoirs 43, 44, and 45 (est. $89.67) or approximately $3,079.61.

NET EXPENDITURES AGAINST 1935 BUDGET

<table>
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<th></th>
<th>Allowed</th>
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<td>Secretary’s expenses*</td>
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<td>Editor’s expenses:</td>
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<tr>
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<td>Insurance; storage‡</td>
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<td>3,385.00</td>
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* Budgeted to include President’s expenses, if any.
† The items for Printing and Illustrations have been combined, the Editor having pointed out that the item for Illustrations was misleading since it really covered engravings only and furthermore could not be budgeted logically apart from printing. The budget as here listed includes an additional amount of $125.00 for printing as subsequently voted by the Executive Committee on April 8, 1935.
### Regular Recurrent Income and Expenditures

#### Net Income

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<thead>
<tr>
<th>Year</th>
<th>1932</th>
<th>1933</th>
<th>1934</th>
<th>1935</th>
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<tr>
<td>Memb. dues collected directly at $6 (less subscription commissions) (AAA)</td>
<td>$3,703.32</td>
<td>$3,263.16</td>
<td>$3,273.73</td>
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<td>Memb. dues from afl. societies at $5</td>
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<td>1,509.00</td>
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<td><strong>Total dues</strong></td>
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<td>4,429.66</td>
<td>4,688.98</td>
<td>5,059.64</td>
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<td>Sales of Anthropologists and Memoirs</td>
<td>570.22</td>
<td>239.56</td>
<td>329.24</td>
<td>366.54</td>
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<tr>
<td>Sales of Reprint Series</td>
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<tr>
<td>Interest (Regular Fund only)*</td>
<td>153.72</td>
<td>164.27</td>
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<tr>
<td><strong>Totals</strong></td>
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<td>$4,833.49</td>
<td>$5,163.03</td>
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</table>

#### Net Expenditures†

<table>
<thead>
<tr>
<th>Year</th>
<th>1932</th>
<th>1933</th>
<th>1934</th>
<th>1935</th>
</tr>
</thead>
<tbody>
<tr>
<td>American Anthropologist, printing and illustrations: No. 4 of preceding year</td>
<td>$2,522.40</td>
<td>1,836.75</td>
<td></td>
<td>2,613.89</td>
</tr>
<tr>
<td>Nos. 1–4 of year</td>
<td>$3,462.33</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Totals</strong></td>
<td>$3,462.33</td>
<td>$2,522.40</td>
<td>$2,711.01</td>
<td>$3,359.76</td>
</tr>
</tbody>
</table>

* The amount for 1932 contains $15.60 in royalties, and that for 1933, $47.00 in interest, from the Memoir Fund and in that respect these amounts are not analogous with the amounts for 1934 and 1935.

† The figures for 1934 and 1935 as tabulated show a surplus carried over (or deficit) which correlates with the difference between the balance on hand in the Regular Fund at the beginning of the year and the cash on hand at the end of the year, as well as between the total regular recurrent net income and the total regular net expenditure. The figures for 1932 and 1933 do not do so but the variation is less than $15 in each year. It accounted as in 1934 and 1935, the surplus for 1932 would be $446.73 and in 1933, $510.34. Except for a typographical error in the amount of the total account of publications for 1933, the figures for the years 1932 and 1933 stand as originally published. The second set of surplus or deficit amounts which have been listed below are computed by adding and/or subtracting accounts paid for previous and for following years. The total deficit for the years 1932 and 1933 is at variance but only to the extent of $9.77.
Anthropologist and Memoirs (distrib., storage, insur., net cost gratis reprints):

<table>
<thead>
<tr>
<th>Year</th>
<th>1933</th>
<th>1934</th>
<th>1935</th>
<th>1936</th>
<th>1937</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cost</td>
<td>$199.66</td>
<td>291.20</td>
<td>559.01</td>
<td>7.08</td>
<td>7.08</td>
</tr>
</tbody>
</table>

| Totals | $465.81 | $475.38 | $490.86 | $632.05 |
| Memoirs: printing and illus. paid by Ass'n. | 67.02 | 232.32 |
| Total account publications | $3,928.14 | $3,064.80 | $3,201.87 | $4,224.13 |
| Sec'y, Treas., and Ed.'s offices | 1,094.03 | 1,102.85 | 1,222.77 | 1,598.89 |
| American Council of Learned Societies | 25.00 | 25.00 |
| Anthropological Reprint Series | 122.55 |
| Reprinting and purchase out-of-print publs. | 296.22 | 165.82 |
| Totals | $5,318.39 | $4,333.47 | $4,449.64 | $5,970.57 |
| Surplus carried over or deficit | 433.12 | 500.02 | 713.39 | -378.48 |
| Annual surplus or deficit | $433.12 | -$573.90 | $982.56 | $440.43† |

Respectfully submitted

Cornelius Osgood, Treasurer

† This surplus exists despite the expenditure of $232.32 for Memoir Number 44.

It was voted that the Treasurer's report be accepted, subject to the findings of the Auditing Committee.

The President appointed the following Auditing Committee: Edward Sapir, Clark Wissler, George P. Murdock.

The Budget Committee composed, by appointment of the President, of the Executive Committee of the American Anthropological Association, presented the following budget recommendations for 1936.

**Budget for 1936**

1. Secretary's expenses: $100.00
2. Editor's expenses:
   - Editor's assistant: $960.00
   - Office expenses: 150.00
   - Total: $1,110.00
3. Treasurer's expenses:
   - Treasurer's assistant: 480.00
   - Office expenses: 100.00
   - Membership charges: 25.00
   - Total: 605.00
4. American Anthropologist:
   - Printing and Illustrating: 2,900.00
   - Reprints: 200.00
   - Distribution: 250.00
REPORT OF THE AUDITING COMMITTEE

The undersigned, appointed as Auditing Committee by the President, report that they have examined the Treasurer's accounts as submitted for the fiscal year 1935, and find them correct.

CLARK WISSELL, EDWARD SAPIR, GEORGE P. MURDOCK

REPORT OF EDITOR

This year's publication record is the AMERICAN ANTHROPOLOGIST in a volume of 716 pages and four Memoirs: No. 42—"Walapai Ethnography," edited by A. L. Kroeber; No. 43—"The Pueblo of Santo Domingo, New Mexico" by Leslie A. White; No. 44—"Hopi of the Second Mesa" by Ernest and Pearl Beaglehole; No. 45—"Kinship Systems and the Forms of Marriage" by B. W. Aginsky—a total of 1387 pages of printed material.

The AMERICAN ANTHROPOLOGIST is still confronted with the problem of finding space for articles. Since I assumed duty as Editor I have received several hundred manuscripts, of which it was possible to print but one in three. Under the circumstances it is obvious that we cannot print long articles. To give more than a minor fraction of the space to one author means then that others must be excluded. I have followed the policy, in general, of asking authors to pay for extra pages or illustrations when they felt condensation or reduction to be unwise, and I will continue this policy unless directed otherwise by the Association.

It does not seem to me that we can enlarge the volume at present. Nor does it appear necessary since authors have been unusually cooperative in condensing their papers when requested. Frankly, I believe that such compact presentation makes for clarity of thought and expression.

Longer papers should find their outlet in the Memoir series. But only a lucky chance brings worthwhile manuscripts with the money to pay for their printing. This leads me to remark that of the four Memoirs published this year, the Association paid for only one (No. 44), the briefest. I recommend that if funds permit we budget a sum to provide at least part of the cost of printing, and further that the Editor be authorized to enter into financial arrangements with authors and others for their share.
A new departure is represented in the designation of Memoir 42 as "Contributions from the Laboratory of Anthropology, I," making use of the Memoir series for the publication of a paper from a local institution. It seems to me that this example might well be followed by other institutions which issue only occasional monographs. The practice is a well established one in other fields, exemplified for instance in the Psychological Monographs.

We were able to affect a considerable saving in the cost of mailing the Memoirs by issuing them as supplements to the American Anthropologist, for which we have low second-class mailing privileges. This action was authorized by the Executive Committee. The identity of the Memoirs as a separate series is nevertheless preserved, since the designation appears only on the cover, which is normally discarded when volumes are bound.

At the suggestion of Dr R. H. Lowie we began reprinting articles from early and current issues of the American Anthropologist for general sale. So far six have been reprinted. That there was a demand for this service may be judged by the sale of 236 copies, with a net return of about half the cost. I recommend the budgeting of a further sum for the purpose in the belief that a larger series will stimulate the use and sale of all the items in it.

Respectfully submitted,

Leslie Spier, Editor

It was voted that the Editor's report be accepted.

An informal account of the deliberations of the Committee on Honorary Memberships (Spinden and Wissler) was given verbally by Dr Spinden. The Committee was continued and asked to report to the next annual meeting of the Council.

An informal account was also given by Dr Spier of the progress made by the Committee on Codification of Practices (Sapir, chairman, Collins, Guthrie).

It was voted that the Committee on Codification of Practices be continued and that there be added to its membership the incoming officers of the American Anthropological Association.

It was voted by the Council that the 108 new applicants for membership in the American Anthropological Association, whose names will appear in the next printed list of members, be elected to regular membership in the Association.

It was voted that it is the sense of the Council that the policy be definitely established of electing presidents to serve one year and one year only.

The president appointed the following Committee on Resolutions to report at the annual meeting, Dec. 28, 1935: E. C. Parsons, (chairman), H. J. Spinden, M. W. Stirling.

The report of the Nominating Committee, as given infra in the minutes of the annual meeting, Dec. 28, 1935, was approved for recommendation by the Council to the Association. The Committee in presenting its report commented as follows: "Our Constitution provides for four Vice-Presidents, one of whom is to be elected annually. In 1920 this was changed, arbitrarily it would seem, and the custom of having two Vice-Presidents elected annually was inaugurated. If the change is desired, the Constitution should be altered accordingly. In the meantime we have returned to the older system by renominating the two Vice-Presidents who served last year and by adding the names of E. Sapir and D. Jenness."

On recommendation of the Budget Committee it was voted that it is the policy of the American Anthropological Association that the Treasurer of the Association should send bills to and collect dues from members of local branches (not affiliated societies) of the American Anthropological Association.

It was voted that for the coming year the costs for non-paying members of the American Ethnological Society be allocated between the American Anthropological Association and the American Ethnological Society and that the whole matter of general policy involved be referred for study to the Committee on Codification of Practices.
The President appointed the following Committee on Abbreviations: A. L. Kroeber, L. Spier, J. M. Cooper. The function of the Committee will be to study the question of a unified system of abbreviated references to anthropological periodicals and serial publications.

It was voted that the invitation extended to the Association by Dr Sapir to meet at New Haven in 1937 be accepted with appreciation by the Council and be recommended with the Council’s approval to the Association at its annual meeting.

ANNUAL MEETING, DECEMBER 28, 2:00 P.M.

President Robert H. Lowie in the chair. The Nominating Committee (Swanton, chairman, Roberts, Speck) presented its report as approved by the Council. After presentation thereof the following officers, Council members, and representatives to councils and associations were elected:

President, Herbert J. Spinden
First Vice-President, Nels C. Nelson (1936)
Second Vice-President, Matthew W. Stirling (1936–1937)
Third Vice-President, Edward Sapir (1936–1938)
Fourth Vice-President, Diamond Jenness (1936–1939)
Secretary, John M. Cooper
Treasurer, Cornelius Osgood
Editor, Leslie Spier
Associate Editors, M. J. Herskovits, Cornelius Osgood, F. H. H. Roberts, Jr., F. G. Speck
Executive Committee, Fay-Cooper Cole, J. A. Mason, F. W. Hodge.

Council


Representative to Social Science Research Council: C. Wissler.


Representatives to Section H, A.A.A.S.: M. J. Herskovits, R. J. Terry.

It was voted that the budget for 1935 as submitted by the Budget Committee and approved by the Council be accepted.

It was voted that the following resolutions presented by the Committee on Resolutions (Parsons, chairman, Spinden, Stirling) be adopted.

1. Be it resolved, that the deep loss to American anthropological science through the deaths of Walter Hough and Marshall H. Saville, past Presidents of the Association, and of Frederick Dellenbaugh, H. F. Cleland, and Mrs Harold F. Ickes, members of the Association, be recorded in the minutes of this annual meeting and that copies of this resolution be sent to the close relatives of the persons here memorialized.

2. Whereas, The Tennessee Valley Authority has been authorized by Congress to build, in addition to the Norris and Wheeler dams, many other dams in the Tennessee River drainage which will create artificial lakes covering large areas of river bottom land, and

   Whereas, Numerous archaeological sites, some of considerable magnitude and known to contain a wealth of material and information, are located in the bottom lands of the Tennessee River drainage, often on the immediate banks of the present streams, and

   Whereas, The rapid progress of the construction of the authorized dams will bring about an early inundation of these archaeological sites, thereby causing important data to become lost to science and history forever, and

   Whereas, In spite of the excellent results of the archaeological studies in the Norris and Wheeler basins, the Tennessee Valley Authority at present apparently is making no attempt to preserve these additional archaeological records, therefore

   Be it resolved, by the American Anthropological Association that it record herewith:

   (1) Its grave concern over the probable wholesale destruction of irreplaceable scientific data now extant in the Tennessee River basin, and

   (2) Its earnest desire that every possible effort be made by the proper authorities to insure an adequate study of these data before their inundation, and,

   Be it further resolved, that this resolution be incorporated in the minutes of this meeting, and copies sent to Dr Arthur E. Morgan, Chairman of the Tennessee Valley Authority, Knoxville, Tennessee, and to the Division of Anthropology and Psychology, National Research Council, Washington, D. C.
3. Be it resolved, that the Secretary be instructed to express the gratitude of the American Anthropological Association to the officers of Phillips Academy, Andover, for the delightful hospitality offered to us and to Dr Warren K. Moorehead and Dr Douglas S. Byers and others for their many personal attentions.

The Philadelphia Anthropological Society communicated, through Dr D. S. Davidson, Secretary, its desire to become affiliated with the American Anthropological Association.

It was voted that the Philadelphia Anthropological Society, in accordance with this request, be so affiliated.

It was voted that the recommendation of the Council to accept Dr Sapir’s invitation to the American Anthropological Association to meet in New Haven in 1937 be adopted and that the invitation be accepted with thanks.

Dr Lowie appointed M. W. Stirling and J. M. Cooper as a Committee on Local Arrangements for the annual meeting at Washington in 1936.

It was voted that the Program Committee be instructed to issue a call early in October for papers designed for the annual meeting, the titles and full papers to be in the Committee’s hands by November 1st, and that the Committee be authorized to select a limited number of papers and addresses to be presented at the joint sessions of the annual meetings of the societies involved; that the names of the Program Committee be printed in the AMERICAN ANTHROPOLOGIST in the issue in which those of the Nominating Committee are printed; that the members thereof be chosen so far as possible from an area restricted enough geographically to permit of conferences by the members without necessity of financial expenditures.

It was voted that the Editor be empowered to enter into arrangements with authors or others for their payment, in part or in full, of the costs of publication in the AMERICAN ANTHROPOLOGIST and the Memoirs.

It was voted that the Secretary draw up an expression of the American Anthropological Association’s deep appreciation of the cooperation given to anthropological research by the American Council of Learned Societies and particularly of the assistance given thereto by Dr Waldo G. Leland.

PROGRAM
FRIDAY, DECEMBER 27
9:30 A.M.

C. W. M. HART, A Reconsideration of the Social Organization of the Natchez
WILLIAM N. FENTON, Contemporary Culture of the Tonawanda Seneca
E. ADAMSON HOEBEL, Association and the State in the Plains
ALEXANDER LESSER, Outlines of Kiowa Ethnology
GENE WELTFISH, The Economics of the Pawnee Village
JULIUS E. LIPS, Public Opinion and Mutual Assistance among the Montagnais-Naskapi
REGINA FLANNERY, Status of Women among the Northern Algonquians
DAVID G. MANDELBAUM, European Factors in Plains Cree Culture

2:00 P.M.

WILSON D. WALLIS, Greek and Iranian Parallels in Polynesia
RAYMOND KENNEDY, Remarks on Social Organization in Western Indonesia
D. SUTHERLAND DAVIDSON, Australian Rock Carvings and Paintings (Lantern)
H. NEWELL WARDLE, Belts and Girdles of the Inca’s Sacrificed Women: A Study in Technique
ERNESTINE WIEDER SINGER, A Study of the Techniques of Three Hairnets from Pachacamac
WILLARD Z. PARK, On the Nature of Band Organization among the Paviotso
RUTH L. BUNZEL, Cultural Horizons in Pueblo History, a Study in Calendrical Correlations
EDWARD A. KENNARD, The Role of the Father’s Sister in Hopi Culture (By Title)
EDWARD SAPIR, Linguistic Evidence Suggestive of the Northern Origin of the Navaho (By Title)

8:00 P.M.
MATTHEW W. STIRLING, By Airplane to the New Guinea Pygmies (Motion Pictures)

SATURDAY, DECEMBER 28
9:00 A.M.

Forum on the Navaho
GLADYS A. REICHARD, The Use of Parallels in Historical Reconstruction
A. V. KIDDER, The Navaho in the Light of Archaeology
RUTH F. BENEDICT, Distribution of Southwest Folk Tales
ELSIE CLEWS PARSONS, Relations between Navaho and Pueblos
LELAND C. WYMAN, Navaho Diagnosticians
WALTER DYK, A Theory and Some Practices on Property Inheritance among the Navaho
ELSIE CLEWS PARSONS, Riddles and Metaphors among American Peoples
PHILLIPS BARRY, A Note on the Psychopathology of Ballad-Singing

2:00 P.M.
ROBERT H. LOWIE, Lewis H. Morgan in Historical Perspective
WILLARD K. MOOREHEAD, The Project for an Indian Encyclopedia
JOHN R. SWANTON, The Siouan Tribes of the Ohio Valley (Lantern)
GEORGE P. MURDOCK, Cultural Position of the Tenino (Warmsprings Sahaptin)
ERNA GUNTHER, A New Approach to Native Knowledge of Environment
HARRY TURNER-HIGH, The Flatheads, a Study in Marginalism
FREDERICA DE LAGUNA, Ethnology of the Eyak Indians, Alaska
JOHN M. COOPER, Scapulimancy

6:30 P.M.
Annual Dinner of the AAA, AFLS, and SAA
8:00 P.M.

WALDO G. LELAND, The American Council of Learned Societies and Anthropological Research
H. SCUDDER MEKEEL, Anthropology and the Office of Indian Affairs
GREGORY ZILBOORC, Suicide among Primitive Peoples

SUNDAY, DECEMBER 29
9:30 A.M.

DOUGLAS S. BYERS, Pottery Making at Las Guabas, Coclé Province, Panama (Motion Pictures)
H. J. SPINDEN, The Intermediate Period of Maya Archaeology (Lantern)
LINTON SATTERTHWAITE, JR., Masonry Buildings at Piedras Negras which Lacked the “Maya Arch” (Lantern)
CARL C. SELTZER, The Racial Anthropometry of the Zuñi Indians, with Special Emphasis on the Relationship between Archaeological and Physical Data in the Southwest

DOROTHY L. KEUR, A Prehistoric Example of Erosion Control (Lantern)

ARTHUR C. PARKER, Iroquois Effigy Combs (Lantern)

WILLIAM A. RITCHIE, New Evidence Relating to the Archaic Occupation of New York (Lantern)

ALLEN H. GODBEY, The Patina Factor in Archaeological Chronology

2:00 P.M.

RUTH C. MACKAYE, Anthropometry of the Female Insane

THOMAS HORACE EVANS, Canon de Chelly (Pueblo) Foot Bone and Hand Bone Types (Lantern)

CARLETON S. COON, Racial Studies in Southern Arabia

REGINALD G. FISHER, A Summary of the Relation of North American Prehistory to Postglacial Climatic Fluctuations (By Title)

DOUGLAS G. HARING, An Anthropological Perspective on Japanese Policy

JOHN GILLIN, Further Consideration of the Configuration Concept in Culture

C. W. WEIANT, Acculturation in Tarascan Villages

CORA DU BOIS, Some Objectives and Techniques in Ethnographic Field Work

JULES HENRY, Integration in Simple Societies

MARGARET MEAD, The Study of Character Formation in Primitive Cultures (By Title)

VINCENZO PETRULLO, Ethnology of the Llanos of Venezuela and Colombia

JOHN M. COOPER, Secretary

American Anthropological Association
BOOK REVIEWS

NORTH AND CENTRAL AMERICA

*The Pinto Basin Site*. ELIZABETH W. CROZER CAMPBELL AND WILLIAM H. CAMPBELL. (Southwest Museum Papers, No. 9. 51 pp., 17 pls. $0.50. Los Angeles: Southwest Museum, 1935.)

In the Mohave Desert region of southern California, Pleistocene lakes and stream courses amid lacustrine clay-beds containing fossils have been recognized, partially described, and dated by competent geologists. Human vestiges have been found by Mr and Mrs Campbell in these same sites. Is it not possible to establish an undeniable connection? they asked themselves.

This paper is a brief indication of one of the arduous efforts the Campbells have made to answer their own question. A large, uninhabited area was selected in the eastern end of Pinto Basin. In the course of more days in the field than most expeditions can allow, this was thoroughly examined from the geological, physiographical, palaeontological and archaeological viewpoints. Mr David Scharf, of the California Institute of Technology, presents the geology. Mr and Mrs Campbell tell of the actual field work. Mr Charles Avery Amsden, of the Southwest Museum, describes the artifacts.

Their analyses point towards a common conclusion. A Pleistocene river and an older lake with banks and beds of fossiliferous clays were established. On the shores were numerous artifacts. The clear cut features of the old river, the very slight rainfall of the region, the location of the artifacts with reference to the topographical features, the absence of sherds (a feature of modern southeastern Californian culture), the "primitiveness" of the culture are adduced to suggest a remote antiquity for man in the region.

The result is not a proof, as they themselves suggest. But the great potential importance of their work is clear. Excellent (and threadbare) though it is, stratification is not usefully applicable to much Californian archaeology. The exuberant exactness of tree-rings has so far been of no help. Hence any attempt to use in a new way established (or should one say, orthodox?) geological criteria may well be a real advance towards that more definite identification of geological and archaeological theories which is much needed in both sciences.

Twentynine Palms, California


These volumes provide one of the first treatments of an American Indian language the field research (1928) for which was sponsored by the American Council
of Learned Society's Committee for Research in American Indian Languages. The grammatical sketch is a section of the long awaited Part 3 of Professor Boas' "Handbook of American Indian Languages." From 1926 to 1928 Dr Andrade studied a mass of inchoate and phonetically inadequate manuscript material in Quileute, including a fair body of texts, collected about ten years earlier by Dr Leo J. Frachtenberg. Dr Andrade's six weeks at La Push, Washington, the principal Quileute speaking settlement, provided only a brief time to repeat and check over with living informants all the Frachtenberg text data and to revise all the morphologic conceptions based on the clues that could be pursued in the defective Frachtenberg transcription. However, in the field Dr Andrade adapted himself skillfully to the phonologic changes his audition obliged him to make; he also worked through the morphologic tangle with extraordinary rapidity, sharpness, and awareness for minor detail; besides, he added some eighty pages of fresh texts to the Frachtenberg collection. The text volume comprises a total of seventy texts: forty-four in a transcription revised from Frachtenberg's original, twenty-six recorded directly by Dr Andrade; most of the texts are myths.

Dr Andrade's indebtedness to Professor Boas has been considerable. There is most impressive evidence of minuteness of observation and analysis. The phrasing of the structure of the language displays some striking and attractive characteristics that involve additions to the Boas technique and that may influence the style of description adopted by other linguists. There is a charmingly frank and illuminating weighting of alternative hypotheses, a procedure which gives the reader the feeling of peering over the shoulder of the exacting and resourceful field worker. Peculiar problems are met by applying old and familiar European points of view which when shown to be inappropriate are followed by an original and more plausible synthesis; there is apt wording of the differences between traits studied in European linguistics and new and only slightly similar traits appearing in the Indian. This aspect of the presentation is pedagogically valuable even though it may not be long lived; comparison with traits of adjacent Indian languages, when they are better known, may provide closer analogues and will reveal regional trait isoglosses. Dr Andrade employs the European analogues only to shed more light on Quileute traits, for present day readers; the analogic treatment never warps the Quileute picture, it reveals it. Parallelizing this expert guidance through new linguistic territory is a style of expression so clear and invariably intelligible that non-linguists may follow with pleasure. Here perhaps more comfortably than elsewhere may the student learn the manner in which a field linguist attempts to identify and characterize unfamiliar linguistic processes.

Melville Jacobs

University of Washington


Tales from the Hoh and Quileute. Albert B. Reagan and L. V. W. Walters.
Dr Adamson's collection of myths not only represents the largest and most complete recording of Coast Salish folklore made to date, but emphasizes particularly those tribes from which data has been scantiest. The collection includes eighty-nine tales from the Upper Chehalis, sixty-six from the Cowlitz, fourteen from the Humptulips, and three from the Satsop. Fifteen Puget Sound tales are included also, from the Puyallup, Skokomish, and White River peoples. The stories were recorded in English, given direct by the narrator, and others rendered through an interpreter, except for a few that are text translations. Native words, phrases, and songs are often recorded in phonetic transcription. The method of transcription is fully explained; unfortunately it varies considerably from that recommended by the Committee of the American Anthropological Association. With a very few exceptions all the stories refer strictly to the mythological period.

So far as a reasonably acceptable English style would permit, the actual words of the native narrators apparently have been retained, though no specific statement on this point is given. An introductory section describes the conditions under which the myths were told and furnishes information regarding the informants. Valuable notes on the geographical location of the Upper Chehalis and Cowlitz are included here also.

A nine page appendix contains transcriptions of nineteen songs occurring in the myths. The songs were recorded on phonograph and dictaphone records and were transcribed by Dr George Herzog.

The student interested in comparative mythology of the region will welcome the abstracts. A certain number of cross references to whole tales or episodes (not incidents) are given, but the selection of sources for comparison seems curiously arbitrary. Dr Andrade's texts from the nearby Quileute are not used, nor are Dr Gunther's tales from the Klallam, though the latter is the largest pre-existing collection from the Coast Salish. Likewise Dr Frachtenberg's Coos Texts are omitted, though a single Hanis Coos myth is used for comparison. Nor are the Folktales of Salishan and Sahaptin Tribes (edited by Boas) utilized, though Dr Jacob's Northwest Sahaptin Texts (incorrectly cited as Sahaptin Myths) are included.

The Tales from the Hoh and Quileute consist of fifty-two stories, of which forty-five were recorded by Mr Reagan between 1905 and 1909 while serving as government agent for the Quileute and Hoh Indians. The task of editing this collection fell to Miss Walters in 1932, whereupon she collected a further seven tales from a Quileute informant, to be used as a standard in editing the Reagan material.

Without the explanatory footnotes, the Reagan tales, in their edited form, would hardly be distinguishable from those recorded by Walters, except that the latter are somewhat longer and in greater detail. Yet the Walters recordings were made verbatim, sentence by sentence, while the Reagan narrations originally were couched in flowery terms supplied by the recorder.
Obviously it would be dangerous to use any of the tales in this collection, other than those recorded by Walters, for purposes of stylistic study. However, Andrade's Quileute Texts furnishes excellent material for that purpose and the present collection makes a valuable supplement for study of motif and plot, and adds to the number of available variants.

VERNE F. RAY

UNIVERSITY OF WASHINGTON


These articles, amply illustrated with figures and plates, demonstrate the breadth of archaeological interest of the Carnegie Institution and are real contributions to knowledge. Their subject matter ranges from a study of the floral environment of Yucatan, through brief comparative cultural studies based on archaeological field work, to a technical statement dealing with Maya epigraphy. Article 5 by A. Ledyard Smith, describes "Two Recent Ceramic Finds at Uaxactun." The finer, presumably ceremonial pottery fragments found in an abandoned sanctuary of Pyramid A-I are described in some detail. Below this deposit, a complex burial vault was discovered which contained eleven vessels (three reproduced here in color). Dr Morley has contributed a discussion of the Initial Series appearing on one of the cylindrical vases from the tomb. Article 6, "The Engineering Knowledge of the Maya," by Lawrence Roys, is a careful study by an engineer of certain problems involving the principles of mechanics, as illustrated in the structural design and utility of the permanent buildings of the Old and the New Empires. It points out that the use of lime cement is the factor which is largely responsible for the direction of this architectural progress. An important phase of archaeological research which has not received the attention it deserves in the Maya area is reported upon in Article 7, "House Mounds of Uaxactun, Guatemala," by Robert Wauchop. This study of the remains of the houses of the common people is divided into three sections: first, a survey of the early sources; second, a review of previous excavation of house mounds; and third, a detailed account of the conditions and materials associated with five of "the low, inconspicuous mounds which are scattered, singly or in clusters of two to four and five, on inhaitable land in the vicinity of the major groups of ruins." Appended is a series of detailed notes on the pottery from these mounds by Edith B. Ricketson. Article 8, "Ruins of Polol and Other Archaeological Discoveries in the Department of Peten, Guatemala," is a factual account by Cyrus Longworth Lundell of discoveries made in the 1933 season, which included: two new sites (Polol, a city of the Old Maya Empire, and Chakantun, apparently dating from the Tayasal period); nineteen new stelae (fifteen in Polol and four in Topoxte); a pictograph at Tayasal; and further explorations at Topoxte. The now famous "Yaxuna-Cobá Causeway" is the subject of Article 9 by Alfonso Villa R. A brief discussion of previous references to the "white roads" of Yucatan precedes a de-
talled account of the 100 kilometer traverse of this road made by Sr Villa and his party. Some beliefs and legends of the modern Maya concerning the causeway and Cobá are also recorded. J. Eric Thompson has contributed two articles to this volume. Article 10, "Sky Bearers, Colors, and Directions in Maya and Mexican Religion," is a critical analysis of the evidence in documentary records concerning these religious symbols. An imposing list of parallelisms strengthens the bonds known to link Mexican and Maya religion, ritual, and mythology. Article 11, "Maya Chronology: The Fifteen Tun Glyph," announces Thompson's discovery that a special glyph is found only with dates that mark the conclusion of fifteen Tuns after a Katun. Problems associated with this glyph cause a discussion of the significance of the Tun, the Haab, and the so-called Lahuntun glyph. Article 12 is another contribution by Cyrus Longworth Lundell, entitled "Preliminary Sketch of the Phytogeography of the Yucatan Peninsula."

CARL E. GUTHE

University of Michigan

Tenayuca. Estudio Arqueologico de la Pyramide de este Lugar, Hecho por el Departamento de Monumentos de la Secretaria de Educacion Publica. (350 pp., illus. Mexico: Telleres Graficos del Museo Nacional de Arqueologia, Historia y Etnografia, 1935.)

"Tenayuca," the sumptuous report just issued by the Mexican Department of Monuments, fulfills a great need among Mexicanists, by giving a masterly exposition of the culture and history of the Aztec and Chichimec populations of the Valley of Mexico. Under the able leadership of José Reygadas Vertiz, chief of the department until 1935, excavations and research were carried on for ten years, and the accumulated data were examined by a body of specialists each of whom contributed to this report articles on his particular phase of the investigation. Thus Enrique Palacios and Roque Ceballos Novelo covered the documentary and traditional history of Tenayuca. Reygadas Vertiz described the exploration and preservation of the pyramid and Ignacio Marquina gave a searching architectural analysis of the monument. The paintings on the structure were examined by Miguel Angel Fernandez, and chapters on the orientation of the pyramid and its religious significance were contributed by Marquina, Palacios, and Luis R. Ruiz. The task of describing the field archaeology fell into the competent hands of Eduardo Noguera, who gave a much desired classification of Aztec ceramics. Moises Herrera presented a zoological study of the serpents carved on the monument. Finally Alfonso Caso and Enrique Palacios had two chapters each explaining the religious symbolism involved in the construction and decoration of the temple. The scope of the Tenayuca report is therefore broad and covers the notable advances in our knowledge of the pre-Conquest history of Mexico, made by the distinguished authors of this volume.

To review in detail so comprehensive a volume is impracticable, but two aspects of the work are especially significant. One is the convergence of the lines of historical
and archaeological research in the Valley of Mexico, which gives to archaeological technique a much-needed emphasis on human factors in contrast to the previous stressing of the statistical treatment of material culture. The other is the broad concept of archaeological research, shown by the Mexican Department of Monuments.

That historical and archaeological techniques have converged at Tenayuca is demonstrated by the unanimity of the conclusions of the various authors. Tenayuca figures largely in the pre-Conquest chronicles, first as the seat of the Chichimec rulers, later as an important fief of the dominion of the Tepanecs of Azcapotzalco. After the Aztecs of Tenochtitlan supplanted the Tepanecs in their hegemony over the Valley of Mexico, Tenayuca still retained its importance, which did not dwindle until some years after the Conquest. Corroborating the long occupation of Tenayuca implied in the chronicles, are the eight stages of construction, of which six at least represent the rebuilding of the temple and its platform on a successively larger scale, the latest edifice being in the style of the Great Temple at Tenochtitlan, begun in 1450 and completed near 1500. If, as Marquina suggests, these additions were made as a part of the cyclical ceremonies at intervals of fifty-two years, there is close correspondence between the hypothetical date of the foundation of the Tenayuca temple and the legendary one assigned to the Chichimec settlement.

The data from ceramic stratigraphy seem equally corroborative of the historical situation, although in this aspect, three periods only were isolated. The latest epoch, in producing Aztec pottery like that found in the Tlatelolco district of Mexico City, must represent the period of Tenochtitlan domination. The second phase, by far the most abundantly represented, yields Aztec II pottery, which is relatively rare in Mexico City, and that distinctive local style called Coyotlatelco by Professor Tozzer and found most abundantly in the Azcapotzalco region. This epoch may be associable with the Tepanec period, even though Coyotlatelco pottery, as yet, has not been specifically associated with sites occupied by the Tepanecs. The earliest ceramic phase at Tenayuca is represented by fragments of pottery vessels related in ware, form, and decoration to the widespread Cuicuilco-Ticoman-Gualupita II complex. This pottery, if not pre-Teotihuacan in date, may well represent the early Chichimec culture.

The Tenayuca report, in demonstrating thus the complicated history of the Valley after the fall of Teotihuacan and previous to the rise of the Tenochtitlan Aztec, has blazed the way to a broad field for investigation. Like the previous great report of the same department, “La Poblacion del Valle de Teotihuacan,” it reveals a wide horizon in Mexico’s past. In examining these two works, one is impressed by two conditions: first, that the sum total of a developed Middle American culture complex with its ramifications into history, religion, social customs, architecture, and variegated material culture can only be analyzed through the work of specialists in the various fields, and, second, that the concerted efforts of the authorities in the Department of Monuments, as exemplified in “Tenayuca” and “La Poblacion del Valle de Teotihuacan,” serve as a model for the organization of historical research of this character to a government or to an institution. The individuals who
wrote the Tenayuca report should be congratulated on the masterly exposition of their results, but we should not forget to praise the Mexican Government which through its enlightened educational policy made possible the existence of the Department of Monuments and its research.

G. C. Vaillant

**American Museum of Natural History**


Dr Caso's preliminary report on the season of 1934–1935 at Monte Alban reveals highly important results. The work centered on the Sunken Patio of the North Platform, Mound B of the North Platform, the Central Mound, Mound L or the Temple of the Dancers, Mound X, and in the tomb fields. Work was continued at Mitla.

In previous years Monte Alban has produced such marvels as the incredibly rich Tomb 7 and the majestic platforms of the northern group of buildings. This season, however, yielded a chronological grouping for the pottery that should be of great benefit in tracing aspects of culture diffusion in Central America, in that two of the periods seem to make contact with Maya ceramics.

Period I produced material qualitatively comparable to the "archaic" groups of Central America; in other words competently executed vessels, by no means primitive, yet far below the ceramic product of the "civilized" tribes of that region. Period II yielded those distinctive forms often found under conditions of relative antiquity in the Maya area, and sometimes referred to as the Q complex. Period III was represented by vessels, some of which in shape and decoration had a strong generic resemblance to the ceramics of Yucatan. Period IV might be described as "classical" Zapotec, since the great mortuary urns are associated with this epoch. The ceramics of Period V are strongly impregnated with influences from the Mixteca and represent an occupation of the neighborhood of Monte Alban by those tribes.

It is chiefly through the establishment of many culture sequences like the one defined by Dr Caso at Monte Alban, that we can hope to create a significant historical pattern for Central America. If the preliminary stages seem to increase the complexity and confusion of the field archaeology of the area, the later ability to cross-date such sequences should dispel many of our perplexities. In continuing his Monte Alban project, Dr Caso is not only building a basal cultural and chronological structure for Oaxaca, but is also establishing a gauge to measure the interplay of cultural influences between the Mexican plateau and the highland and lowland Maya cultures.

G. C. Vaillant

**American Museum of Natural History**
AFRICA


Mr Hambly clearly expresses his aims in this work. There are numerous cultural traits revealing what are probably distinct stratifications of culture. These diverse elements have been welded together into a pattern, the examination of which constitutes the present problem.

The object of this study is an analysis of these traits with a view to showing the sequence in which they have been received, from whence they came, and the processes which have been responsible for coordinating them so as to form the present social system.

The author has been more successful in the first two objectives than in the third.

There are one hundred and seventy-seven pages of description whose chapters are captioned Geographical Factors, Historical Sources, Physical Appearance, Economic Life, Social Life, Education, Language, and Religion. More details in certain of these phases would probably have given a sense of complexity lacking in the present description. Thus geography is only slightly treated and the map does not materially elucidate specific queries. No idea of total population is given and there is no systematic description of tribal subdivisions.

Two chapters follow which are entitled Cultural Contacts. The first of these shows trait distribution between the Ovimbundu and the Congo basin, Rhodesia, and Southwest Africa. The second includes distributional reviews of five topics in the light of Africa as a whole. Summarizing tables would doubtless have clarified and condensed these sections. The second chapter closes with a résumé of the Kulturkreise assumption of Oceania-West African contacts whose validity Mr Hambly questions. The last chapter is called Cultural Processes. In it much of the previous descriptive material is repeated to illustrate sources of Ovimbundu traits, cultural losses, and trait integration.

The volume as a whole presents a sound theoretical outlook and has value as a systematic description. However, one feels that theory and description do not blend as fruitfully as Mr Hambly attempted to make them. For instance, the author states in the section on Integration of Traits that

To the methods of research already followed there should be added a psychological approach with the object of showing the way in which various elements of culture are blended and made to function (p. 337).

This may be a legitimate viewpoint but the ensuing paragraphs simply repeat that vocabulary reflects knowledge of nature lore, etc., or that marital psychology is revealed when a husband picks burrs off of his wife’s skirt as a sign of reconciliation after a quarrel.

The plates deserve the highest praise. An index enhances the usefulness of the volume.

*Cora Du Bois*

*Harvard Psychological Clinic*
Le Antiche Rovine e Miniere della Rhodesia. LIDIO CIPRIANI. (x, 103 pp., 43 pls., 2 maps. Firenze: R. Bemporad e Ciglio, 1932.)

Osservazioni sui Pigmei centro Africani. LIDIO CIPRIANI. (Archivio per l’Antropologia e Etnologia, Vol. 63, Nos. 1, 2, 18 pp., 7 pls. Firenze, 1933.)

A recent article called attention to the growing interest of Italian scientists in the exploration, ethnology, and natural history of Italian possessions in Somaliland, Tripolitania, Cyrenaica, and Libya.¹

Two publications of Professor Ldidio Cipriani indicate that Italian interests are by no means limited to local problems within Italian spheres of influence. A map in that first cited above indicates that he traversed the length and breadth of Africa during the period 1927–30, and his work “In Africa dal Capo al Cairo” describes the greater part of the route followed.²

The article “Osservazioni sui Pigmei centro Africani” gives the measurements made by Cipriani on Pygmies of the Tiki-Tiki, located in the Ituri region, northeast Belgian Congo. The average stature of the six males was 1386 mm., and the stature of the females 1285 mm. For the males the average C.I. was 77.9 and the most brachycephalic was 81.1. The females were slightly more brachycephalic than the males, for the average C.I. was 80.8 and one woman had an index of 83.5.

Cipriani compares his small samples with the more adequate data of Czekanowski, Schebesta, and Poutrin, while he rightly makes distinction between true Pygmies such as those of the Ituri, and the taller “pigmoidal” brachycephals of the Sanga, who were Poutrin’s principal study. Measurements and face masks from the Ituri Pygmies are, however, only a part of Cipriani’s contribution to physical anthropology, for he has, in addition to anthropometric measurements of many Negro and Hamitic tribes, a large collection of face casts from tribes as widely separated as the Tuareg and the Zulu. Photographs in the article relating to the Ituri Pygmies make comparisons of hands and feet of Europeans, Pygmies, Negroes, and Bushmen. The article concludes with a bibliography of thirteen items relating to physical anthropology of Pygmies.

The publication “Le antiche rovine e miniere della Rhodesia” describes the ancient ruins and mines of that region, and the study is assisted by a clear map. The work is a summary of the history, archaeology, and local ethnology of the Zimbabwe area and stone ruins associated therewith. Eight chapters deal with the following subjects: 1, The first arrival of Europeans in South Africa, and the decline of ancient Rhodesia; 2, The principal characteristics of the ruins; 3, Human remains and objects brought to light during excavations; 4, Washing for metals, and mining activities; 5, The end of mining operations; 6, Bronze and casting in Rhodesia; 7, Ancient exportation of gold from Sofala; 8, The African origin of the ruins.

For students of archaeology the work is a valuable summary which includes a

² Firenze, 1932.
bibliography of eighty items from fifty-nine authors. Forty-three photographic plates, some of which are borrowed from acknowledged sources, include views of archaeological sites, details of artifacts unearthed, and physical types of Bantu tribes of Rhodesia. An index would have greatly enhanced the value of a work whose main object is the provision of a summary of the facts and theories pertaining to the growth, acme, and decline of Zimbabwe, with its associated industries, physical types, and socio-religious background.

Wilfrid Dyson Hambly

Field Museum of Natural History


The Rev Canon J. Roscoe based his work "The Baganda" (London, 1911) on information given by Sir Apolo Kagwa, though no doubt Canon Roscoe's long residence in Uganda served to amplify the data. Sir Apolo Kagwa wrote his own account of Baganda customs in Luganda with the definite intention of improving on the presentation of Canon J. Roscoe.

To make a critical appraisal of this effort would involve a detailed comparison of the two works in order to assess the relative merits of their subject matter and arrangement, a task far beyond the possibilities of a brief review. Mrs Edel, the editor, explains that the translation of Sir Apolo Kagwa's book is essentially an annotation and an expansion. It is true that certain portions of the Luganda edition have been omitted from the translation because of their identity with chapters of Roscoe's monograph, yet new sections on games, and a greater wealth of historical detail characterize the translation of Kagwa's Luganda edition.

The Luganda edition is as purely objective as that of Roscoe, the arrangement being of a formal kind without any particular claim to logical sequence or effort to show the mutual dependence of the parts. The translation describes ceremonies centering round the king, marriage, women's taboos, twinning, food taboos, death customs, gods and fortune tellers, judicial procedure, hunting customs, blood brotherhood, musical instruments, fishing, canoe-building, the navy, pottery, metal-working; then back to the pomp and ceremony of royalty. The book concludes with twenty-five pages of Luganda words, phonetically spelled, which have been retained in Kalibala's translation.

Allowing for omissions in the translation to avoid repetition of Roscoe, there is some argument in favor of Roscoe's grouping of data which begins with a general survey of the country and its people, then follows a life cycle of events: birth, infancy, puberty, marriage, sickness, death, burial. Roscoe gave a detailed account of relationship systems, clans, and totems, and further expanded his account of social organization by a discussion of royal prerogatives, religion, and war-time activities. Next in Roscoe's work came the economic side of tribal life: domestic animals, agriculture, hunting, markets, and currency; lastly folklore, and anthropometric tables. Ethnologists are glad to have both works, but if pressed to make a decision
between them there might be some justification for preferring the original to the
"improved" version.

The value of Roscoe's work as a background against which present-day cultural
change may be appraised has recently been demonstrated by L. P. Mair in her "An
African People in the Twentieth Century,"¹ and perhaps Africanists would agree
that both the works of Roscoe and Kagwa, though of the static, objective kind are
of inestimable worth. That Sir Apolo was not free to attempt a subjective work
relating to the social and psychological composition of his own heritage is hinted in
the preface to this translation, but such a work for the Baganda, as for other African
tribes, would be the ethnological complement to existing arrangements of factual
material. The historical and sociological importance of the Baganda call for an
inner and intimate study after the manner of "Chaka,"² "Man of Africa,"³ "The
Autobiography of an African,"⁴ and "People of the Small Arrow."⁵

WILFRID DYSON HAMBLY

FIELD MUSEUM OF NATURAL HISTORY

Die Baja, ein Negerstamm im Mittleren Sudan. Bd. 2, Ergebnisse der Expedition zu
den Baja 1913–14: Teil 1, Materielle und seelische Kultur. GUNTER TESSMANN.
(xii, 243 pp., 322 figs., 36 pls., map. Stuttgart: Strecker and Schröder, 1934.)

Die Nyamwezi: Gesellschaft und Weltbild. WILHELM BLOHM. (xiii, 208 pp., 186 figs.,
8 pls. Hamburg: Friederichsen, de Gruyter and Co., 1933.)

The Baya live in the eastern portion of the Cameroons, not far from the Pangwe,
who were described by Tessmann some twenty years ago. Beginning with a brief
discussion of the linguistic affiliations of these people and their physical traits,
analyses of the population of each of several villages are given, after which the author
proceeds to a discussion of material culture. One useful portion of this section has to
do with forbidden foods (pp. 80–88), an important element in all African cultures
which has been greatly neglected. The major portion of the remainder of the book
deals with technology: the brief section on art is of little value; but the accounts of
games are more competently handled. Musical instruments are carefully described
and some of them figured: of the folklore, twenty riddles, three proverbs, and twelve
tales are presented.

Blohm's study of the Wanyamwesi deals first with social organization. Next
comes a consideration of social classes, followed by a conventional discussion of
political organization. To be remarked are the detailed studies of death customs, of
totemism, and of magic. The closing section deals with the "demons" and gods
worshipped by these people. There is a fine series of some two hundred line drawings

¹ London, 1934.
² T. Mofolo, Chaka (London, 1931).
³ S. Y. Ntara, Man of Africa (London, 1934).
⁵ J. H. Driberg, People of the Small Arrow (New York, 1930).
which illustrate both objects of everyday and of religious use. In addition, one finds a number of excellent plates of photographs, which show the physical types and dwellings of the people.

Both these books are almost entirely innocent of any indication that individual differences in attitude, temperament, or points of view exist among the people studied, and thus afford but little data for those who are interested in anthropological problems which have come to the fore in recent years.

Melville J. Herskovits

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OCEANIA

*Kinship in the Admiralty Islands.* Margaret Mead. (Anthropological Papers, American Museum of Natural History, Vol. 34, Pt. 2. 178 pp., 17 figs. $1.75. New York: American Museum of Natural History, 1934.)

Dr Mead has produced a most valuable kinship study. The mere collection of the data must have required immense patience and skill. The Manus are a people among whom the kinship structure is of fundamental importance in social and economic life. Their strict code of morals carries the sanction of the cult of the dead, yet good informants gave genealogical data that did not tally with that of their closest relatives, even on such important points as own parents. Were these informants stupid, or lying? And if the latter, what motives had they for their conduct? A fair understanding of the whole culture must be reached and Manus values appreciated before it can be understood that these persons were both truthful and intelligent.

The Manus are essentially middlemen. Except for the fish they catch, they depend entirely on outside products: economic success is their ideal in life.

"To understand *kawos*" (trade) in Manus means actually to understand the manipulation of the kinship categories, to exploit one's possible relationship claims to the full and to be able to find ways of rewriting any relationship should it seem desirable. Samoan society presents the picture of a fixed hierarchy into which the able climb. Manus presents each individual with a set of possibilities which the most able will recognize and use (p. 313).

The kinship system gives a person definite privileges over certain relatives; there is a precisely defined preferential marriage. Yet a wise man chooses his relatives, and thus builds up his economic position as well as ensuring his own cult when dead, and may even raise his own family from plebian to chiefly rank:

An adult views his genealogy from a strictly practical point of view, not of what blood runs in his veins, but of what kin connections he has upon which it will be profitable to act (p. 324).

In this he is aided by two important factors, one inherent in his environment and the other a cultural feature.

In an enclosed area, although the taboo on marriage within the gens is observed, actually everyone is in fact related in some way to everyone else; it is but necessary to trace the path. In doing so no use is made of classificatory kinship, but kinship is claimed by the astute to useful members of other clans through various family
connections and by adoption. The latter practice is very common, and is determined mainly by economic considerations. An adopted child can count his relatives through his real or his adopted parents or through both, as he finds most profitable. But not only adopted children have this privilege; anyone can claim kinship with another if the bond promises to be profitable, counting the line through some adopted person in his own, his wife's, or his mother's family.

Thus the Manus have solved great social problems without recourse to revolt against their own formal social system. They have changed the spirit but not the letter of their own kinship practice. While retaining a kinship system with classificatory terminology, itself giving scope for communal activities of an economic nature and pivoted on the preferential marriage with its elaborate system of exchanges between the families united by marriage—so usual in Oceania—they have yet developed a system which gives full rein to individual capitalism. In the exchanges in which a large number of people are involved and property passes from one group to another, individual contributions from persons within one kinship group are received by individuals in another group. Although property passes along defined lines, yet it is willed by a man to his ablest heir. The same process is at work in the succession to rank.

The Manus seem to pay a high price for the smooth working of their highly individualist society with its many pitfalls likely to cause enmity, in the rigid code of morals imposed upon them by their own dead. The patron ghost of the household, called "Sir Ghost" by Drs Mead and Fortune, brings illness on his descendants in the male line for departure from an extremely puritan code of morals. Death is never a natural phenomenon: it is caused by the anger of the ghosts, and it is interesting to note that while in so many societies it is usual to project the sense of guilt and find in the sorcery worked by some enemy the cause of death, the puritan Manus introject their sense of guilt and attribute almost all misfortune to misconduct on the part of some member of their own household.

Here may be mentioned the interesting custom called ngang. When a death occurs, the men of the deceased's mother's family pillage the house ceremonially and actually throw into the sea the skull of "Sir Ghost," who is now deposed from his place of honor. But he is succeeded not by a member of the pillaging family but by the recently dead man. The Manus explain this custom by saying that the mother's kin do this because they are outraged by the wanton behavior of "Sir Ghost," who has not used his power to protect a son of one of their women. Dr Mead regards the ngang as a formal presentation, a ceremonial conflict between two cults—that of the "Sir Ghost" and the tandritanitani cult.

The two cults represent the Manus ideas of kinship in an interesting way. The "Sir Ghost" cult belongs to the unilateral male line in which succession is counted. The tandritanitani is connected with an unnamed group consisting of the descendants of a brother and of a sister, which holds together for three generations. Dr Mead regards this group, which she calls the descent group, as pivotal in the regulation of marriage and of economic life. The problem of the prescribed marriages and Manus descent is extremely interesting, but too complicated to be discussed in a
review. I can only point out that in the "descent group" the woman in the third generation, of the same gens as the brother and sister in the first generation, has the choice of two spouses within the group, while the man of the same gens cannot find a spouse at all. He must get a wife from the descent group of which his father's mother or his mother's mother was a member.

Kinship behavior is described with reference to concrete examples, so that a comprehensive picture of Manus society is given.

In a work so well documented as this there are yet some points that require elucidation. We are told that the average age of death for males is from 35 to 40, so that a son is seldom old enough to succeed his father. This should be a factor of great sociological importance, and it becomes necessary to know how the figures were arrived at, and whether any causes for such early death of the males (women apparently live longer) can be suggested. Again, the economic system gives rise, we are told, to three classes: the financiers, their dependents, and a poor but independent class. Clear pictures of the first two classes are drawn, but no light is thrown on the third. Yet from the point of view of kinship this would be interesting, marriage and the ceremonial duties of kin being so largely influenced by financial considerations. The status of the female medium in the family is also left untouched.

We look forward to the publication by Dr Fortune of a book on Manus religion, which is referred to in footnotes,¹ and it is to be hoped that Dr Mead will also publish an account of the economic and ceremonial life of this most interesting people.

BRENDA Z. SELIGMAN

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PREHISTORY

New Light on the Most Ancient East. The Oriental Prelude to European Prehistory.

Within a few decades, archaeological investigations in Egypt, the Near East, India, and China have added several thousand years to the ancient history of the world: this fact itself is widely recognized although the main results of this work have remained generally unknown except to specialists. Professor Childe, a teacher of archaeology as well as an excavator, has appreciated the need of summarizing the data in a comprehensive manner; in 1928 he did so in "The Most Ancient East" which has served as the standard work on the subject for the last seven years. But new facts have been accumulating rapidly, and in "New Light on the Most Ancient East" the author has included them. The result is a new book, not a mere revision.

Childe's field is the enormous region extending from the Sahara in the west to the Punjab in the east, across what are today the relatively arid regions of Egypt,

Arabia, Mesopotamia, Iran, and Baluchistan; in time, he is concerned with all that preceded written history. The melting of the European ice-sheet at the close of the last Ice Age, caused extensive climatic changes farther south and man concentrated in the three great river valleys of this area, the Nile, the Tigris, and the Indus. In these there grew up the three oldest centers of civilization, Egypt, Sumer, and the Indus valley in the vicinity of Mohenjo-daro. All three were dependent on irrigation farming; in all three pottery was made, domesticated animals were present, and, within the period of the author's survey, the working of metals reached a high degree of excellence. These resemblances veil, but do not conceal, the vital differences in technique and style in the three areas, and they serve to emphasize such differences as between the theocratic governments of Egypt and the apparent bourgeois society of the Indus. Particularly in the western area from the Sahara to the Tigris—to which the author applies the term Afrasia—there were common cultural elements indicative of a common ancestry; but each region developed its own civilization, influenced however, by reciprocal trade and by foreign contact, as well as by the necessities of its own environment. It is obvious that no more than a beginning has been made in the archaeology of Sumer and the Indus, and almost nothing in such vital areas as Syria, Iran, and Baluchistan, to which the author devotes one chapter. Yet enough has been done to make clear in these three areas of antiquity the growth from simple peasant hunter-farmers at the edge of the marshland to the builders of cities and temples, with craftsmen in bronze and precious metals, and possessing strong systems of government. Moreover, the author describes conclusively the development between the fifth and the third millenia B.C. of elements of culture which form the basis of European civilization: the early history of Europe must be sought in North Africa and Asia.

To the American reader, some of Childe's conclusions will appear to be based on slight evidence. For example, the parallels between Mohenjo-daro and modern Indian cultures cited (pp. 220–22) as proof of the essential Indian character of the former seem less convincing to the reviewer than they do to the author. But in this, as in many other cases, it must be remembered that this volume is based on a study of dozens of sites and thousands of specimens; conclusions so reached have much greater force than the data, often summarized, given to justify them. Irrespective of the extent to which future work will modify such conclusions, all students of the early history of human culture will appreciate this well-written and comprehensive study which enables the non-specialist in Near Eastern archaeology to incorporate in his teaching the results of the extensive work in this difficult, but vitally important, field.

T. F. McIlwraith

University of Toronto

GENERAL


The author's general standpoint is that law in any society is a living, dynamic, and changing thing, and not a mere mechanism of procedure; and that it is related to all phases of the social life, or culture, of a people. The development of law, therefore, is a phase of the development of man's self-domestication. Hence law can be understood only in relation to all the functioning phases of the culture.

There is an account of religious, social, and political phases of law, as embodied, for example, in supernatural sanctions, such as oath and ordeal, and blood-bond, and of social forces which delimit or direct law, for example, clan, family, and larger-family organizations. A section is devoted to punishment. The treatment is topical, rather than ethnographic or by culture stages, and is essentially descriptive.

W. D. WALLIS

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In "La Mythologie Primitive" Lévy-Bruhl has chosen myths from various parts of Australia and Papua for the excellent reason that the material is abundant and satisfactory. The analysis is made with the avowed purpose of discovering the relation of mythology to primitive mentality. To this end the author discusses the questions of primitive emphasis on locality, space we might say, rather than time; the relationship of animal to human characters—the matter of half-human, half-animal characters in myth and as ancestors receives a great deal of attention; relationship between myth and the supernatural; and between the "real" and the supernatural. The matter of classification by primitives as compared with our so-called "scientific" classifications is discussed at length, and throughout the discussion there is considerable wonder at inconsistency.

The rites of initiation as well as of fertility are accepted as dramatization of the myths both in Australia and in Papua. From our experience in America where myths can occasionally be shown to have grown up with the ritual or to have been formulated after a ceremonial has been developed we must question this conclusion, as indeed we must doubt the general possibility of proving that one phase of culture predated another. The power of mythology is carried further as an explanation of prehistoric art manifestations, and includes some discussion of symbolism.

The reader will not be surprised to learn that the discussion and analysis of these and other subjects proves the theory of participation. It would be better I think to say further "illustrates" the theory for I feel that one who has collected myths from primitives, especially in the field, must admit that participation is a universal fact.

Up to this point I am in agreement with the author, if one may be in agreement with the obvious, but the next question is, does the statement of participation and its ample illustration prove that "primitives" have a mentality different from our
own? It proves of course that the "content" of the mind is different, but that is true of civilized people of the East and West. We could cite many examples from our own society in which Lévy-Bruhl's concept of participation is illustrated. A locomotive engineer may so thoroughly identify himself with his engine that his whole life is void if circumstances require that he give it up. He treats it like a person and even refers to it as "she." I have seen people pat an automobile as they would a horse or dog. The element of contradiction in its relation to participation is aptly illustrated in our most highly sophisticated and "logical" group by the scientist who demands physical proof for his scientific research and at the same time accepts the miracle of the Host. I submit that these reactions, this kind of "reasoning," compare exactly with those of a Papuan who personifies a yam, or in his rites, believes he is the bird of his totem.

An analysis of myth from a human rather than from a strictly philosophic point of view would, I believe, reveal a deeper glimpse of primitive mentality than we glean from Lévy-Bruhl. Such an analysis would yield a tremendous body of fact upon which the savage bases his conclusions. It would show an astounding knowledge of nature, not only of the behavior of animals but of their habits and even psychology. This knowledge, unknown to the white philosopher and unnecessary in his environment, is indispensible to the hunter to enable him to compete. An Australian can capture a kangaroo only if he "can beat it at its own game." A hunter who stalks his prey for three days on an empty stomach may come to the conclusion on the day of success, that he has been aided by the supernatural, rather than successful because of his own patience, endurance, and care. It is impossible to predict the results of such a situation if an individual of our own society had to meet it, but they may be readily imagined. There is no way of testing his "mentality" in the face of three days of stress, thirst, hunger, and for him, but not for the native, fear.

He who analyzes mythology from the point of view here recommended will find many surprises besides an uncanny understanding of animal psychology. He will find humor and pathos, bravado and devilment, sympathy and condemnation; in short, a deep comprehension of human values. In other words, he will treat literature in its relation to every phase of daily life, instead of taking out one activity and regarding it as if it were or could be a thing in itself. He is more than likely to get the clue to a totem cult in a secular tale, perhaps in one told for a joke, and for this reason he must include all the stories and not just those which deal with ritual. He will have his obvious generalizations to start with. They will indicate similarities, which have been frequently treated by the comparative method of which Lévy-Bruhl's is a modification. The analysis of myth from a human rather than from a philosophic point of view may reward the student with some reasons for differences, which after all need more explanation at the present time than similarities. The study of culture as well as of psychology has taught us that the two are so intimately interrelated that they can never be entirely separated. Lévy-Bruhl's treatise implies that they can.

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Man and His Ancestors. M. F. Nesterkh. (438 pp., illus. Rubles 5.25. Moscow: Union of State Publishing Houses, 1934. [In Russian].)

This book brilliantly sets forth the Marxian point of view on anthropogenesis. While Soviet scientists pay high honor to the Darwinian theory for having dealt a deathblow to teleology and to the metaphysical conceptions of the organic world which had previously prevailed, they still find fundamental errors and essential insufficiencies in it. Darwin, after all, could explain but a small part of the evolutionary process; he most uncritically applied social laws in the field of biology and biological laws in the field of society. He did not appreciate the fact that man is qualitatively as well as quantitatively different from the other animals. He said "Natura non fecit saltum," and belittled the gulf between man and the anthropoids, attributing man's evolution to the same factors as operated in the case of the other animals: to mutability, natural selection, heredity, struggle for existence, sexual selection, use or disuse of organs, etc.

But in fact [says our author] it is only through . . . both social and natural sciences that it is possible to understand how on the basis of the biological the social arose, how out of the animal the human arose. And this joint application of biological and social sciences is possible only on the basis of that all-embracing study of nature and society—dialectical materialism.

The more man's ancestor used his hands for work, the less he used them for locomotion. True, the "work" of that time was not yet true work. But this becoming-work reacted on the animal's brain, and the brain on it, and on becoming-society and becoming-society on it. Then it became truly work, a rational activity for the creation of usable values, the appropriation of the materials of nature for human use (Marx).

The general use of prepared implements is what distinguishes man from the anthropoid.

The mass transition to the use of tools must have borne the character of a leap. For we have here to deal with an anthropogenetic phase in which development sharply changes its direction and biological laws begin to stand in a subordinate position . . .

All subsequent evolution of the human body took its course under the flag of the dominating social laws.

Speech was also the product of work and a part of the humanization complex. When men began to work together, the need arose for more definite communications than were possible with the anthropoid's stock of animal sounds and grimaces. The first form of speech was gestures ("kinetic speech"), accompanied, perhaps, by grunts or cries having mainly an emotional content. But

"Man began to cross over to the phonetic speech, driven by the complicated demands of his economic life, which made more complex social groupings necessary. Lingual speech developed gradually, accompanying and supplementing the as yet not outgrown speech of gesture and mimicry."

Prof Nesturkh defends the position that the earlier types of man did not die out, nor were they exterminated: they represent various stages in a single evolution. The theory of polygenesis is untenable, as is also Montandon’s theory of ologenesis. Beside, both are being used to support current narrow nationalistic views in Europe.

The data on the behavior and mentality of the anthropoids is presented, down to the very last researches. The author makes this general comment:

A large number of scientists who study the behavior of anthropoids approach the question with a restricted bourgeois viewpoint, drawing comparisons and parallels with the behavior of man and indicating the similarities and differences without taking into account the fact that the behavior of modern man is class-socially conditioned. We have to assume a profound qualitative difference from the behavior of man in just this respect, and must evaluate the behavior of anthropoids, even when they approach nearest to that of man, as weak far-fetched hints of that remote past when our ancestors began to pass to the use of natural objects as “pre-tools” and lived in herds.

R. F. Barton

LENINGRAD, U.S.S.R.
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DISCUSSION AND CORRESPONDENCE

KINSHIP AND HISTORY

I hasten to admit the contention of Radcliffe-Brown, made in "Kinship Terminologies in California" in a recent number of this journal,1 that I claimed too much in stating that the time for an attack on the problem of the relation of kin terminologies to coexistent institutions was on the whole after some insight into their history had been attained rather than before. Actually, of course, the factual problem of what the correlation is, especially in the norm or average, is not intrinsically affected by historical considerations. Brown is quite within his rights in attempting to solve the problem as he sees it, which is unhistorically, without reference to complicating antecedents. If one seeks to isolate constants, simplification of the issue and its extrication from the chaos of apparent phenomena are important, and anterior stages may be irrelevant. What I should have expressed was my conviction that the factors at work in the phenomena in question are numerous and variable enough to make it seem highly questionable whether determinations of constants other than of narrow range or vague nature can be made, or at any rate have yet been made, while historical considerations are omitted. I cheerfully make this correction. That I was not trying to say that probabilities or inferences or hypotheses should be allowed to supersede facts, will probably be believed without explicit reaffirmation.

Among "historical considerations" in this connection I reckon language, of which kin terminologies are part. Hupa and Tolowa are Athabascan, and this fact therefore must enter ultimately into the problem of terminological-institutional correlation in northwest California. One can of course refrain from ultimates and limit the problem to the question of whether or not correlation exists in these particular tribes. This is perfectly legitimate, but seems rather narrow, and I do not believe Brown would wish to draw such a limit. In fact I admit without hesitation that there must expectably be some correlation: both because speech is not an independent universe, and because in other cases hitherto we have always found some accord between terminologies and institutions. A much more real problem is how much correlation there is, and what factors have made it stronger at some points and weaker at others. Here I believe language cannot be left out, in the sense that Hupa being Athabascan, and Yurok being if not Algonkin at least non-Athabascan, the speakers of the two languages must at one time have come into contact and into acquisition of a highly similar culture, not only with differently pronounced words, but—if all precedent holds—with kinship words of somewhat or considerably different meaning. The situation, in short, is characterized by the impingement on each other of a set of social institutions and usages at least highly uniform in the area and of several languages which are thoroughly different—so different that their contained terminologies still remain extremely diverse in plan or system of concepts. I do not doubt that detailed investigation will also reveal a number of differences of

1 Vol. 37, 1935, pp. 530-35.
social usage corresponding to differences in the terminologies, and that there will be value in knowing all such cases. I shall be happy to assist Brown, or anyone like-minded, in planning field investigation directed at these very matters. Nevertheless, however many fine points of this sort may have been overlooked by those of us who have studied the tribes in question, enough work has been done to make it clear that the great mass of the social system of the five tribes is similar, so similar as to be fairly designable as one in plan and pattern, whereas their kinship terminologies are of two types or patterns. This fact seems to me of more significance than the expectable one that there has also been a certain amount of adjustment between the impinging institutional and terminological patterns. It seems doubly significant in view of the Hupa and Tolowa being Athabascans, and the Wiyot and Yurok proto-Algonkins, if Sapir is right, and at any rate having a kin system of the type of the Salish and Wakash, irrespective of whether or not the Salish and Wakash are also proto-Algonkin. Perhaps I should say "of greater interest to myself" rather than "of greater significance," for significances change with interests.

The problem has by no means been exhausted even within the frame of the north-west California area. For instance, I have pointed out that each of the five terminologies shows certain assimilations to the others, contrary to, or on top of, its basic type or pattern. I have assumed that these assimilations were due to inter-influencings of the terminological systems, which are of course also systems of thought or unconscious semantic logic. It is however equally possible, theoretically, that the assimilations are due to the leveling influence of the more uniform institutions. Quite likely both factors have been at work; and it would be interesting to know to what degree and at what points and circumstances. Again, therefore, I renew my invitation to more fieldwork.

As regards the generic problem of term-usage correlation, I am ready to retract some of my intransigence of earlier years, which I now construe as a reaction to the once-prevalent abuse of seeing in kinship systems chiefly instruments for reconstructing systems of social structure; and I suggest the following basis for a permanent and productive peace.

The relation of kinship-term systems to institutions and practices seems analogous to the relation of dress to the human body. One expects normally a considerable degree of fit; but it would be dogmatic and futile to say that body conformation "determines" dress, or that dress "reflects" the body. Sometimes it does, sometimes it does not. The real problem obviously is when it does and when it does not, and how and why. Styles have a way of traveling their own course, sometimes to the point of requisitioning mechanical inventions to preserve even adhesion to the body. Fit may be loose and cool or snug and warm, comfortable or uncomfortable whether loose or tight. Similarly with the fit of kin terminology to social usage: it may be close or wide. Expectably there will always be some fit, and there may be a great deal, but it may also be remarkably partial. Every kinship system is also a little system of classificatory thought, and unconscious peoples sometimes are as ingenious in their logical productions as ethnologists in their analyses. There is no reason why such systems should not have a history of their own: not of course wholly cut
off and self-determined, but partially so, with their own novelties, growths, diffusions, and contact modifications. They are styles of logic in a limited field of universal occurrence.

But they are also more than this: they are parts of languages which always have a long history and ordinarily change slowly. They therefore tend to contain precipitates of greater or less age: old elements with changed function, also elements with unchanged function inconsistent in a new pattern. The Navaho system for instance is almost certainly more than a mirror of Navaho clan system, marriage avoidance, obligations, familiarities, etc., though it will undoubtedly fit these usages in part. It is also still an Athabascan system, presumably not only in its sounds but partially in its concepts. If these concepts have been made over completely to fit the institutions of the Navaho in the Southwest, or Pueblo ideology, it would be a surprising fact. Expectably the Navaho system is an adjustment between functioning Navaho social usages, no longer functioning ones, an ancient northern Athabascan system, and Pueblo ideology, with the two latter in turn the resultant of adjustments between practice systems and thought systems.

A normally large amount of play or give in fit is evident. Portions of a naming system can be indifferent from the point of view of social structure, or vice versa. Grandparents as compared with uncles, siblings with cousins, more often lie in these areas of indifference; but they are no less important, in the conceptual system, or in life, except on the premise that fit to social structure is the most important aspect of the subject. To me it is not: the ideologies as such possess at least equal interest and significance. More, in fact, as long as they continue to be under-weighted in pursuit of the social-fit theory. Can we not all meet on the common ground that the determinants are multiple and variable, and then amiably follow the ways of our respective bents as these most profitably lead us, with tolerance also of other approaches? It does seem a symptom of immaturity in Anthropology that we should still divide up into militant camps like eighteenth century Vulcanists and Neptunists. Perhaps I threw the first stone, but herewith I extend the olive branch.

On the broader question of the relation of sociology and history, Brown and I have expressed ourselves at greater length in articles in the same and following numbers of the American Anthropologist, and a few comments must suffice. In common with most historians I hold the essential and characteristic thing about history to be neither documentation nor time sequences, but an attitude of mind, a particular approach in trying to understand phenomena. The distinction between the "detailed and documented history of the historian" and "the hypothetical history of the ethnologist" is valid enough but hardly seems to go to the root of things. Neither does the distinction between the "evidence of eye-witnesses" as distinct from inferences "based on circumstantial evidence." The most documented history that limited itself to eye-witness testimony and refused to infer from circumstantial evidence would be only skin deep. Brown's double characterization sounds like a scientist's conception of history. As I have said before, all historians reconstruct. If they do not reconstruct, they are accomplishing nothing, because historical interpretation is reconstruction. And the values of all historiography lie precisely
in its being hypothetical, if a categorical paradox may be pardoned. Brown’s distinction of ethnology, which he sees as historical though apparently condemned to being an inferior kind of history, from social anthropology or comparative sociology, which investigates “the nature of human society,” I would accept, with reservation as to the inferiority if that was implied, and with reservation also as to the emphasis on society instead of culture. And I would accept his distinction as referring to two currents within Anthropology rather than to two disciplines. We agree that their aims, methods, interests, and I think values, are different. I take it that the investigation of the nature of human society, or culture, has for its end the determination of constants, in other words of abstractions extricated from phenomena as they occur in space, time, and variety of character. This is a genuinely and wholly scientific objective, evidently very difficult to attain from social or cultural material, but certainly important and significant. Any method which will really bring us there is a good method. Whether the better procedure is to dissociate as much as possible from the historic approach, as Brown seems to want, or on the other hand to envisage and emphasize the historicity of phenomena, as Lesser advocates, I do not know. Brown’s course seems the purer, more drastic, and to date more sterile. But it may in the end carry us farther into new concepts. Good speed on his journey.

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NOTES ON THE TUBATULABAL LANGUAGE

The language of the Tubatulabal of California, belonging to the Uto-Aztecan stock, has been ably presented by C. F. Voegelin in two recent monographs.1 It is an extraordinarily interesting language to any one interested in curious linguistic phenomena, with a remarkable word-structure that is not emphasized in the grammar and which I describe below. It has a process that the author calls initial reduplication but which I should like to call “exfixation,” in which the word is postfixed to its own first vowel (wic-: twic; tik: uik; ma: g-: amak). Sometimes the exfixed vowel disappears from within the stem when it appears in the exfix (no’ in-: phonetically no’ in-:; o’ in:; po’ han-: opha’ n). A nasal after the stem-vowel is also exfixed and assimilated (pa’ n: amban). The category of true past tense is developed in nouns (my house: what used to be my house) but not in verbs, which have a different, two-tense system: realized and future. In contrast to the usual verb technique of suffixation to the verb stem, certain modalities can be expressed by using a naked stem along with a detached auxiliary verb to which suffixes may be added. There is a stylistic distinction between the two techniques: the naked stem is “strong talk,” piling of suffixes on the stem is “high language.”

Voegelin has not yet worked out to its last details the complete phonemics of Tubatulabal but he has given a good general account of the phonemes.

One statement in his preface is a little misleading: that Tübatulabal differs strikingly from other Shoshonean languages in that its stems frequently end in consonants. The actual facts are that quite like many other Uto-Aztecan languages (Southern Paiute excepted) Tübatulabal employs a reduced form of stem with final vowel elided, alongside the full form with a final vowel. But in Tübatulabal the full form bears suffixes and hence its vowel-ending character is masked, especially since Voegelin treats this final stem-vowel as a "vowel-increment," that is, in the nature of a suffix. In so doing he is guided by his on the whole sound feeling for the actual patterning of Tübatulabal, in which suffixes prevailingly control or assimilate vowels before them that are not part of these suffixes in a historical or comparative sense. They do not always assimilate such vowels, whence there result irregularities that general Uto-Aztecan comparative theory accounts for. The author is to be commended for giving first place to the outstanding observables of his language, and for his compact and ingenious system of expressing morphological data through symbolic devices such as vowel-increments, but his formule must undergo certain transformations before they can be used comparatively, and his "consonant-ending stem" is such a formula. Stems that Voegelin admits as ending in a vowel are merely of a class that does not elide this vowel in the form that contrasts with the formal suffix-bearing full grade.

The beginnings and ends of Tübatulabal words are defined by position of stresses and by phonemic distribution, e.g., voiceless stops may be initial and final but not voiced stops, and an apparently organic voiced stop getting into such a position unvoices. A word may contain three structural parts, each having theoretically any number of syllables; viz. a stem, a string of "true" suffixes, and after that a string of "conjunctive particles," suffixes of a somewhat different kind. The stem and true suffixes form a unit characterized by a scheme of alternating stresses in which the conjunctive does not participate, also by unvoicing a stop at the end of the suffix position as though the word ended there. Moreover the conjunctive particle may be a pronoun that is not in any syntactic relation with the word to which it is postfixed, but belongs with some other word in the sentence. Thus the conjunctive particle could and should be considered another word were it not for these disquieting facts: (a) it may not occur alone but only postfixed to a suitable antecedent, (b) its initial may undergo peculiar phonetic interactions and fusions with the final of the antecedent, (c) apart from (b) its initial may break the rules for a word-initial, e.g., be a voiced stop.

The string of true suffixes is also set off from the rest of the word by a remarkable scheme of alternating vowel-lengths that is distinct from the alternating-stress scheme and in which neither the stem nor the conjunctives participate. This scheme is variously disturbed by particular suffixes that control their vowel-lengths in spite of it, but it manifests consistently otherwise.

Within these three frames of stem, suffixes, and conjunctives, embraced by the larger frame of the word, the distribution of related sounds according to position is not quite the same throughout, but depends on the particular frame of location. For instance, the relationships of voiced and unvoiced stops are quite peculiar in this
respect, and not the same for the various pairs. The pair $k$, $g$ will serve to illustrate. Word-initial we have only $k$. Within the stem, except for word-final, $g$ occurs after both long and short vowels, $k$ only after short vowels; the distribution of $g$ and $k$ after short vowels follows no apparent rule. In suffix-position also, except immediately after noun-stems, $g$ occurs after all vowels and $k$ (much the more common in suffixes) only after short vowels—but suffixal $k$ is unchangeable and controls its preceding vowel, making it short in spite of the length-alternation scheme. Suffixal $g$ is also unchangeable, but tolerates the length-alternation scheme. In the special case of nouns, which fall into three classes like the Latin declensions, a suffixal guttural is $g$ immediately after a class A or C stem, but $k$ immediately after a class B stem, and changes to agree with the stem, regardless of preceding vowels as far as can be told from Voegelin’s examples, which do not quite clear up this point. Voegelin calls one such suffix, -\textit{kan}: -\textit{gan}, a conjunctive, but it participates in the stress-alternation and accords with my specification of a suffix. In conjunctive-particle position only $g$ occurs, except as it reacts with a preceding voiceless consonant of the antecedent to give $k$. This is in striking contrast to suffix-position, where $k$ is the rule. Finally, in word-final position only $k$ occurs, whether it be in stem, suffix, or conjunctive, or after a long or short vowel. It is possible that analysis of all pertinent phenomena of this sort might resolve the $k$ and $g$ into one phoneme.

The glottal stop in Tübatulabal would seem not to be a true consonant on a level with the others, as in Hopi and Southern Paiute. In Tübatulabal, unlike Hopi, a syllable may begin with a vowel as well as with a consonant. The glottal stop is merely the way of actualizing this zero-consonantal beginning when no other consonant precedes a vowel. Thus where Voegelin writes $\text{i}^{\prime}\text{i}^{\prime}k$ and $\text{n}o^{\prime}\text{i}^{\prime}\text{in}$- it would be more phonemic to write $\text{i}^{\prime}\text{i}^{\prime}k$ and $\text{n}o^{\prime}\text{i}^{\prime}\text{in}$. It is evidently impossible in Tübatulabal to pronounce two adjacent vowels otherwise than with glottal stop between. This glottal stop does not represent any permanent structural element. Thus when the $o$ phoneme in $\text{n}o^{\prime}\text{i}^{\prime}\text{in}$- is "exfixed" and the element $\text{n}o^{\prime}$- becomes $\text{o}n^{\prime}$-, the next phoneme after the $n$ is not glottal stop but $i$, and we get $\text{oni}^{\prime}\text{n}$. The non-existent $\text{*on}^{\prime}i^{\prime}n$ shows a cluster that does not occur in Tübatulabal, for where Voegelin writes $n^{\prime}, m^{\prime}$ closing a syllable he means only a positional sub-type of the $n, m$ phonemes. The form $\text{oni}^{\prime}\text{n}$ is phonetically $\text{oni}^{\prime}\text{n}$ but when the exfix is re-infixed the first element becomes not $n^{\prime}o^{\prime}$- but $n^{\prime}$-, while the second element $\text{-in}$- must now be actualized as \text{\textquoteleft}\text{\textquoteleft in}-. It would be desirable to eliminate the symbol’ from the writing of Tübatulabal for phonemic reasons, and this would necessitate some device other than repetition of the vowel-letter to denote the "re-articulated vowel," which is merely the final fraction of a peculiar sub-type of long vowel.

Tübatulabal is divergent from the general Uto-Aztecan type in having (1) ex-fixation as a functional process, (2) no prefixing at all, (3) no compounding except in petrified survivals, (4) detached auxiliary verbs used with uninflectable naked stems, (5) no suppletive sets of singular versus plural verbs (also true of Nahuatl), (6) re-
patterning of stem-final vowels to predominant control by the suffix, (7) a scheme of vowel-length alternation in suffixes, (8) reduplication of entire stems, (9) supplementation of the first person singular pronoun *ni by *gi/*ki. Of these traits, (3) and (4) impress me as the most divergent and unlike typical Uto-Aztecans. On the other hand conjunctive particles and other queer features of structure that I have described above are not untypical of Uto-Aztecans; I stress them because of their interest to general linguistics. Exfixation is apparently an archaic Uto-Aztecans process leaving traces in various languages, while Tubatulabal alone has carried on (and perhaps elaborated) this ancient process. Also Tubatulabal preserves such archaic Uto-Aztecans features as the three-case system in nouns, the general absolutive noun-suffix *t, the nasalized stops and the nasalized s (Tubatulabal nds), initial l, and w before o.

In spite of the number of uncharacteristic traits I have listed—of which some are indeed archaic Uto-Aztecans—Tubatulabal cannot be called aberrant, not with the number of distinctive Uto-Aztecans features that it possesses. Among these are the case system, the absolutive-relative system in nouns, the nominative-case object of an imperative verb, the subordinating verb-forms (gerundials) of two kinds for identical and non-identical subjects, the objective-case subject of the non-identical gerundial (contrary to the conjecture in Sapir’s Southern Paiute that this form might be a genitive since Tubatulabal has a genitive distinct from objective), many particular suffixes (transitives, passives, instrumentives, etc.), the type of morphology with its relatively few afformatives as opposed to “polysynthesis,” the use as shown by Voegelin’s texts of sentence-introducing words and the order sentence-introducer, subject-pronoun, verb, and finally the thoroughly Uto-Aztecans vocabulary. Such features to my mind outweigh the aberrations. I consider Tubatulabal no more divergent than Ute-Paiute, less so than Piman. Still, Kroeber is perhaps justified in classing it as a separate division of the stock. To my mind it shows most resemblance to the as yet little known southern Californian languages. It is interesting that the divergent traits 2, 3, 5, 6, 9 above are rather characteristic of Penutian, with which Tubatulabal is in contact. Also Tubatulabal possesses in eminent degree for Uto-Aztecans the characteristic of modulating and shifting vowels according to fixed patterns (cf. traits 1, 6, 7) that is still more marked in Penutian, reaching its height in the Semitic-like Yokuts language. Since Yokuts is a neighbor of Tubatulabal some of these traits may be due to the little-understood process called language mixture, although not only Tubatulabal but reconstructed archaic Uto-Aztecans shows Penutian-like characters.

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ON BEING UNHISTORICAL

A synchronic grammar in linguistics has the same general connotation as a functional ethnography in anthropology. Even some of the motives for writing on a deliberately unhistorical plane are similar in the two fields. Thus, Dr Powdermaker says in the preface to her functional ethnography, “In this part of the world it is
difficult to apply an historical point of view, because there is so very little real historical information."¹ And in my "Tübatulabal Grammar"² I found it difficult to apply an historical point of view not because comparative data were lacking, but because I was lacking in an understanding of how to make use of these data.

More common than this enforced limitation is the rather exciting spectacle of ethnographers and linguists making a virtue of their lack of interest in historical developments. It has always seemed to me that the fountain-head of this attitude in ethnography, though not often acknowledged as such, is to be found in Radcliffe-Brown's presentation of researches carried out in the years 1906 to 1908.³ The situation in linguistic studies is rather different. One does not find first rate synchronic grammarians who never turn their attention to genetic relationships. However, as a healthy reaction against quoting parallel formations in Latin, Americanists have come to treat the genius of each language independently, sometimes showing little interest in available comparative data.

Now whether one chooses to ignore the insights which comparative data might yield, or is ignorant of the comparative data or the technique necessary to utilize these data does not materially alter the obvious fact that certain advantages are gained and other potential advantages lost in a study limited to one point of time. An outstanding advantage in the functional-synchronic approach to data is simplicity. Conversely, an analysis which takes cognizance of historical developments is more or less encumbered by considerations which tend to distract attention from the powerful simplicity of the central theme, from the clear-cut picture of movement arrested for a static moment; but what is more significant, the clear-cut picture may turn out to be not only less clear-cut but quite another picture when viewed in its historical perspective.

This point is nicely illustrated in Tübatulabal where a synchronic presentation and an interpretation having historical depth¹ yield different patterns of stem-suffix combination. Possible alternative interpretations are not altogether unexpectable

³ A. R. Brown, The Andaman Islanders (Cambridge, 1922), pp. 229–406. While the author feels that "hypothetical reconstructions of the past" are unscientific ("My own view is that such studies can never be of any great scientific value," p. 229, n. 1), he none the less demonstrates his ability to employ the technique of which he disapproves (see especially pp. 407–94).

⁴ For an understanding of this interpretation, I am indebted to the linguistic lectures and the studies in Uto-Aztecan by Edward Sapir, and to the brilliant Uto-Aztecan scholar, Benjamin Whorf, who has given me an insight into his latest researches and has in addition criticized my "Tübatulabal Grammar" from the comparative point of view. In this paper, I indicate the historical interpretation schematically, and do not duplicate Mr Whorf's specific criticism. It was, however, this astute criticism (see pp. 341–44 of this issue of the American Anthropologist) which finally convinced me that the advantages of an interpretation influenced by Uto-Aztecan presuppositions outweighs the advantages gained in the simpler synchronic presentation.
when certain phonological complexities are encountered. It is, for example, generally true that whenever fusion occurs between elements (as stem and suffix or between suffixes themselves), it is difficult to isolate the elements with certainty. This difficulty is partially resolved in Tübatulabal because fusion is almost exclusively centered in vowel increments which may be regarded metaphorically as synapses between the stem and the first suffix and between successive suffixes. There are two vowel increments, a and i. The former may be influenced by the preceding element to appear as any other vowel except i, but the latter is not subject to such progressive influence.

The difficulty of isolable elements is thus focused on the vowel increments. There remains to be made an interpretation which will argue either that each vowel increment belongs with each preceding element in the word, or that each vowel increment belongs with each following element in the word.

The synchronic presentation assumes that vowel increments belong with each following element in the word. Practically, this means that verbal suffixes begin in i or a (=not i); but it must also be recognized that these vowel increments have a certain semi-independence of their own because they may be reduplicated as such, because they may precede the zero nominal suffix, and because they are phonetically contrastive. Semantically, they form firm units with their associated suffixes which might be said to fall into two classes, those beginning in i and those beginning in a. Stems enter into this picture merely as a special type of preceding element, and word patterns are of the following type (with S for stem or suffix; V for i or a): S, S-VS, S-VS-VS, S-VS-VS-VS, and so on.

From the comparative Uto-Aztecan point of view, there can be little question but that vowel increments in Tübatulabal reflect the stem final vowel; accordingly, the vowel increment here belongs to the preceding element. This accounts for the vowel increment between the first suffix and the type of stem which ends in a consonant when used in final position, i.e., without a suffix. In order to account for the vowel increments between successive suffixes, an ancillary assumption must be made: every suffix ends in a vowel which is preserved only when followed by another suffix but lost when a suffix appears in final position. This ancillary assumption may be successfully tested for the so-called medial suffixes which appear either in medial position when they are followed by a vowel, or in final position when they end in a consonant. Accordingly, the vowel increments here also belong to the preceding elements.

The regularity of this formulation is disturbed by two factors. Certain stems and one or two medial suffixes end in a vowel even when in final position. Mr Whorf has some comparative data which suggest that in some instances at least the uncontracted final vowel in Tübatulabal is a reflex of a stressed final vowel in Uto-Aztecan. The second factor is operative in Uto-Aztecan no less than in Tübatulabal. Each stem is ambi-morphic in the sense that a given stem will end either in i or a (=not i), with contrastive semantic value in Uto-Aztecan and also in an attenuated way in Tübatulabal. A correlative assumption must be made to accommodate Tübatulabal at least: whenever a stem (or stem + suffixes) may add a suffix, one operates with an
extensible theme; and then in every step of its extension, a theme ends in i or a. Thus, word patterns will be of the following type (with S for stem or suffix; V for i or a): S(V), SV-S(V), SV-SV-S(V), SV-SV-SV-S(V), and so on.

This is not the place to demonstrate the advantages of the S(V), SV-S(V) pattern in Tübatulabal. It is sufficient for our purposes to have demonstrated that this pattern differs from the synchronic pattern, and that this pattern is less simple than the synchronic pattern. This situation suggests certain objections which should be discussed. The cogency of some of the discussion which follows will depend upon whether the reader is willing to grant, dialectically, that the S(V), SV-S(V) type (=an interpretation influenced by historical considerations) is in some ways superior to the S, S-VS type (=a functional-synchronic interpretation).

One possible objection to all this is that if a superior patterning is to be found in descriptive material, its discovery depends upon the ability of the student and not upon the utilization of comparative data. A correlative objection would be that if the student is truly sophisticated in his field, he will already be aware of comparable difficulties and thus anticipate subtle patterning from parallel instances in other parts of the world rather than from comparative data in the strict sense. Also, it might be argued that in some sort of participation ("learning to speak the language" or "living the life with the natives"), one arrives at certain feelings for the true inwardness of cultural forms which would only be falsified if influenced by historical considerations. And as a correlative to this objection, it might be said that if one has the opportunity of observing "living cultures," comparative data can add nothing to a picture which is already clear. These represent types of objections which might be extended and subdivided but which cannot be categorically denied.

I regard the relative abilities of different students as irrelevant in a consideration of methodology and take it for granted that intuitively imaginative students, given scant clues, can arrive at conclusions which generally presuppose heuristic data. Thus, in his study of Zapotekan dialects, Paul Radin found that k in one language corresponded to p in another language; he therefore reconstructed a *kw form and in a subsequent investigation of Mountain Zapotekan found that his hypothetical *kw was indeed a living reality. Comparable feats were accomplished in Athabaskan by Edward Sapir and in Algonkin by Leonard Bloomfield. Yet I am quite sure that none of these scholars would have deliberately neglected the missing-link forms, had they been available in the comparative data, for the sake of virtuosity in discovering them out of an arbitrarily limited group of languages.

Apropos of Dr Radin's reconstruction of Zapotekan *kw from p and k, a parallel development is found in Indo-European:

The other series, known as "labiovelars" and denoted here by qʷ (some use kʷ), gʷ, gʷʰ, is represented in the centum group by kʷ sounds (e.g., L. qu) or sounds resulting therefrom (e.g., Osc. p, G. π, r), but in the satem group by simple gutturals or in part by palatals derived

therefrom. The name labiovelar and the designation by qʷ, etc., are intended to indicate two distinct characteristics of these sounds, by which they differed from the palatals: (1) the term velar and the use of q that they were back gutturals, (2) the term labio- and the use of w that they were accompanied by rounding of the lips. It is only the first characteristic which is important for the satem group, in which the second characteristic plays no rôle (that is the w disappears), and the different development of the IE palatals and labiovelars depends wholly upon the difference in their guttural positions (front or back). Conversely for the centum group it is only the second characteristic which is important, and the difference between palatals and labiovelars resolves itself into one between simple k-sounds and kʷ-sounds.

From this we may infer that had Dr Radin been equipped with less linguistic feeling but had borne in mind this parallel information, he might have arrived at the same conclusion.

Praises have long been sung over encyclopedic knowledge, and it is not in a spirit of belittling the lyrical quality of this song that one kind of knowledge is here valued more highly than another. For our purposes, parallel knowledge is something of a luxury while comparative knowledge is indispensable. An example of parallel data in anthropology is the Shawnee leaching of acorns⁸ and the leaching of acorns in California. Leaching as practiced by the Shawnee is one of the processes in extracting oil from acorns, and the whole complex is said by informants to be borrowed from the Creek. Swanton says for the Southeast, "Walnuts, chestnuts, hickory nuts, chinquapins, and acorns were all dried, and from some of them, particularly the hickory nuts and acorns, they extracted oils which they used in preparing food and to anoint their bodies." But Dr Swanton does not specifically mention leaching and I do not know of any comparative data for Shawnee leaching. Neither can I see how a (parallel) knowledge of California leaching would contribute much to an understanding of the Shawnee complex, nor for that matter why any particular emphasis should be given to a description of Shawnee leaching. In California, the opposite condition holds true. Not only is acorn leaching widespread, but leaching is also applied to buckeye. And in general, techniques applied to one kind of food are extended to other kinds of food. Acorns are gathered like pine nuts, and so on.⁹ This interrelatedness in the gathering and preparation of foods leads Kroeber to say, "The human food production of aboriginal California will accordingly not be well understood until a really thorough study has been made of all the

⁷ E. W. Voegelin, unpublished Shawnee field notes, 1933-1934.
⁹ John R. Swanton, Aboriginal Culture of the Southeast (Forty-second Annual Report, Bureau of American Ethnology, 1928), p. 692. However, in this area acorns used as solid food were known to be leached. Dr Swanton cites a French manuscript of the eighteenth century which describes how leached acorn flour was made into a paste and cooked, but "at a more recent day" acorns cooked with hominy often caused cramps, which suggests that the acorns were not properly leached (Source Material for the Social and Ceremonial Life of the Choctaw Indians, Bulletin, Bureau of American Ethnology, 103, 1931, pp. 38, 48).
activities of this kind among at least one people." Such a pattern of interrelatedness, once it is described for a single California tribe, will form a strategic part of the comparative (not parallel) data in the study of other California tribes, for if some of the successive tribes do not follow the expectable pattern, the disturbing factors in basic subsistence will receive certain emphasis.

It goes without saying that comparative knowledge is not the only oblique focus which can give additional perspective to a flat description. In order to gain a real appreciation of Yokuts leaching, for example, it is necessary to eat Yokuts acorn mush. This point is of course generally recognized and often practiced when possible. If the Yokuts Jimson weed ceremony were fully alive today, some anthropologist would doubtless be "Jimson weeding," as Dr Kroeber translates the native term for the rite. In what degree the anthropologist's inner experience would correspond to the inner experience of a Yokuts Indian is a matter for psychological analysis. The whole question of direct or vicarious participation in another culture is really very important, and any simple approval or disapproval of the matter proves nothing, except perhaps ignorance of the complexities involved. The claims which are sometimes made for, e.g., a speaking knowledge of an exotic language, as though this were an open sesame to every baffling problem, are pathetically unrealistic. Yet it would be a pity if these early enthusiasms should detract serious attention from an approach in anthropology which is as yet insufficiently analyzed. I am not alone in suspecting that when direct participation in other cultural forms is properly investigated, matters of psychological interest will emerge, scarcely concerned with "culture" in the present connotation of the term. But it is idle to conjecture as to what part historical considerations will play in a picture yet to be designed.

What has been and is being accomplished in parts of the world where native life still flourishes is direct observation of this life as it is lived from day to day. Observers of the daily island life do not characteristically give consideration to comparative data (curiously enough, parallel instances are often cited instead). The chief implication made is that observation of a "living culture," as opposed to an informant's memory of a defunct culture, is necessarily accurate and indisputable. And this, I have tried to show, is not strictly true: a language when it is spoken at all is a "living culture," and if comparative data can influence the presentation of a spoken language, then the same principle should apply to any other department of "living culture." There is really no question of comparative data adding anything in the sense of a mass of naive footnotes acknowledging similar practises among neighbors, but rather of offering some assurance that the interpretation presented has a certain validity. Subtle patterns in language and culture are difficult to delineate with certainty. Perhaps more often than is consciously admitted, alternative interpretations present themselves. There must be a tendency in everyone to regard his first fresh impression as convincing, and to stifle subsequent doubts—and then compensate for the doubts by being overly confident in presentation.

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10 Kroeber, op. cit., p. 525.
An instructive instance of overconfidence in linguistic presentation is to be found in Geers' interesting contribution to the problem of stems and prefixes in Blackfoot. To begin with, Dr Geers announces that his intentions are synchronic:

... I hope a sound treatment of this matter in Blackfoot may not prove superfluous, may even deepen our insight into the structure of Algonkin. But I emphatically declare that I draw conclusions only as far as Blackfoot is concerned, and that I do not wish to follow the example of Jones* and Michelson** who both talk of "Algonquian" word-formations, etc., whereas they only deal with material from Fox, one of the dialects of the so-called Central Algonkin.\textsuperscript{13}

It should have been added, for the sake of fairness, that Dr Michelson is by no means lacking in comparative information but that by utilizing this information without specifically producing it in every instance of his discussion of Fox, he is able to achieve the remarkable succinctness which characterizes all of his work. Judging from internal evidence, Dr Geers is not influenced by comparative data. In explicit references to other Algonkian languages, he discusses, for controversial purposes, the types of stem classification suggested but apparently does not observe the actual data upon which these classifications rest.\textsuperscript{13} Virtually, then, his only objection to the Jones-Michelson stem classification is that it is exceedingly complicated. In its place he offers, as one would expect from a synchronic study, a simpler pattern to account for the Blackfoot verb, and he presents some Blackfoot data\textsuperscript{14} which neither prove nor disprove that his preferred interpretation is better than that of Jones and Michelson. Dr Geers' confidence rests upon the greater simplicity of his own interpretation. This advantage may be accepted as a decisive factor only when other factors in alternative interpretations are equally balanced. Whether or not this is true for Blackfoot cannot be determined from the material which Dr Geers offers us.

It is unreasonable to ask each student to be equally balanced in both training and interest between the synchronic-functional and historical perspectives. This ideal has at any rate not yet been actualized. The only danger in the present situation is a patriotic faith that the preferred perspective is the only perspective. Any methodology which regards more or less inimical schools as of coordinate value presupposes complementary (not complimentary) criticism. For one school to regard the efforts of another as unscientific is, to put it bluntly, childish. To do this is to leave unrealized the great service which can be rendered by constructive criticism.

\textsuperscript{11} G. J. Geers, The Adverbial and Prepositional Prefixes in Blackfoot (Leiden, 1917).
\textsuperscript{12} Geers, \textit{op. cit.}, p. 2. The asterisks in this quotation refer to \textsuperscript{13} W. Jones, Some Principles of Algonquian Word-Formation. American Anthropologist. N(ew) S(eries) VI (1906), p. 369 sqq. \textsuperscript{14} W. Jones and T. Michelson, Algonquian (Fox) in Handbook of American Indian Languages I."
\textsuperscript{12} Geers, \textit{op. cit.}, pp. 5–12.
\textsuperscript{14} Geers, \textit{op. cit.}, pp. 19–130.
THE SIGNIFICANCE OF THORSTEIN VEBLEN FOR ANTHROPOLOGY

The recent appearance of a biography of Thorstein Veblen1 recalls a figure whose significance for anthropology has been greatly overlooked. Though technically an economist, he drew on the literature of many fields to make the keen analyses of the socio-economic structure of our own culture, and to formulate the stimulating hypotheses concerning the motivating drives behind economic phenomena, that are associated with his name.

Veblen, even in his earliest writings, manifested economic heresy of the most extreme sort. Before 1900, he was stating that

... "the economic man" of the classical economists is an anomaly in the animal world, for no species which has "a consistent aversion to whatever activity maintains" it, can survive in the struggle with other species.

In line with the emphasis he placed on processes of development, he laid down the proposition that, in early primitive societies of small peaceable groups, the appearance of industrial incapacity is to be avoided and thus there is emulation in industrial efficiency;

while,

with the advance of tools, including weapons, there arises a distinction between "honorific employments" involving exploit, and "humiliating employments" involving the peaceful industrial aptitudes; ... in a word, in the more complex cultures there is the development of class and caste, where the member of the honored group "severely leaves all uneventful drudgery to the women and minors. ... He puts in his time in the manly arts of war and devotes his talents to devising ways and means of disturbing the peace. That way lies honor."

Let us but substitute the phrase "societies of relatively simple technological achievement" for "early primitive groups," and "more complex economic societies" wherever a time-sequence of an evolutionary nature is postulated in what has just been cited, and how valid these principles come to be! For whether it be head-hunting in Fiji, or the counting of coups on the Plains of North America, or the capture of prisoners in Dahomey—"That way lies honor."

Veblen's approach to his data was thus almost ethnological. "Social laws," as such, held little interest for him. Always it was process, and the psychological drives that made for the economic institutions found in human societies, that claimed his attention. Thus it came about that he formulated those phrases for which he is best known, and which have come to be commonplace in current economic writing—"conspicuous waste," "invidious consumption," "pecuniary emulation"—phrases which, taken with their connotations, throw light on the motivations which underlie so many of the apparently illogical and non-advantageous aspects of economic life, not only in our own culture but in primitive ones as well.

That he had this approach, rather than the "legalistic" one of the classical economists, is perhaps due in some measure to Veblen's knowledge of and interest

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1 Joseph Dorfman, Thorstein Veblen and His America (New York: Viking Press, 1934).
in studies of contemporary primitive folk. In line with this interest, he encouraged "most of his able students" to read in the literature on primitive peoples:

As for the anthropological reading which I have inveigled you into [he wrote one of them] I do not know that it will be of much direct use, but it should be of some use in the sense of an acquaintance with mankind. Not that man as viewed by the anthropologists is any more —perhaps he is less—human than man as we see him in everyday life and in commercial life; but the anthropological survey should give a view of man in perspective and more in the general than is ordinarily attained by the classical economists, and should give added breadth and sobriety to the concept of "the economic man."

Other aspects of his anthropological interests show in his writings and his research plans. He was from his earliest years fascinated by the history of the Norwegian people, from whom he drew his ancestry, and this finally took its form in his translation of the "Laxdaela Saga," the introductory notes to which constitute an ethnographic document of no mean order. To a similar cause can be traced his concern with the Nordic theory, with which, however, he had no sympathy. Two papers, "The Mutation Theory and the Blond Race" (1913, republished in his "Essays in Our Changing Order"), and "The Blond Race and the Aryan Culture" (1913, republished in his "Place of Science in Modern Civilisation and Other Essays") constitute his contributions along this line, though attention is paid, in passing, in numerous others of his books and papers, to the rôle of the "dolicho- blond." Another anthropological tentative was his request to the Carnegie Institution and to the Archaeological Institute of America, for a grant to enable him to make an investigation into the early cultures of the Baltic and of Crete.

The problem on which my interest in prehistoric matters finally converges [he wrote] is that of the derivation and early growth of those free or popular institutions which have marked off European civilisation at its best from the great civilisations of Africa and Asia. A study of other primitive cultures, and not visibly related to this early European civilisation, shows a close correlation between the material (industrial and pecuniary) life of any given people and their civic, domestic and religious scheme of life, the myths and the religious cult reflect the character of these other—especially the economic and domestic—establishments in a particularly naive and truthful manner.

The chief reason why Veblen has not been more widely read is his style, which is so marked with tortuous clauses and involuted phrases that one sometimes wonders whether Veblen did not do all in his power to hide his ideas from those who might utilize them. Furthermore, and more specifically, the label of "economist" which is attached to him is not calculated to attract anthropological readers. The almost metaphysical "laws" which mark most discussions of economic theory are received with particular suspicion by those who profess a discipline which has demonstrated the lack of validity of so many kinds of "social laws," and has accepted the dictum that the application to historic phenomena of the concept of law utilized in the physical sciences is well-nigh hopeless. Even anthropologists who are not in entire agreement with such a position, however, find little of usefulness in most works on theoretical economics, and even less in the discussions of practical
problems by economists. Seeking principles that are applicable to primitive cultures, one is confronted by a terminology which not only is baffling in itself, but, more seriously, is practically useless in aiding the study of the economics of any culture other than our own, with its machine technology and pecuniary expressions of value. It is small wonder, therefore, that anthropologists have beaten a hasty re-treat from economic theory as ordinarily conceived into the more comfortable field of material culture, which, after all, is the basis of economic life; and it is thus understandable why the study of primitive economics has developed no farther than it has, and why in most ethnographic treatises one finds a discussion of technology when one turns to the section headed "Economic Life."

Veblen, like most economists, was primarily interested in his own culture. As is ordinarily the case when those who have been trained in one discipline attempt to utilize materials and hypotheses from another, his employment of anthropological data was as unsatisfactory as was his use of psychological material. Yet though his presuppositions might be untenable, his conclusions were brilliantly valid. Though he may have postulated an "instinct of workmanship" that is not sanctioned by the findings of psychologists, his conclusions based on the assumption—namely, the existence of a drive in human beings to do their work as well as possible, whatever the cost; the resulting development of technological processes; and the significance of the technology of any culture as the factor underlying its economic organizations—are impeccable, and have deeply influenced contemporary economics. Similarly, though certain of his ethnological assumptions are not acceptable to modern anthropology, yet his concept of the manner in which a leisure class develops, and of the rôle played by conspicuous consumption of economically valuable goods in establishing the vested interests and maintaining the prestige of the priestly and ruling classes, cannot be challenged. One may well wonder whether or not Veblen, particularly in his book "The Theory of the Leisure Class," has not, in this proposition, put his finger on a process that invariably operates in those societies where the technology is developed enough to allow of the production of an economic surplus that can be translated into social leisure, and thus permit the presence of socio-economic classes.

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NOTES AND NEWS

CONGRÈS INTERNATIONAL DES SCIENCES PRÉHISTORIQUES ET PROTOHISTORIQUES

The second session of the congress will be held in early August, 1936, at Oslo. Applications for membership should be sent to the Bureau du Congrès, Universitets Oldsaksamling, Oslo, not later than May 1, 1936: subscription is 25 Norwegian crowns.

CONCHOLOGICAL RESEARCH

Dr H. J. Boekelman, Department of Middle American Research, Tulane University, New Orleans, invites the submission for identification of shells secured in the course of archaeological and ethnological research. Worked and unworked shells are equally suitable for the purpose.

REPRODUCTIONS OF MEXICAN CODICES

Photographic reproductions of a series of codices in inexpensive form are available from Sr Salvador Mateos, Museo Nacional, Mexico, D. F.:

Codex Borbonicus, 36 pp., original size, in preparation.
Same, 36 pp., 8×8 inches, $8.00.
Same, 36 pp., 9.2×9.2 cm., $3.00.
Tira del Museo (Codex Boturini), one long sheet, $1.00.
Codex Tlotzin, one long sheet, $1.00.
Codex Quinatzin, one long sheet, $1.00.
Codex Telleriano Remensis, 100 pp., in preparation.
Codex Borgia, in preparation.
Codex Fejervary-Mayer, 44 pp., in preparation.
Codex Vaticanus B, 96 pp., 9.5×11.2 cm., $8.00.
Codex Cospi, 24 pp., 8×8 inches, $5.50.
Same, 24 pp., 9.5×9.5 cm., $2.25.
Codex Siguenza, one large sheet, 8×10 inches, $.50.
Codex Cortesiano, 42 pp., $3.00.
Collections of ten cards of gods in the Codex Borbonicus, in colors, $.35.
Collection of nine divinities from the Codex Borgia, arranged on a folding screen, $.35.

CHANGES IN INSTITUTIONS ABROAD

A National Bureau of Archaeology has been created under the Department of the Interior, Johannesburg, South Africa. The Director is Professor C. van Riet Lowe.

In Peru there has been created under the Ministerio de Fomento a Sección de Asuntos Indígenas for the investigation of life in the Indian communities. The Government has given charge of the protection and investigation of antiquities to a committee named the Patronato Nacional de Arqueología.
On July 1, 1935, the Ethnographical Department of the Naturhistoriska Riksamuseet, Stockholm, was given separate corporate identity as the Statens Etnografiska Museum, still remaining subordinate to the Royal Swedish Academy of Sciences.

It is reported that Professor Georg Thilenius, Director of the Hamburg Museum für Völkerkunde and Professor of Anthropology in the Universitäts Hamburg has been retired, his successor being Professor Termer, a geographer.
FUEGIAN SONGS

OF Fuegian native music scarcely anything was known until recently, except a few poor notes scattered in ethnological literature, and half a dozen musical examples recorded by ear and hence of doubtful reliability. Colonel C. Wellington Furlong, on his visit to Tierra del Fuego in 1907–1908, was the first to take phonographic records of Ona and Yahgan songs, copies of which were sent to the Berlin Archives and studied by the present writer. The publication of this study, however, has been delayed first from outward reasons, then deliberately, the material available having been increased by a great number of records taken among Ona, Yahgan, and Alakaluf by Professors M. Gusinde and W. Koppers in 1922–1923. Thus the conclusions already arrived at could be founded on a much broader basis. Considering the special interest of the Fuegians as preserving one of the most primitive culture types and, moreover, the frightful speed of their disappearance, the value of the phonographic documents collected in the eleventh hour cannot easily be overrated. In what follows I must content myself with outlining the main facts and conclusions concerning anthropology, and for a fuller discussion may refer to my contribution to Gus-
inde’s comprehensive work on the Fuegians. Colonel Furlong’s singers were: among the Ona, two women, Ichjh (Phonograph Record 12) and the “doctor” Yoyo (5), both in Rio Fuego, and two men, Ishtone in Najmish (Via Monte) (3) and Tininisk, a medicine man in Haberton (9, 11); among the Yahgan, in Lauwi (Punta Remolino) the women Simoorwhilliss (Alice) and Weemanahakeepca (Gertie) (1), and the men Danushtana (Alfredo), Nyempenan (David) (13, 6), with Aselensinjiz (Charley), Wuroyinjex (William), and Calderon (13), or Chris (6) joining in; and, in Rio Douglas, Navarin Island, Edward (? 2, 7).

In the music printed here a few additional signs have been used: + sharpens, − flattens the note up to a quartertone, constant deviations throughout a song being indicated in the beginning. A heavy line between

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6 M. Gusinde, Die Feuerland-Indianer (Mödling bei Wien, in press), Vol. 2.
7 Gertie, Calderon, and Chris (Crees) were also among Professor Gusinde’s singers.
8 Whenever possible the actual intervals have been ascertained tonometrically. There is, however, no need to give the results here as they are of no consequence to anthropology.
two noteheads indicates _glissando_. Headless notes and notes in parentheses are indistinct, parts in square brackets are variants, ∞ shortens the note; bar-lines divide into groups, not into bars; 9 V marks a breathing space.

The women’s songs were apparently distinguished neither as a particular category, nor musically, from the men’s songs. Duties and rights, with

the Fuegians, are almost equally distributed between the sexes. Thus, female shamans, as with some Siberian peoples, are not uncommon, and of course they will use the same kind of chants, and for the same magic purposes, as the medicine men (P. R. 5, 9a, 11a). 10 The Yahgan initiation cer-

9 The time as measured with the metronome on Professor Gusinde’s records averaged 98 for the Selk’nam, 90 for the Yamana.

10 Particularly those for healing the sick (Musical Examples 39–41, Selk’nam, in Gusinde _op. cit._, Vol. 2); increasing fish (Yamana, M. E. 38); other categories are probably reserved
emony is gone through by both boys and girls; consequently both know all the songs sung at this occasion mostly to accompany pantomimic (animal) dances (P. R. 1, 13, 6).11 Like the Eskimo, the Ona medicine men sing invective songs in order to deride each other (P. R. 9, 11b). Calls and answers voiced with a musical tone, i.e., rather sung than spoken, are used as signals by the Yahgan (P. R. 7c). The same custom has been reported from the primitive Kubu in the forests of Sumatra.12 Somewhat similar in character to a call is the fl-phrase preceding and following the song proper in Musical Example 14. Again, such a framing, which very probably has (or originally had) a magical meaning, frequently occurs in native songs: cf., e.g., the stereotyped formulae of the Kubu (beginning and end), Uitoto Indians in Colombia (end flourish),13 and Pangwe in the Cameroons (introduction).14 Realistic imitations of birds' cries—uttered by the bystanders, not the singers, during Yahgan pantomimic dance songs—bear no relation whatever, not even rhythmically, to the music, but testify to the mimic talent of these people.18 (Cf. the coyote bark frequently added to the end of a song by North American Indians, e. g., the Pawnee.)

All Fuegian songs—as has likewise been ascertained for the Tehuelche16—lack (meaningful) words. The meaningless syllables, however, being unchangeably associated and handed down with the tunes, must be considered as survivals of original words which for one reason or another ceased to be understood, and then were liable to distortion. Such a process is evidenced by many examples from primitive as well as from civilized peoples.17

for men, e.g., war songs (Selk’nam, M. E. 38), weather charms (?). Certain magic songs are the personal property of an individual doctor (Yamana, M. E. 19, 20), as is frequently the case among North American Indians.

11 Cf. Gusinde, op. cit., Vol. 2, M. E. 1–10 (Yamana), 28–31, 33–37 (Halakwalup). Other songs sung during the initiation ceremony: M. E. 12–16 (Yamana), 43–44 (Selk’nam). In the Ona Kloeiten it is the women’s duty to sing certain songs (Gusinde, op. cit., Vol. 1, p. 1038 and M. E. 43–44). The Yamana women also take an active part in the death chants (Gusinde, op. cit., Vol. 2, M. E. 21, 22), and, of course, in the singing of pasanna, secular songs for “entertainment” (M. E. 23–26). Possibly the songs the Halakwalup women sing when going for water (M. E. 32) are specific to that occasion.


15 W. Koppers, Unter Feuerland-Indianern (Stuttgart, 1924), p. 66, and phonographic records.


17 It generally happens when magic chants or spells or ceremonial texts are introduced
The songs—as in fact, the whole culture—of the Fuegians can be said to be characteristically American Indian and at the same time extremely primitive; the first statement applying more particularly to the Ona, the second to the Yahgan and Alakaluf.

The most conspicuous factor contributing to the character of Indian music is the manner of singing. It will be readily apprehended when hearing Indian singers (or records); but it is almost impossible to analyze the immediate impression and to convey to someone else a clear notion by enumerating its "elements." Bearing this in mind, we may describe Indian singing by such epithets as emphatic, pathetic, impressive, grave, solemn, dignified, weighty, stern, etc., (which would also apply equally to the Indian's dancing, general motor behavior, and temper). Among the features responsible for this impression will be found strong accents, often further increased by audible expiration, on almost every crochet; a tendency to connect the notes by a portato and to subdivide lengthened notes by pulsations; the time being moderate or rather slow and remaining constant throughout the song. Thus, the characteristic Indian movement is never sprawling or jumping (as with African Negroes), nor gliding or softly waving to and fro (as frequently in Oceania), but rather striding in a heavy regular pace. Perhaps the most important knowledge to be derived from Fuegian songs is the fact that the "emphatic" singing manner, though as strongly conspicuous among the Ona as among any other Indians, is entirely absent among the canoe-faring tribes, the Yahgan and Alakaluf.

Another basic feature hardly to be separated from the one already mentioned, is the Indian's rhythmic feeling, which is strongly contrasting with our own. Our normal rhythmic groups are iambics, rising from a light (unaccented, short) beat to the heaviest accent of the group (which may be lengthened). In our usual notation the two beats are separated by the bar-line and the notes preceding the bar-lines are called "upbeat" (arsis). Indian rhythm, on the contrary, may be described as lacking an upbeat. The typical group is a "dynamic trochee." It begins with the strongest beat—which is generally shortened and followed by a breathing space, its strength thus being further increased—and then drops off through a

from a foreign people whose language is not understood. Cf., e.g., for Australia, A. W. Howitt, The Native Tribes of South-East Australia (London, 1904), p. 414 (songs); for Torres Straits, Report of the Cambridge Anthropological Expedition to Torres Straits (Cambridge, 1901-1935), Vol. 4, Chapter 12 (ceremonial songs); on Shivaistic ritual texts in Bali, Dr Poerbadjaraka in T. de Kleen and P. de Kat Angelino, Mudras auf Bali (Hagen in Westphalen, 1923); magic chants in Hellenistic papyri, e.g., A. Dieterich, Abraxas (Leipzig, 1891); and the old formulæ surviving in our children's game songs and counting rhymes.
series of, as it were, rebounding accents. Though the length of this series is variable, even in one and the same song (cf., e.g., M. E. 6, 13), the second pair of a group is, as a rule, longer than the first: triplets are commonly of the 1+2 type (M. E. 1, 8, 10), and analogously four beats are grouped into 1+3 (M. E. 6), five into 1+4 (M. E. 11, beginning), etc. The same principle governs the subdivisions and, naturally, the structure of the whole song. In M. E. 9 the Western hearer will feel the phrases as consisting of three $\frac{6}{8}$ bars, i.e., groups of 3+3; whereas they are almost certainly meant as 1+2 groups of 2+(1+3) quavers each, the three pulsations on the long note again being a 1+2 grouplet.

The characteristic type of rhythm here described prevails in Yahgan and Alakaluf as well as in Ona music and, in fact, in any Indian music from North or South America. Deep rooted in general motor behavior as it is, it reappears in the most common Indian dance movement: one foot, after a wide step aside, is stamped on the ground; then the other foot is drawn after and stamped down, though less heavily, near the first one; a short rest may be filled with treading on the spot, then the movement is repeated.

A characteristic of Fuegan music is its narrowness; this appears both in tonal range and length of melodic units. The backbone of the melodic structure is a pair of notes, limiting a distance which rarely exceeds a whole tone (M. E. 1, 3b) and in many cases is confined to a semitone or less (M. E. 8, 4, 14), or is merely the result of fluctuations of a single tone, following the fluctuations in dynamic stress.18

Sometimes the whole song is limited to the principal "second" (M. E. 1). A third tone, however, is frequently added when the higher principal tone is sharpened by an accent (M. E. 3a'; 4, 8, 11, 12 in the beginning), or when the lower principal tone is further dropped on a weak beat (M. E. 10b, 12, 7c). Moreover, a two-tone motive may be shifted downwards, its lower note becoming the higher one in the repetition (M. E. 4, 11, 14). The range thus being enlarged, consonant intervals are aimed at as a frame to the melody, viz., a fourth (M. E. 10, 11, 12, 13; 3, 5, 6, 2), or, with the Ona, a fifth (M. E. 7, 8) or even an octave (M. E. 9). In Ona songs the fourth and fifth also occur as melodic progressions (M. E. 2, 6, 7, 9), and in one instance (M. E. 8) the motive is transposed downward by a fifth.19

By such means Ona melody escapes the primitive narrowness and

19 In chorus singing, octaves (M. E. 14) or fifths (Gusinde, op. cit., Vol. 2, M. E. 13, 14) are used unconsciously instead of unison. This fact proves that consonance also acts, if physiologically, on the Fuegians.
approaches the Indian type. Usually an Indian melody starts on the highest note with utmost strength, descends by shifting the motive from step to step—hence this type has been styled "stair-pattern melody"—at the same time decreasing in loudness, and finally dies away on the lowest note, which in some cases is more than two octaves below the initial pitch; the second verse repeats the first one, starting from the next to the highest level, and so forth, the last verse being confined to pulsations on the lowest note. Approximations to this form may be found in the Ona songs (M. E. 7, 8, 9).

There are several features in Fuegian songs, on the other hand, which offer exceptions to the exclusively downward trend of Indian melody. Frequently, and with the Yahgan almost as a rule, motives or whole melodies end with a rise to the next to the lowest note (M. E. 11; 12; 13; 14, v, vi; 15a, b; 1b; 4b; 9a, c). A similar rise sometimes occurs in the beginning (M. E. 2; 6; 15d). Even the level, in strict opposition to Indian style, may be gradually shifted upwards from verse to verse.\(^\text{20}\)

To summarize: Indian musical style is easily to be recognized by (1) its "dynamic trochee" rhythm which, contrary to our Western tendencies, avoids the upbeat; (2) a peculiar "emphatic" manner of singing which results from such factors as a certain voice-quality, strong accents on every time-unit, pulsation, slow and constant time; (3) a downward step-by-step shift of the main theme (stair-pattern) and a collapsing tendency of the melody, which from start to end continually decreases in pitch, intensity, and tonal range. This style prevails among the Indians of both Americas, including the Eskimo (also in Greenland), and among Siberian tribes who are related to the Indians, both somatically and culturally, as, e.g., the "Palaeo-asiatic" Chukchee and the Keto (Ostyak) on the Jenissei River, and among the semi-Tungus Orotchee on the lower Amur River, and in Korean folk-songs.

On the other hand, there are exceptions to the Indian style even in America. The Yahgan and Alakaluf, though conforming to the Indians in their rhythmic and general motor behavior, differ from them in their manner of singing which entirely lacks the Indian characteristics, and further in their melodies by several "primitive" features, viz., extreme narrowness, simplicity, and shortness of themes, a tendency to rise (or return) to an upper level contrary to the general downward trend. The same archaic traits, including non-emphatic singing manner, have been found in some other places in America, though they are buried under more recent layers

of Indian culture and hence less frequent and less clearly pronounced. As the Ona in some of their songs approach the style of the canoe-faring tribes in narrowness and melodic rise—but not in singing manner—so do, if to a still lesser degree, their continental relatives, the Tehuelche. As with the Fuegians, so equally with the Tehuelche the words sung have lost their meaning. Again, songs of the primitive “Fuegian” type have been reported from the Uitoto in the virgin forest on the Colombian-Peruvian frontier. (The old stratum to which this musical style belongs may be detected beneath still other South American cultures, e.g., that of the Botocudo and other tribes of the Geez stock, from whom unfortunately phonographic records are not yet available.)

In North America the primitive musical style survives in songs from Southern California Indians and tribes of the Yuma group closely related to them. Their voices and singing manner lack the characteristic “Indian” quality; the “rise” in melody is even more apparent than with the Fuegians and has developed into various forms; though the tonal range is usually not very small, traces of original narrowness may be found in the fact that the two prominent notes are only a second apart from each other, and of about equal importance but for the higher note being occasionally stressed as a final goal. In many of the songs the meaning of the words is obsolete. The relation between Californian and Fuegian cultures is further confirmed by parallels in the realm of religious, social, and economic life.

Considering, on the other hand, the uniformity of the Indian musical style extending over the whole area from Magellan Straits to the Arctic sea and from the east shore of Greenland to the Jenissei; and on the other hand, the strong contrast to this style shown by songs of the Yahgan and Alakaluf, we may feel inclined to distinguish the latter tribes culturally, if not somatically, as belonging to a separate pre-Indian group. These

23 F. Bose, loc. cit. One Uitoto song (No. 36) is almost identical with the “personal” song of a Yahgan medicine man (Gusinde, op. cit., Vol. 2, M. E. 19).
26 Physically the Yahgan are clearly different from the Ona. There is no space here to
people then would have been literally the forerunners of the real Indians' immigration into the American continent. Here they were not only driven to the remotest borders, or places difficult of access, but in the course of time naturally have been subject to influence from their less primitive neighbors. Thus the Yahgan very probably have borrowed the mask plays, that are kept secret from women, and the medicine men's organization and ceremonial meetings from the Ona, who in turn apparently are not the originators but merely mediators of these customs, at least of the first.

Outside America, the primitive pre-Indian musical style is closely paralleled by that of the Vedda\textsuperscript{27} and the Andaman Islanders.\textsuperscript{28} Their melodies are extremely narrow, frequently rising at the end of the lines; the number of time-units varies arbitrarily (or according to the number of syllables) in almost every repetition of one single phrase, though the time remains constant. (The Andaman Islanders also sing in octaves and fitths instead of in unison, as sometimes do the Yahgan.)

The well-known fact that in really primitive cultures sound instruments are almost completely absent, though being of no great importance in itself, enhances the purport of those rare parallels which may be found in this realm. In their death dance, as witnessed by Colonel Furlong,\textsuperscript{29} the Yamana women were tamping the ground with pairs of long thick posts (which may have been substituted for oars). Sachs,\textsuperscript{30} quoting Playfair, has drawn attention to the similar custom of the Garo in Assam, where the women, when watching the dead, accompany their songs by beating or tamping on the floor with a piece of wood. Likewise among the Kurnai, one of the most primitive southeastern Australian tribes, in a ceremony preliminary to the initiation, the mothers of the novices keep time by tamping their yam-sticks on the ground.\textsuperscript{31} (Note that in every instance it is the

discuss the problem whether the manner of singing is a hereditary physical character, as the voice quality and the general motor behavior undoubtedly are, or a mere traditional custom. It has been suggested that the "Indian emphasis" was a device of the shamans to render their incantations more impressive; but then it would be difficult to account for the fact that the Yahgan should have borrowed the medicine men's practice from the Ona with the exception of the emphatic singing manner.

\textsuperscript{27} C. S. Myers, Chapter 13 in C. G. and B. Z. Seligman, The Veddas (Cambridge, 1911); M. Wertheimer (Sammelbände der Internationalen Musik-Gesellschaft, Vol. 11, 1909, p. 300).


\textsuperscript{31} A. W. Howitt, The Native Tribes of South-East Australia, p. 620.
women who use the tamping sticks.) In their mask plays the Ona and (hence) the Yahgan make frightful noises by beating the ground with rolled-up hides in order to stage the raging of a wrathful spirit of the earth. Again the same instrument is used by southeast Australian women, though not as a beater, but rather as a drum. The Ona custom is further paralleled by southeast Australian men, when (in certain dances) they beat small mounds of earth or simply the ground with strips of bark. Still more striking a coincidence is the belief, common to both southeast Australians and Fuegians, that the medicine men learn songs and spells in dreams from one of their defunct relatives or from other spirits. (With both peoples in olden times the medicine men, or rather the spirits “talking in very curious voices” through the mouth of the shaman, used to inform their tribesmen of distant events, e.g., of a whale stranded on the shore.) As with the Yahgan, so likewise with the southeast Australians, the initiation ceremonies include pantomimes and dances in which the behavior and cries of (totem) birds and animals are realistically imitated. Unfortunately no phonographic records were made of southeast Australian (and Tasmanian) songs before the curtain dropped forever on the history of these natives. Yet, from the competent remarks and notations of Wurunjerri songs contributed to A. W. Howitt’s work by Dr Torrance, we may at least feel sure that southeast Australian melody was of a primitive narrow type. Every verse begins with a downward glide through a whole tone (or less) and continues, with strong accents on every beat, throughout on the lower note thus reached, until, at the beginning of the second verse, the level is shifted one step downward. Usually the song being then repeated, its entire compass does not exceed a (minor) third. In other songs there occur one or more downward shifts with every repetition, yet this variant of the “stair-pattern”—which is used incidentally by the Papuans of Torres Straits in their ancient ceremonial songs—may be a more recent importation from the north. This seems the more probable as the songs of

26 Howitt, op. cit., p. 391; Gusinde, op. cit., Vol. 1.
27 Howitt, op. cit., pp. 547, 568, 582.
the central and west Australians⁴⁹ (whose culture is not primitive) actually belong to the "stair-pattern" type, and—but for the singing manner—so closely resemble American Indian songs, that, e.g., an Arunta song may easily be mistaken for, say, a Pawnee song, even by an expert. Thus the situation in Australia and South America as regards musical culture appears to be exactly homologous, and this fits well in the cluster of remarkable correspondences in cultural details, on which Professor Koppers⁴¹ has based his hypothesis of a common origin of the southeast Australian and Andamanese cultures on the one hand, and Fuegian and Californian cultures on the other. According to such an hypothesis, the forefathers of the primitive tribes (Fuegians, Californians, southeast Australians and Tasmanians, Andamanese, Vedda) would have lived as neighbors somewhere in Asia in very remote times, and from there would have migrated under pressure of more advanced tribes (American Indians, Australians, Papuans, etc.) on divergent lines, until they reached their present habitats.

CAMBRIDGE, ENGLAND


MARITAL PROPERTY RIGHTS IN BILATERAL SOCIETY

By RUTH BENEDICT

EARLY anthropological students of social organization took account only of clan societies. The curiosities of the unilateral social forms caught and held the attention of an entire generation, and led to far-reaching theories of the evolution of man. With the gradual accumulation of anthropological information it became obvious that many primitive people were bilateral, and in 1905 Dr Swanton\(^1\) pointed out that in North America bilateral forms existed precisely in those tribes in which material culture and social institutions were the most primitive or uncomplex. In other parts of the world, also, bilateral forms were found in simple cultures, like those of the Andamanese, the Fuegians, the Bushman and the Semang, and Professor Lowie\(^2\) pointed out in 1920 that these findings involve a complete reconsideration of the theory of the priority of unilateral social organization.

It has become a commonplace of anthropology to point out this bilaterality of the most primitive known tribes, and perhaps it was inevitable that this bilaterality should be confused, in subsequent anthropological theory, with the bilateral family under our own social forms. In these primitive societies, as in our own, kinship is reckoned equally through the father and the mother, and upon marriage the newly formed social unit of man and wife tends to have somewhat greater autonomy than is usual among societies where the clan is the important social unit. It was natural for students to assume, therefore, that this bilaterality in primitive societies as in our own was a recognition of "the duality of parenthood,"\(^3\) and the family, i.e. the spouses and their children, represented a focal point upon which converged property and status prerogatives drawn equally from the consanguineal lines of the father and the mother. Primitive bilaterality, according to this view, is a form of society which, in contradistinction to clan forms, does not "disrupt the household,"\(^4\) i.e. the common interests of man and wife, which in exogamous clan organizations are primarily directed toward opposing groups. In the present paper I propose to examine this

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4 Olson, loc. cit.
characterization of primitive bilateral societies especially with regard to property rights.

It is necessary to distinguish sharply between that right to the use of property which falls to the small family as a function of its common domicile, and the right to control of property over time, that is, inheritance. In all societies, except in the regions of the so-called visiting husbands, spouses have a common domicile, and their children remain with them for a time at least. Food, shelter, and household paraphernalia are therefore shared. In our own society this right to shared property in the family extends also to inheritance. Both spouses may legally inherit from their separate family estates, and the property of both spouses legally descends to their children or the living spouse, though each has the right of testamentary disposition. In other words the small family has prior claims to property which any member holds individually during his lifetime, and this priority overrides any right of the dead spouse's consanguineal kin to claim this property. The marital group is entrenched therefore in a joint right to economic goods which is by definition bilateral, and the family derives property freely from the lines of both spouses.

Such a recognition of the duality of parenthood is as alien to primitive bilateral forms of society as it is to unilateral. The closest approach to such a condition occurs among such tribes as the Ojibwa⁵ where the thorough-going recognition of individual autonomy in all aspects of life guarantees to each person complete liberty of testamentary disposition, and the hunting territories, medicine bundles, and personal effects may be left to anyone, even a white, without social interference. Property is, therefore, in certain instances left to the spouse, and even though the prevailing line of descent is patrilineal the mother may leave her property to her children. Except for such special cases, even in simple societies bilaterally organized the small family does not constitute a legal unit for the pooling of permanent rights over economic goods; the husband does not inherit from the wife nor the wife from the husband, nor do young children have prior rights to property drawn from both the father's and mother's line.

Professor Lowie⁶ in discussing this fact gives in explanation two widespread principles: (1) the division of labor between the sexes; such articles as bows and pots are sex-linked and are therefore not regarded as property which can be pooled in an inheritance that involves the passage of property from one sex to the other as in the inheritance between husband and wife.

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⁵ Ruth Landes, The Ojibwa Woman (in press).
⁶ Primitive Society, p. 244.
(2) The second causal factor Professor Lowie calls the sib factor and recognizes that this exists in tribes which are not formally organized on a sib basis. The first principle is certainly inadequate in this connection, for houses, currency, and gardens are regularly inherited by women in a very large number of bilateral as well as unilateral societies. Equally, the husband, who as a male does not labor under property disabilities, does not inherit from the wife's line, even from her father, which would involve a transfer of property only from male to male. It is the second principle which is determinative, and the point which it is important to recognize is that it is as clearly recognized in the bilateral primitive societies as in those which have a formal clan system either patrilineal or matrilineal. Property is entailed to the consanguineal relatives, excluding the spouse, and commonly not even their children, who stand within the consanguineal group in bilateral societies, are given priority, but the brothers of the deceased. Sometimes, however, if the children are grown they have priority over the deceased's brothers. Among the Arapaho the dead man's brothers claim his property and there is little generosity or charity towards the wife and children,7 that is, they are left destitute as far as the economic goods of the husband and father are concerned. Among the Eskimo, wives and young children have no claims; the children become the proverbially pitiable "orphans," and the widows are taken by other men as wives. Among the Kai of New Guinea8 upon the wife's death her brothers take all her property, as do the husband's brothers upon his death. No spouse inherits from a spouse. Among the Chukchee9 if there are no children the widow or widower returns to his or her family, and can take nothing at all. A widow with children may either marry the deceased's brother or may return to her kin; if the former, the brother (or cousin) will start the children in life in due time from the deceased's property which he takes over; if the latter, the children have no further claim, and, just as if the man had died childless, the herds will be taken by his brother and pass to the latter's children. It is not necessary to pile up examples. They recur all over the world. Against such a background the widespread institution of the levirate takes on its real significance in bilateral societies as well as in unilateral.

This channeling of property in the consanguineal group is just as striking if instead of considering fairly simple clanless societies we consider instead

those like the Samoans, the Kwakiutl, and the Ifugao whose bilaterality is of a different nature.\textsuperscript{10} In these societies high rank is inherited in lineages which nevertheless draw bilaterally from both sides of the family. In the three examples just named, from which we have adequate information, the property disabilities of the woman as woman are much less than in the preceding group. Especially in the last two she may be rich in her own right, and she may manipulate property roughly on a par with a man. In addition, in all three, the wife shares on her own account in her husband’s status activities. Both these facts make it the more striking that here again there exists the same legal claim of the consanguineal group to the property of either spouse. In Samoa\textsuperscript{11} the fundamental consideration is that each individual, male or female, has throughout life claims upon the land possessed by his or her kin of both father’s or mother’s line—either of which claims he can exercise according to his choice, but not both at the same time—but not to that of the spouse. The widow or the divorced wife with her children returns to her relatives. She does not inherit from her husband. Nor is the affinal exchange, which is here of primary importance, used to finance the new bilateral family; certain goods are transferred from the kin of the husband to that of the wife and other goods are transferred from the kin of the wife to that of the husband, but this wealth is not in any sense the property of the marital group.

Among the Kwakiutl\textsuperscript{12} a woman by a legal fiction may function as a man in any property or status activity, but she inherits from her own father’s and mother’s lines, not from her husband’s. House privileges descend in strict patrilineal succession from father to son. Other and more important privileges descend in the matrilineal line, mother to daughter and daughter’s daughter, but these privileges are regarded as held in trust for the offspring of these women by their husbands, so that the girl’s dowry upon her marriage is phrased as passing from her father to his son-in-law. Here, as we have seen above among the Ojibwa, a great emphasis upon individual autonomy allows the individual the right to alienate this property upon occasion, and the chain may be broken. Even with this freedom, however, inheritance from husband to wife is extremely rare.

The Ifugao\textsuperscript{13} have a meticulously codified law of inheritance. Barton

\textsuperscript{10} Paul Kirchoff, The Principles of Clanship in Human Society (ms.).

\textsuperscript{11} Margaret Mead, Social Organization of Manu’a (Bernice P. Bishop Museum, Bulletin 76, Honolulu, 1930), pp. 73, 97.


says: “It can never be too strongly emphasized that husband and wife
never are united into one family.” Their fundamental affiliations remain
with their consanguineal kin, and if a conflict between these two lines be-
comes bitter each will adhere to his own family and the marriage will
dissolve. Yet from this tribe come perhaps the strongest cases of joint mar-
tal enterprises. For as long as a marriage lasts, each spouse may act as a
member of both families and participate in their economic activities. On
account of the children a wife has a veto power over a sale made by her
husband; the sale will be valid, but she may collect damages. This joint
marital claim to property is most strikingly expressed in the very consid-
erable funding of the new family at marriage. In Oceania it is usual for the
affinal exchange to pass between the kin of the two spouses without becom-
ing in any way the property of these spouses, but among the Ifugao both
spouses receive allotments of land and property from their own kin and
these have been the subject of much dickering if the families are wealthy.
These goods conveyed to each spouse at marriage remain the individual
property of each, and the other spouse acquires no interest in them during
life. At death or divorce these goods return to the kin group to which they
belong and the other spouse has no claim. The law is the same for men or
women. Property acquired after marriage, if it is in no way the consequence
of a family transaction of his line or hers, is pooled. At divorce this property,
domestic animals, food stores, implements, and gold ornaments, is apportioned
between them by umpires, along with possible liabilities. At the
death of either husband or wife the living spouse inherits one half of this
property acquired since marriage by the pair, and the other half goes to
his or her brothers and sisters or other heirs in his or her father’s line. The
Ifugao have secured the husband and wife in their joint prosecution of
economic enterprises, and that without blurring the universal primitive
legal provision that entails property in the consanguineal line, although in
this case another category of property is created—property acquired after
marriage—upon part of which the spouse has claim.

This fact of the primary rights of the consanguineal kin to economic
goods, to the exclusion of the spouse, makes clan institutions, either matri-
lineal or patrilineal, not a catastrophic change in social arrangements, but
an ever-recurring possibility.14 Clans represent a formal naming and classifi-
cation of kin groups which were everywhere present in the economic
sphere even though unformalized.

I have considered this solidarity of the consanguineal kin only in rela-
tion to property because the facts in regard to the transmission of status

14 R. H. Lowie, Social Organization (Encyclopaedia of the Social Sciences, New York,
have recently been admirably assembled by Professor Radcliffe-Brown in a paper which deals with mechanisms of transmission in societies where unilateral forms obtain. His conclusion from this material is that the development of such unilateral forms is due to a sociological law according to which more and more precise definition of rights and obligations is socially achieved and continuity over generations more adequately secured. This is certainly true. It seems to me equally important to recognize that unilateral transmission of property and status patterns bilateral sibless societies no less than unilateral ones. A thoroughgoing recognition of this fact would make the development of unilateral institutions in tribes where conditions are more complex merely post hoc evidence of the fundamental importance of this mechanism in early simplest social organization. This mechanism, from this point of view, is one of the early and universal "inventions" of the human race, and wherever social environment becomes more elaborate, naturally becomes more explicitly and definitely formulated. Thereupon, such societies are classified as unilateral. If we understand the matter in this way, many current statements become impossible, such as "Unilateral societies, wherever found, represent deviations from the expectable, abnormalities in the special structure," and, "Unilateral institutions are in themselves anomalous and artificial. Matrilineal ones doubly so." All such formulations rest upon a lack of analysis of the property and status mechanisms of bilateral primitive societies.

The facts just considered have bearing also on the old debate as to the relative importance of clan and family in primitive social organization. It is probable that the debate would never have been set up in these terms but for the classical anthropologists' priority in the field with their far-flung theories about social evolution suggested by their preoccupation with clan forms of organization. Clearly, the family as a group of spouses and their children who, whether they are by themselves or in some larger group, live and work together is fundamental in all human society except in the region of the visiting husbands. But just as clearly, the unilateral kin group has vested in it the status and property rights which gave an individual his place in the community, and this is as true of bilateral as of unilateral societies.

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16 Olson, op. cit., p. 411.

17 Olson, op. cit., p. 409.
FATHERHOOD IN THE WIK MONKAN TRIBE

By DONALD F. THOMSON

The ignorance of the Australian aboriginal of the fact of physiological paternity has been affirmed so often that it is now generally accepted in anthropological theory as a fact. Any conception of the existence of a physiological, as distinct from a sociological, bond between father and child is said to be foreign to the aboriginal mind. Nevertheless, in a recent communication I have shown that the natives of the Koko Ya’o and neighboring tribes of the east coast of Cape York Peninsula, North Queensland, are aware of the relation between sexual intercourse and pregnancy, and that they recognize a physical, as well as a social, bond between father and offspring. But in the same paper I showed that these tribes of the east coast of Cape York Peninsula had been influenced strongly by hero cults of Papuan origin that had modified considerably the indigenous culture.

The extension of field work on the Gulf of Carpentaria subsequently has shown that the knowledge of physiological paternity is widespread in this region. The natives of the Wijk Monkan tribe of Archer River, and also those of neighboring tribes, distinguish between physiological and sociological aspects of paternity by use of special terms, which are employed in conjunction with those of the kinship system. On account of the interest and importance of this subject, a study of the place of the father in the Wijk Monkan tribe is presented at some length in this paper.

The social organization of the Wijk Monkan tribe differs in several respects from that of the Koko Ya’o and allied tribes of the east coast of Cape York Peninsula. It is organized on a basis of localized totemic clans with patrilineal descent, and as in the tribes of the east coast, there is no class or “section” system for the regulation of marriage. The Wijk Monkan and allied tribes differ from those of the east coast, however, in the absence of personal totemism, which in those tribes is of an anomalous and specialized type, and in the absence of named moieties, which are present in the Kanju tribe to the east and in the Wijk-speaking tribes to the south.

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2 The phonetic system employed for vowel sounds is: a as in father, o as in no in open, e as in fetch. i as in feet, u as in fool. I as in fit, y as in full.
over, while the marriage system in all the tribes of the east coast is with
the classificatory first cousin, in the Wjk Monkan tribe it is of the second
cousin type, i.e., with the father’s mother’s sister’s daughter’s daughter.

It would be impossible to live for long in close contact with the Wjk
Monkan people and deny their knowledge of the relation between sexual
intercourse and pregnancy. It is revealed repeatedly throughout their social
life, and it is implicit in their mythology. “A woman cannot get a child for
nothing,” my informant stated.

But in spite of this knowledge, which is very different from that re-
corded by some workers in Australia, “a baby totem center” (púk au’wa),
exists in one of the clan territories of the Wjk Monkan, at which ceremonies
for the increase of babies are carried out. At a superficial glance this may
appear to be incompatible with a knowledge of the relation between sexual
intercourse and conception. A careful examination of the facts must there-
fore be made so that the relation of this cult to the knowledge of physiologi-
cal paternity may be understood, and the whole oriented with the totemic
system.

The Wjk Monkan recognizes, and freely affirms, the fertilizing influence
of seminal fluid (tänkarrä), but on the physiological aspect of conception
and pregnancy, his knowledge is less exact. He recognizes that pregnancy
results from the introduction of seminal fluid, but as to how the embryo is
produced, his ideas are as vague as those of any white man who possesses no
biological knowledge. His belief is that the seminal fluid enters the uterus
(po’q mompa) and gradually builds up the body of the embryo, and thus
he insists that a single sexual act is not sufficient to produce conception,
which can result only from repeated intercourse.

At Mapoon Mission on the Batavia River a young man came to me and
complained bitterly that he had been induced to marry a girl, who had sub-
sequently given birth to a child of which he was not the father. He admitted
that he had had sexual intercourse with the girl before marriage, but he
affirmed that as this had occurred only once, he was sure that the baby that
had been born later could not have been his own. Nothing that I could say
to him would shake his belief: married men, he declared, had told him that
conception followed only after repeated acts.

PREGNANCY AND CHILDBIRTH

The actual statements of my informants, in the Wjk Monkan language,
are set out in full below, for they reveal not only the beliefs about concep-
tion and pregnancy, but also that these people recognize most of the im-
portant indications of pregnancy that form the basis of present day diag-
nosis.
päm etjina wäntj ma'a yotäm'4 etjina putta
(The) man copulates with (the) woman many times; copulates until
pük owun n'yill tänkärrä etjina n'yill tänkärrä etjina ma'a
baby finds. He semen copulates; he semen copulates, many
yotama ma'a yotama tänkärrä n'yill ark wöi'yin nät'n aköiyäk
times, many times. He semen it road (canal) closes menstrual
blood for.

“That tänkärrä blocks,” added my informant.

aköi käñä wänt'n 'ñany pukkäk nät'
Menses finish stop: me child for might (be).

“The man now starts to give extra food to that woman to help that
body,” my informant said.

n'yill tänkärrä tip màkän päm puttäm wäntjkuntj
It (the) semen belly sticks; (the) man again wife his
etjin n'yilla pük emän tänkärrä nät'tjn qì'yìn pek
copulates with. It (the) child grows. Semen goes in hole inside,
tita mompäm' wùn pek puttämä n'yilla päm wäntj nonåndäm
egg uterus in lies inside. Again he (the) man woman his
etjin puttämä ma'qì n'yillä wäntjä tau'wän n'ai'ya
copulates with again plenty times. She (the) woman speaks, “I am
pukkätäm n'yillä wäntj nät'tjìn yerp ännän pük mer
child with.” She (the) woman feels hypogastrium heavy baby eye
pitj'n
break.

aköi wänt'nä putt'puttämäk ark nè'yjn
Menses left off again more for waiting for (anticipating) menses for
put ya'a puttämä käp tonäk n'yillä nèrtäm nè'yjn put
but nothing; more (again) moon another for she anticipating, but

4 Ma'a yotämä, plenty times, is idiomatic. Yotämä means plenty, ma'a, hands; hence the
literal meaning of the phrase is “plenty hands.” The word is derived from the fact that time
is reckoned by counting with the hands. Similarly, ma'qì, occurring later in the same passage,
is derived from ma'a, hands, -qì, a suffix meaning plenty. For example, kek, a spear; kekqì,
literally “plenty spears,” is the proper name of the echidna (E. eculata).
5 See also emät'n, grower, page 384.
6 Mompa is applied to the foetal membranes and to the placenta, pq'qì mompa to the
uterus.
7 The expression above, ark nè'yjn, is used for “looking forward” to a thing, anticipating:
 nàrtäm nè'yjn is, however, used for expectation of the recurrence of something.
ya'a mer pük pîjtîn putâ pukktâm ñul
nothing. Eye baby breaks but child with now.

The belief that repeated sexual acts are necessary to build up the baby from seminal fluid is evident in this statement. The recognition of the cessation of the menses as an early indication of pregnancy is of interest, as is also the conception of the closing of the genital passage which is, of course, a biological fact, in the formation of the plug of mucus in the internal os. The natives also associate with pregnancy the appearance of the stria gravidarum on the hypogastrium (yerp), although they attribute the stria to the fingernails of the unborn child rather than to the distention of the abdominal wall. The Wik Monkan believe that there is a bag (mompa), the name applied to the foetal membranes and also to the placenta, in which the seminal fluid is stored, and within which it assumes gradually the form of an egg (tita). At first this bag (pô'ô mompa), which my informant described as ark pukkâk (place baby for), is situated in the hypogastrium.

My informant suggested that the reason old people did not have babies was because "they start to shut this place," and again that "old people do not go much," i.e., they become impotent.

The following is a description of the early signs of pregnancy as given to me by a woman informant:

When the woman becomes aware of the cessation of the menses she talks only to her husband and to her mother. People notice now that the breast (pap) enlarges and changes, but they do not yet speak openly of her condition. The nipples (pap mer, literally "breast eye") become black (not-tändâm). Then the hypogastrium swells. The woman is not yet known as ìmpânâŋ. The navel (kört'n) is now thrust outward and everybody knows definitely that the woman is pregnant. Her entire body enlarges (kempijiti), her abdomen grows bigger (tip kân emân, stomach starts growing).

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8 The uterus; while mompa includes the foetal membranes, pô'ô is the generic name for the female pudenda; pô'ô mompa is therefore literally "the bag of the genitalia," the uterus.
9 Ìmpânâŋ is the name applied to a pregnant woman. It is derived from nonk wonka tonn, a restricted language, apart from the language of ordinary conversation (wik), that is reserved for certain specialized uses, particularly in speaking in the presence of relatives of certain orders before whom ordinary speech is prohibited. Tip is the ordinary or wik word for belly; impan is the nonk wonka tonn word, hence ìmpânâŋ, belly-with (-ân) is a pregnant woman. (For a fuller account of this restricted language, see Thomson, The Joking Relationship and Organized Obscenity in North Queensland, American Anthropologist, Vol. 37, pp. 460–90, 1935.)
10 Kemp, body, -jiti, suffix meaning big; hence kempijiti, big body.
At this stage she begins to be known as ĭmpānāŋ, a name that is used as a term of address, even in polite conversation, and takes precedence over the kinship terms in general use. Her husband is now known as ĭmpānāŋ wunjpuŋ (įmpānāŋ, the pregnant one; wunjpuŋ, placer, begetter), a term also used in place of the usual kinship terms. This terminology may be extended to other relations; e.g., the father's younger brother may be called pip emät’n ĭmpānāŋ (the growing father of the pregnant one).

The pregnant woman is now subject to a great many tabus, especially to restrictions on the eating of certain foods.

No pregnant woman may eat the large water snake popularly known as Javan file snake (Acroterpeton javanicus, mįn mūllai), and there are numerous restrictions on the eating of other foods, especially of small furred mammals (mįn šātjà) such as the native cats (Dasyurus), bandicoots (Isoodon), and also goanna (Varanus sp.). She may eat only small goanna, especially a little spotted one (mįn pūpāṟā), young native cats (mįn yāsāntān), young bandicoots (mįn pūk [mānį]), but not the adults of these animals, nor any big Varanus. Emu (mįn āṭjimbā) is especially tabu (nāiṇtjà), and she must not eat even the chickens or the eggs of this species, lest the child should have weak knees (punk, knee, mįtjà, soft, weak). She may eat any wild honey, except that of a certain species of very small bee or from a nest in a leaning tree up which fire has travelled, lest the baby should have “boomerang” legs (yānk wīntjà).

Nor may she eat native companion (mįn korra), jabiru (mįn mont), pelican (mįn mōrt), flying foxes (Pteropus scapulatus, mįn wūk, and Pteropus gouldi, mįn okāmāttà). Among fish she is especially forbidden to eat large specimens of rock cod (mįn nombaŋōmbjn), night fish (mįn wonkām), a stingray with tubercles on its back and a short tail (mįn mūttà), and a stingray with a long tail (mįn wāŋān).

On no account may she eat any old male wallaby or kangaroo of any species (mįn būrrijān, a term generically applied to the old male animals), but she may eat a female (mįn o’läk), and she may eat a species of fresh water tortoise (mįn puntuāj).

All snakes, as well as certain other reptiles, are tabu. Chief among these are the black-headed python (Aspidites melanocephalus, mįn yumuṭjà), scrub python (Python amethystinēs, ʂīŋoṛpān), carpet snake (Python spilotes, mįn pōla), and rock python (Liasis fuscus, mįn tīntau). The crocodile (mįn pīk’u) and its eggs (mįn pīk’u tit) are tabu. But she may eat the eggs of scrub fowl (mįn tįtikołok), scrub turkey (mįn kōnt), Burdekin duck (mįn ko’ān), and black duck (mįn temp). No restrictions are placed on the eating of vegetable foods.
The tabus on the eating of all these foods remain in force until the baby walks about (pyk kän i'n), when all, with the exception of those on the eating of emu and plain turkey (mën màntäbä, plain turkey) terminate. The woman may now, however, eat old male wallaby or kangaroo. The tabus on the eating of most of the birds named above are removed gradually as the child grows, the last being those on the eating of emu, the turkey, and jabiru, which remain in force until the child is able to speak properly (tänánt tāi'jīn, tongue strong). If the mother should break any of these food tabus, the child will develop sores (ṇaintjjm) on its head.

But during pregnancy the imposed tabus are not confined to the woman, for the husband of the pregnant woman is also prohibited from eating certain foods, chief among which are emu, plain turkey, and native companion. He may eat wallaby (mën pänk, specifically Macropus agilis), even the old males, except those with very large forepaws (mën pänk būrgiän ma'-jīti).11 Neither the man nor woman may eat a wallaby or red kangaroo (M. rufus, mën kōimpī) that has been killed by a bush dog (kō'a nekānäm), but if killed by a camp dog (kō'a) he may eat it. If this tabu were broken the child would always be sick and thin (pyk alpana, a poor, sickly baby). This tabu must be observed by the father until after the child walks, and until it is able to speak well. Nor may the man eat rock cod or the two kinds of stingrays mentioned above.

When the birth of the child is imminent, that is, at the onset of labor pains (tip kän pikän, belly strike), the husband moves to the single men’s camp, and the woman moves at once to a shelter (ark yōink, literally “a shelter place”) some distance from the usual camp. This place becomes tabu and is known as the ark yānwi; it may be visited only by women until after the newly born child has been presented ceremonially to its father.

The following account, given to me by a woman, of the birth of a child up to the time it is presented to its father is written in the exact words of my informant.

wäntj pe'jnäk enk taa jнт'näk yänk yul
(The) woman minding for sides rub heart forehead (precordia)
wäntj pe'jnäk yerp put mänk nák kōrtja12 nörtj kän
woman minding for hypogastrum and back water head mucous finish
pijt kūtjäk kän pent'n känä n'yillä katkuntj
break head for finish coming out. “Finish!” She mother its

11 Ma'a, hand, paw, -jīti, suffix to express bigness; see also footnote 10.
12 The amniotic fluid, literally “the head water;” kōrtä, head, also kūtjäk.
yäm’yäm n’yin ark wänt’n pükkäk kört’n wùnyatän wa’ànäk a little distance sits place leaves child for navel?
call over
nämpäk n’yìll känä nämpä ne’jin wäntj pentjändän puk wòt’o’oh¹² name for. It finished name heard woman. “Child boy!”
kört’n we’ä kört’n n’yìll’o päm’älkän’oh “Navel who?” “Navel (name) it (is) Pämälkän (personal name).”

After the birth of the child the navel name (nämp kört’n) is determined by divination. The midwife takes the umbilical cord in her hand and shakes it, at the same time calling aloud names of various relatives of the child. The name called at the moment the placenta is delivered is the name of the baby. The umbilical cord is cut a few inches, about a finger length (ma’a puk),¹⁴ from the navel and is left until it dries and falls off. It is then encased in beeswax and carried as a token or charm by the individual whose name the child has taken. The afterbirth is buried at once. The relatives whose names may be called are the father and father’s brothers and sisters, the child’s actual brothers and sisters, mother’s brothers and sisters, but not the mother herself, father’s father or mother’s father, as well as many other relatives of a similar order, actual or classificatory. Thenceforth a special relationship exists between the baby and the one whose name it has taken: they are “those two (one) navel (name)” (pull kört’n). To one another they say, “We two (one) navel (name)” (ußall kört’n). Some weeks later the baby, painted and decorated for the occasion, is ceremonially presented to the person whose name it has taken.

The following is an account of the finding of the navel name by divination, as given in Wjk Monkan by an informant:

kört’n ompäin ompäin tjìll onk ka ma’a pük mopmäp
Umbilical cord cut, cut little’bit long like finger. Afterbirth
wùkùrräŋ kampän kört’n wäntän n’yìll n’yìlläm
bury cover navel leave all alone (lit. it it, i.e., by itself)
e’emän wäntjìnta kat pi’in
dry itself old woman. Mother big (i.e., own [physiological] mother)
kem waiyo ŋoink’ŋ pe’jìndän pullmäny’
mother’s mother shelter in mind woman who has given birth

¹² Wòt, old man, is also used to denote male sex in children and in certain animals in which the sexes are not distinguished by separate names, e.g., mìn kòlàn wòt (a male opossum, Trichosurus).
¹⁴ Ma’a, hand, puk, child; hence the fingers are literally called “the children of the hand.”
n’yenkänäm nulta ke’an ma’a yotäm wun pylkän tätättänä new to remain nothing long time stays child there watching kemp nōtn’atān pyl ka’ätäm kemp met ark ŋai’iŋ pyl skin black turning. Baby at first body white (light) born. From nämäm ma’a tjilloi wun n’yill kemp kān notn’mān pyl kɔrt’n there hand little stay it body finish turn black, until navel kekān kempāŋ putta nämäm nām pipkuntjt falls body from from there father its to.

CEREMONIAL PRESENTATION TO THE FATHER

Following the birth of the child the mother remains in isolation in her shelter for a period of from two weeks to a month. During the whole of this time neither the father nor any other man may see the mother or baby.

During her seclusion the mother is attended only by her female relatives, especially by her own mother (kat), ŋātjawé’o, mūkk waiyo, mother’s mother, and her sisters. The older women remain almost constantly with her, while her mother and sisters go hunting. As she grows stronger, she may walk about accompanied by these relations, but she must not be seen by men. When I asked my informants how this could be prevented, they replied simply, “The men know which way they go and therefore avoid that place.”

After the birth of the baby, the woman, known throughout the period of pregnancy as impānŋ, is now called pylmāny n’yenkānäm (the new pylmāny), and except when she is again pregnant is called pylmāny’ (woman who has borne a child) for the rest of her life. The child is known as “tabu child” (pył ŋānwi) until after its ceremonial presentation. The father is ŋānwi wūnpun, or more correctly ŋānwi ŋārrpān, for the use of ŋonk tonn, the indirect language to which reference has already been made, is preferable to ordinary speech at this stage. After the ceremony the father becomes “placer or begetter of the child” (pył wūnpun). The relatives of the father are also distinguished in the same way by terms which express their relationship to the child rather than by personal names. Thus the father’s elder brothers and sisters become ŋānwi pinjin, his mother’s elder brothers and sisters are ŋānwi mūkkānjin—the pinya or mūkkka respectively of the newly born child. The name of an adult individual is almost never called aloud, nor is it employed as a direct term of address. If the use of the appropriate kinship term is not sufficient to distinguish an individual, the name of a child or even of a brother’s or other relative’s child may be used, as in the following: “The father or begetter of Pāmkotjāttān!” (Pāmkotjāttān wūnpun). And I have heard a women hailing another from a long
distance as, "Pinya of Pämktjättän" (Pämktjättän pínjin), in this case the father's elder sister of Pämktjättän, who was a little girl.

The ceremonial presentation of a child to its father that is described below took place on the Archer River in May, 1933. On this occasion the new mother was attended by her husband's mother to whom she applied the term father's elder sister (pinya). The relations (päm kämpän) squatted on the ground, the father a little in front as shown in Plate 8. Meanwhile, in the tabu shelter the mother and baby had already been painted in preparation for the ceremony, the mother with white clay (pip met) only, the body of the baby smeared first with red ochre (wa'a) and then painted with white. Its breast was marked with longitudinal streaks of white, and it was adorned with a mother of pearl breast pendant (önkäm); one necklace of mother of pearl nacre (männ ättuwa) was placed on its forehead and another on its neck with native companion feathers inserted under the latter.

With the baby in her arms the mother now left her shelter and advanced slowly toward the waiting relatives. The old woman walked behind her carrying a bird's wing fan (punta pam ko'äń), in this instance made from the wing of a magpie goose (ko'än), for the purpose, according to native statement, of driving off the flies that might follow from the place of seclusion. This was said to symbolize the severing of the association of the mother and child with the tabu place. More often it is the mother's mother (kemä) of the mother, a very old woman (who stands in the relation of elder sister [yap] to the baby) who follows the mother with the fan. A quantity of vegetable food, chiefly mai ka'arra and mai wättiyi, is usually carried by the new mother, but if she is too weak, by her mother's mother. But this time the presentation of mai was omitted.

The mother slowly approached the group seated on the ground. This was the first glimpse the father had had of his infant, but he sat with eyes downcast:

\[\text{pip ka tuťj'näŋ n'yin}\]
Father nose (face) bowed with sids

When the mother reached the group she approached her husband and walked twice around him in the same direction; after which she walked towards his eldest sister (pinya' mäntaiyin)\(^\text{16}\) (plate 8, fig. 7), and completely encircled her as she sat on the ground, the old woman still following closely behind with the fan to drive off the flies. After this the mother walked up

\(^\text{16}\) Mäntai'yin is a "big" person, an elder, a person of special importance in some aspect of social life, e.g., wöt mäntai'ín, a big man, literally a big old man.
Ceremonial presentation of a child to its father among Wik Monkan. Group of relatives awaiting the arrival of the mother with the baby. 1, Father's younger brother; 2, Father; 3, Father's elder brother; 4, 5, 7, Father's elder sisters; 6, Father's younger sister; 8, Mother's mother of the child. (The postures are typical attitudes assumed on ceremonial occasions.)
Mother and child, painted in readiness for the ceremonial presentation, at the place of seclusion. To the right, the father's mother, the attendant during seclusion.
to the father, and seating herself before him, presented the baby to him. The food is also usually presented to him at the same time. He takes the food and looks at it, but he may not eat it: this is the privilege of his father (poła, the father’s father of the child) and of the mother’s father (ηάτjά).

When he had received the child in his arms, the father passed his hand under his axilla and rubbed the sweat, or “smell” as the natives say, on the head of the child (plate 11).

\[
\begin{align*}
\text{pip wùnpŷn} & \quad \text{ma’a yuānāŋ} \quad \text{tentj poŋntjilŋ}^{16} \quad \text{pʊk te’ān} \\
& \quad \text{(The) begetting father hand axilla at wipes smell with child smears.}
\end{align*}
\]

Then the mother in turn rubbed her own axillary sweat on the shoulders (pįtjām) of her husband (plate 11).

The object of this rite was to avert the bad luck in hunting that might otherwise ensue.

In addition the father rubbed his axillary sweat on the knee and elbow joints of the child; then taking each of these in succession between his teeth, he bit it gently in order that the child might grow strong.

SOME ASPECTS OF FATHERHOOD

It is usually stated that, to the aboriginal, fatherhood is purely social, and that any conception of a physical bond between father and offspring is foreign to the native idea of this relationship. It must already have become apparent that the father among these people is very different from the father as described hitherto for most Australian tribes. The conception that “group marriage”—a hypothetical state wherein a group of men consorted more or less indiscriminately with a group of women, and nobody knew his own father—was formerly practiced universally in Australia was held to be responsible for the fact that the same term was applied without any apparent distinction to the physiological father, and also to a number of men who were brothers of the father. This conception has not only left its stamp on much of the early theoretical work dealing with Australian aborigines, but also has exerted a profound influence on field workers.

I have shown that the natives of this tribe are aware of the fact that pregnancy and sexual intercourse are related, and that they recognize and speak freely of the fertilizing power of seminal fluid. But they go far beyond this, and in employing the kinship term for father, they recognize and distinguish by the use of special terms, between the physiological and

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16 Although it is actually sweat (kimp) that is employed, the word poŋntjil means “smell;” therefore the application of the father’s or mother’s “smell” is a vital part of this rite.
sociological aspects of this relationship. This fact is emphasized in the ceremonial presentation of the child to its actual father, its pip ṭunta wûnpuń (pip, father; ṭunta, arm; wûnpuń, place), as described above.

The generic kinship term for father is pip, the reciprocal term, nenk. A child has only one pip wûnpuń, although it may have many pip. Thus all the father's younger brothers are pip mãń', little fathers; but one, usually the one immediately succeeding the father, is called literally the growing father (pip emätt'n). Under the levirate a man may marry the wife of a deceased elder, but not of a younger, brother. Socially this man is already "father" to his elder brother's children, and they already call him pip. Since he will have first claim to the widow of his elder brother, i.e., since he is the potential husband of the widow and adopter of the children, he is known as their "growing father." Thus any man who may marry the mother of the children may become their "growing father."

The actual brothers of the father, to whom the term father is usually extended in the kinship system, are sharply divided into two groups according to whether they are older or younger than the father. Thus while the father and all his younger brothers, own and classificatory, are pip or pip mãń', the elder brothers of the father are pjnya or puk pinjin, and although they take a great interest in the child, they are in a different category from the father and his younger brothers. This distinction between the elder and younger brothers is evident even before the child is born, from the days when its mother is first pregnant. The husband of a pregnant woman is distinguished by a name applied to the actual husband only (impănåŋ wûnpuń), while his immediately younger brother is "the grower of the pregnant one," (impănåŋ emätt'n), the man who will marry the pregnant woman if her own husband should die. At this period the elder brothers are all impănåŋ pinjin. At birth the father is ŋänwi ńärpåń;17 the younger brother of the father is ŋänwi emätt'n, and the elder brother is ŋänwi pinjin.

But this distinction between the older and younger relatives is not confined to the brothers of the father; it extends also to the mother (kat) and her sisters. A child has many kat, but again only one physiological mother, kat pi'ın, literally "big mother." Of her own child a woman says:

puk ŋai'ya tip ŋai'ya källän
Child I belly I carried.

"The child that I carried in my belly." The real mother is referred to by others as:

17 Nänwi ńärpåń belongs to the special tabu language, as impănåŋ does (see footnote 9).
The mother encircling the father.
The father rubbing his "smell" on the child, the mother hers on the father.
kat énomändäm tip käll’n
Mother his very own belly carries.

If a man or woman wishes to stress the fact that he or she is a full sibling of a third person, he will speak of him or her as tip tonäm, belly one, i.e., from one womb.

All the younger sisters of the mother, who are potential wives of the father, or father’s next younger brother, are kat möny’, little mothers, to a child. One of these, usually an actual wife of the child’s father or of the “growing father,” is the kat ñkkänpyän, the adopting mother. She will adopt the child if the mother should die. As she is the younger sister of the mother, she is often already married to the same man, or if unmarried, is his potential wife.

As with the elder brothers and sisters of the father, those of the mother are grouped together as måkka. They are the potential husbands and wives of the father’s elder sisters and brothers. One of the important aspects of this elaborate terminology is the stress it lays on the distinction between the physiological and sociological aspects of paternity, and the provision for the adoption of children. While there is only one begetter, all the younger brothers of a man are potential husbands of his wives, the mothers of his children. They are recognized as fathers in the classificatory sense; but only one, he who will marry the mother of the children, is the “growing father;” and only one woman, usually the wife of this man, is the “adopting mother.” On the other hand, none of the father’s elder brothers may marry the widow: they are not potential adopters of the child. With the father’s elder brothers are grouped the father’s sisters, both older and younger. In keeping with this distinction is the fact that the pattern of behavior of the child toward the father’s younger brother is very similar to that obligatory to the father himself, but towards the elder brother, who has a considerable amount of control over the child, more restraint is expected: the pattern is one of respect and deference rather than that of love and freedom which characterizes the relationship with all those to whom the term pip is applied. When I asked a woman how a child behaved towards its father, she replied, “They play up” (män kenn), then she added, “They behave half like naughty children.” The greatest indulgence is shown by a father to his children. When they play up he pretends to be angry; he makes signs of anger such as protruding the tip of his tongue between his teeth, a gesture called tep bättän or tänjant bättän (tongue biting).

Subject to certain tabus and restrictions, a father feeds his own children. But he may not eat any vegetable food (mai’) provided by his daughter until she is a well grown girl (kommän), nor may he eat any food killed by
his son until the boy is partially initiated, that is, has passed through the first stage of initiation (ŋãniwi) known as ʊtjinäm. The first foods taken by a boy are eaten by his grand-parents including his father’s father (poł wọt; poł, father’s father, wọt, old man), father’s mother’s elder brother (näjt wọt, i.e., his father’s mukka), because that are “big men.” Neither of the parents may eat food of any sort procured by the young child, even when it brings little offerings of food gathered in play.

If further evidence is required to prove the belief of the Wjk Monkan people in the fertility of seminal fluid, it is provided by the myth of Këpp, the moon, who formerly dwelt on this earth as a man. Because of the importance of this subject and the bearing that it has on primitive psychology, I give the native text just as I took it down. It loses nothing as evidence by the fact that it was obtained originally merely as a myth, with no prior knowledge of the light it would shed on the subject of the present discussion.

këpp wjk kät¹⁸
Moon talk old

ka’ätäkäm päm iä këppä mën kọi’yä iyä¹⁹ ompi’yin
Long ago man was Moon. Fish string fish poison (he) cut,
teppiyin kättiyin kättiyin kättiyin ƞäkk’ƞ teyin teppiyin
hammered, tied up, tied up, tied up; water in threw, hammered,
teppiyin mën ampäŋä tjintayin pantiyän ƞäŋämä këppä
hammered. Fish rose up (he) spearèd. (He) slept; at daybreak the moon
tutja mën mamonkiyin wäntjinta worr ampäŋ
sank down fish he was eating. (An) old woman got up (from sitting)
pekä tätta wäntjinta tau’wa mën än pek ƞtämä ƞai’yä
down looked (the) old woman spoke, “Fish there inside dead. I
workamana tutja eppänka tonn wórkäm koiyam eppänk
shall swim.” Sank, turned round. Another swim, back turned round.
wọt wórkäm ya’a tätta wọta tonn wórkäm ya’a
(An) old man swim; nothing saw. Old man another swim; nothing
tätta wọta tonn wórkäm ya’a tätta wọta tonn wórkäm
saw. Old man another swim; nothing saw. Old man another swim;

¹⁸ Kät is most frequently used in the sense of bad, stinking, decayed; in this sense when used for “myth” it simply means old, old fashioned; wjk kät, talk of olden times, hence a myth or legend.
¹⁹ Mën is any animal food, in this case fish is intended. Iyä is the dynamite plant (Derris trifoliata, var. macrocarpa); koi’yä, string, the vine or liana of this plant, which is gathered and tied into tight bundles for use in poisoning fish.
ya'a tättä wōta tonn yul wɔrkām ya'a tättä wōta nothing saw. Old man another now swam; nothing saw. Old man tonn wɔrkām ya'a tättä wōta tonna wɔrkām ya'a tättä another swam; nothing saw. Old man another swam; nothing saw.

wōta tonna wɔrkām ya'a tättä wōta tonna wɔrkām Old man another swam, nothing saw. Old man another swam;

ya'a tättä nothing saw.

wot tonn tänp' tonn yul wɔrkām tonn yul Old man another bumped into. Another now swam. Another now wɔrkām wäntjinta wäntjinta tonn wɔrkām wäntjinta tonn swam old woman. Old woman another swam. Old woman another yul wɔrkām wäntj tonn wɔrkām wäntj tonnām wɔrkām now swam. Woman another swam. Woman another swam.

päm kɔmpā wɔrkām kɔmpā tonn yul wɔrkām pu̍k ūṭjin Young man swam. Young man another now swam. (A) little boy wɔrkām ūṭjin tonn yul wɔrkām ūṭjin tonn yul wɔrkām swam. Boy another now swam. Boy another now swam.

kɔmpā tonn wɔrkām kɔmpā tonn wɔrkām kɔmpā Young man another swam. Young man another swam. Young man tonn wɔrkām pu̍k ūṭjin wɔrkām ūṭjin tonn yul wɔrkām another swam. Little boy swam. Boy another now swam.


pull kɔmmān wɔrkām owā pull owā pull Those two young women swam. Found those two; found those two;

owa pullā pull kɔmmān kān pimpa found those two; those two young women. Peeped out (from water)

pulta māmnā ⁵⁰ pullāŋ kān etj' pull etj' pullāŋ arm seizes those two copulated, those two copulated those two with;

etj' pullāŋ wany' matāttā tāttāi'nyi copulated those two with. On top (above) go up. “Look out,

⁵⁰ Punta is the arm or brachium; ma'a punta, literally “hand arm,” is the forearm; hence ma'a punta māmnā, forearm seize, is the term for any forcible seizing of a woman, especially for marriage by capture, which was of frequent occurrence in former time on the rivers about Albatross and Archer Bays, on the Gulf of Carpentaria.
(tättäī’ nānyi) tänkārrā kān yetjīn ni’ndā pām look at me. Semen poured out for you all.” (A) person, wot wāmpā īn wai’yā tonn wāmpā īn wai’yā (an) old man came. “This (is) bad!” Another came, “This (is) bad!” tonn wāmpā īn wai’yā tonn wāmpā īn wai’yā Another came, “This (is) bad!” Another came, “This (is) bad!” wāntjinta wāmpā īn wai’yā pūk ūtjīn wāmpā īn (An) old woman came, “This (is) bad!” Baby boy came, “This wai’yā pūk māny’ wāmpā tāttā īn wai’yā ūtjīn wāmpā tāttā (is) bad!” Baby little came, saw, “This (is) bad!” Boy came, saw, īn wai’yā wot wāmpā īn wai’yā pūk mānya tonn “This (is) bad!” Old man came, “This (is) bad!” Baby small another wāmpā īn wai’yā wot tonn wāmpā īn wai’yā came, “This (is) bad!” Old man another came, “This (is) bad!” wot tonn wāmpā īn wai’yā Old man another came, “This (is) bad!” kāppā tau’wa nāiyām mūnkāŋ ma’āŋ māiyāŋ (The) moon spoke, “I myself will eat (it)!” Hand with picks up, mūnka ta tekā te’e ni’yā utāmānnāŋ ark omptēnām nāiyāŋ ate. Mouth speak threw, “You will die for good. I te’āŋ wunāŋ nāiyā’ ūtāmmāŋā nāiyā’ pentāŋa puṭtām throw, lie down, I shall die. I shall get up. Again utāmmāŋa nāiyā’ pentāŋa puṭtām ūtāmmāŋā nāmmānām (I) shall die. I shall get up again. (I) shall die from there; iāŋ nāŋkā te’āŋ nāmmānām ūtām pent kōiyām pent (I) shall go heart throw.” From there dead come out again, come out.

The following is a free rendering of the myth.

Long ago, when the moon was a man, he went fishing. He cut some stems of the dynamite plant which was used as a fish poison, tied them into bundles, hammered them, and threw them into the water. When the stupefied fish rose to the surface Moon speared them; then he slept. At daybreak, as he was eating his fish, he sank down under the water.

An old woman got up, looked down into the water, and cried, “There are some fish down there dead; I shall swim for them.” She went down but she turned back without getting any fish. Another old woman swam, but she too turned back. Then an old man swam, but he saw nothing. Another old man swam, and then another

21 Kān, finished, in each instance signifies past tense.
22 An idiomatic phrase, “I shall rest.”
and another but they all saw nothing. One of the old men only bumped into one of his fellows; then another tried. Then an old woman went into the water. Still another old woman went down, and then little boys and girls; but they did not see anything there. At length two young women went into the water. They found it (fish?). They peeped out of the water, but Moon seized them and began to rape them. He copulated and copulated with them; then he came up and said, "Look at me; I am pouring out my semen for you all." But an old man came and said, "This is bad!" And another old man came, and he also said, "This is bad." Still another came, and then an old woman, and after these other women, and young boys and children. Each in turn looked at the semen, but they all cried, "This is bad!"

At length Moon spoke, "I shall eat it myself!" He picked up his own semen and swallowed it, and then cried, "You shall all die altogether. I shall lie down; I shall die, but I shall come up again. After that I shall die again, but I shall come up again and again. After that I shall rest, but each time I die I shall come back again." So, after the old moon dies he rests awhile, but the new moon always appears again later.

The legend of the moon is associated with the Moon totem center, käpp au'wa, at ark käppau'wëŋäm (literally, place Moon totem center belonging to) on Archer River.

BABY TOTEM CENTERS

Reference has already been made to a baby totem center (pyk au'wa) at which rites for the increase of babies were carried out. The existence of a baby totem center has often been taken as final proof that these people were ignorant of physiological paternity.

I shall not recount in full here the long myth that tells of the origin of the baby totem center, but will give in outline that part which is associated with the totem center and its relation to totemism and to the knowledge of physiological paternity.

The totem center is situated at Ark Örnyau'wa on the south side of Archer River, about fifteen to twenty miles east-southeast of the mouth. Ark is a place, a country, or a native land. Örnyä is the Wîk Monkan name for a ghost or spirit, generally the ghost of a dead man or woman; au'wa22 is a totem center, or any source of plenty. The Örnyau'wa clan was a large and powerful one, and like many of the outlying clans of the Wîk Monkan tribe, its language showed a few dialectical peculiarities. For this reason it was sometimes spoken of as if it were a separate language, Wîk Örnya.

22 Au'wa is also used idiomatically in an abstract sense for any habitual action, e.g., a curious person is ka taiyín enkän wentá, literally, nose strong asking mad; hence ka taiyín enkän wenta au'wa is a person who habitually asks questions, a person with an insatiable curiosity.
But my informants added that the differences were trifling, and that it was in reality Wjk Monkan.

A myth tells that in the beginning a "little man" (päm mäny' mäny'), a little ghost, ŏrnyâ, lived at Ōrnyau'wa with a little woman. They were diminutive people; dwarfs, not babies. My informants declared that a ghost or spirit is always small at first, soon after death, but later it grows bigger. At first there were only these two people, the man and the woman; no others. They went hunting together, and they copulated and copulated.

waŋtkuntj24 tau'wâ ńäll i'ällâ put puţtâm wâmpull ark
Woman his spoke, "Let us go." Once again go back place
ńqink'ń kän wun waŋtj puŋka tän
shade (any leaf shelter). Lay down (the) woman knees raised.
pualla etjwo pualla ma'a yotâm etjind puḳ
Those two copulated; those two plenty times copulated. (A) child
wuṇpund waŋtkuntj ĭmpânâń ńul ĭ'i tîpând
begot (lit. placed). Woman his pregnant. Bye and bye was belly with
ta'ayî ńul puḳtâm
big, bye and bye child with.

This passage shows that even in the myth of Ōrnyau'wa, the relation between sexual intercourse and pregnancy is stressed.

After this the man and woman made the baby totem center.

ark au'wâ puḳkâk ńul yûmpân
Place totem center baby for bye and bye made.

When they had created the big swamp at Ōrnyau'wa they made a representation of the female genital organs, now associated with Pantiau'wa, the "sweetheart au'wa," as the natives speak of it today. Then they left their children in the big swamp, and the children played there and sunned themselves on logs as they came out of the water. Since, the totem center has sunk and they are no longer to be seen, but the place is still associated with babies and called puḳ au'wâ.

Every living member of the Ōrnyau'wa clan is represented by a tree that springs up in the totem center. This tree starts to grow as soon as a woman becomes pregnant, and continues to grow throughout the life of an individual. Among the species of trees that represent members of this clan are eucalyptus (Eucalyptus papuana, yûkkâ pûttâ), fig (Ficus opposita. yûkk kommâ), and milky pine (Alstonia scholaris, yûkk tantjil).

24 The suffix -kuntj denotes possession, his, belonging to him.
My informants cited an actual instance in which a tree associated with an Ornyau'wa man had been cut during his lifetime. The tree withered and gradually died; when it commenced to wither the man sickened, and as the tree dried up and slowly died, so the man declined and died too.

Pantia is a lover or “sweetheart.” If a yam stick is thrust into the part of the center that represents the vagina, sexual license (marîtji) spreads all over the country.

At the baby totem center at Ark Ornyau'wa increase rites (au’wā kent’n) were performed by men and also by women when they wished babies to go to other places. The center itself is said to have sunk out of sight under water, but the breaking off of the tops of termite mounds close to the totem center today constitutes the increase rite. As this rite is performed, the names of places to which it is desired to send babies are mentioned. Sometimes the names of individuals may be called, especially, my informants added, in the case of a woman who is marîtji, “too much run about,” and who will not remain with her own husband, in order to “make a big row come out,” i.e., in order to bring down vengeance upon her head.

DISCUSSION

The facts that have been presented form conclusive proof of the knowledge of the fact of physiological paternity among the aborigines of the Archer River district on the Gulf of Carpentaria. In a previous paper facts have been adduced that show that this knowledge is also shared by the people of the eastern seaboard of Cape York Peninsula.

On account of the controversial nature of the subject, and the fact that the knowledge of physiological paternity is in conflict with the findings of previous workers both to the north and south of this region, considerable care was exercised in the collection and checking of information and in the testing of informants. For this reason I have preferred to depend rather upon the evidence provided by spontaneous statements of the natives themselves made in their own language, especially as recounted in the text of myths, than upon statements in reply to direct questions, where it might be suggested that my own beliefs had influenced my informants.

The statement of the fertilizing power of semen and of its action in “building up” the body of the baby, and the revelation of the life-giving powers of seminal fluid that forms the theme of the Moon myth, are incompatible with ignorance of physical paternity.

Furthermore, the distinction in terminology in the kinship system between the physiological and sociological aspects of fatherhood is in itself

The Hero Cult (loc. cit.).
significant enough to place fatherhood in the Wjk Monkan tribe in a category very different from that recorded elsewhere in Australia; e.g., the statement of Sir James Frazer,26 after summarizing all the available literature on the subject,

to the European mind the tie between a father and his child is physical; to the Central Australian it is social. If we wish to avoid confusion in discussing the institutions of a race so different from our own, we must clearly distinguish between these two very different conceptions of paternity, the physical and the social, which we confound under the same name.

Nevertheless one cannot but feel that the ignorance of physical paternity was taken for granted by many of the early workers who had come under the influence of the "group marriage" beliefs.

But it is possible that one may be faced with the criticism that these people of Cape York Peninsula are not primitive in the same sense as the natives of Central Australia. Indeed my own work has been directed largely to demonstrating, by an analytic study of their social organization, that at least some of the tribes of Cape York Peninsula have been influenced very strongly by the infiltration of Papuan culture, and even to showing in what way they have reacted under its influence, and how each social institution has been modified by contact with it. It is well known, however, that Professor Malinowski, the most fervid advocate of the belief that ignorance of physiological paternity is a fundamental and primitive one among savage peoples, himself drew most of his evidence from a people who were culturally far more advanced than these natives on the threshold of the Australian continent, the people of the Trobriand Islands and the associated archipelago; a people who had an established village life, who made gardens and planted crops, and who not only bred pigs but castrated boars. Nevertheless, Malinowski concludes with the statement:27

My firm conviction is that the ignorance is an original feature of primitive psychology, and that in all speculations about the origin of Marriage and the Evolution of Sexual Customs, we must bear in mind this fundamental ignorance.

Such an ignorance appears to me to be quite incompatible with the facts now presented from a study of the Wjk Monkan people, and with those from the Koko Ya’o of the Lloyd Bay district, that I have recorded in an earlier communication. The life-giving properties of semen, demonstrated by Moon, who after offering his semen to others (all of whom refused and subsequently died, while he alone partook of it and survived to be reborn

27 The Father in Primitive Psychology (Psyche Miniatures, 1927), p. 93.
each month), seems to point to a knowledge of these facts, the fundamental nature of which cannot be denied.

So far the existence of the baby totem center has been omitted from this discussion. The myth of its origin is typical of those that explain the origin of the totem centers of these people. The fundamental fact revealed in the myth is the discovery by the totemic ancestors (the man and woman who made the totem center at Ornau'wa) of the fact that pregnancy resulted from sexual intercourse, and the importance attached to this is indicated by the fact that at Pantiau'wa the center representing the female genitalia is regarded as the most important of the small centers (au'wā mány') that compose the whole totem center. The increase rite, the thrusting of a yam stick or other implement into the symbolic vagina, is believed to give rise to babies in no more literal sense than that the increase rites performed at any other totem center give rise directly to animals. The belief is simply that a ritual state of well-being results, something like mana, and causes them to multiply.

The existence of this knowledge of physical paternity, which has a fundamental character, in this region lying midway between the areas of Central Australia and Papua, where such beliefs are said to be absent, presents some difficulties. The conclusion that seems to be inevitable is that the ignorance of physical paternity is not primitive but that, as Professor Carveth Read suggested,²⁸ where it does exist at the present time among primitive peoples, true knowledge has been masked by animistic beliefs, superimposed upon the primitive condition. I consider that the evidence now presented from the Wjak Monkan tribe is conclusive, and that if such knowledge is absent from more primitive people to the south, and from the more advanced peoples to the north, the facts admit of no other conclusion.

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²⁸ No Paternity (Journal, Royal Anthropological Institute, Vol. 48, pp. 146-54, 1918).
AN OUTLINE OF THE PROBLEM OF MAN'S ANTIQUITY IN NORTH AMERICA

By EDGAR B. HOWARD

STUDENTS of American prehistory have been engaged for many years in attempts to determine the length of time man has existed in the New World. In endeavoring to supply an answer to this question one becomes involved, not only in every phase of anthropology, but in geology, and palaeontology as well, since all play an important rôle in relation to early man in America.

The question of origin is no longer the controversial subject that it once was, for it is generally accepted that man represents a migrant to the New World from Asia where types nearly relative to him now live. When he came, however, depends upon a good many factors which can only be touched upon in a brief review of this kind. Recent discoveries may give the impression that this problem is approaching a solution, but one cannot make even a casual investigation without becoming convinced of its complexity, and discovering the enormous amount still to be accomplished before a clear picture of American prehistory is realized.

The literature on the subject is voluminous and records discoveries, made even prior to the middle of the last century, purporting to show that man had lived in America during the Pleistocene epoch. Such a discovery, which went unnoticed at the time, was made in Missouri by Koch\(^1\) in 1839 of stone points associated with the mastodon. A few years later, in 1846, at Natchez, Mississippi,\(^2\) a human pelvic bone was discovered in association with extinct animals, and in the same state of mineralization. In 1872 Abbott's announcement of the finding of what he considered to be Palaeolithic tools from glacial deposits near Trenton, New Jersey,\(^3\) stirred up a controversy that has never been satisfactorily settled. In 1879 Cope\(^4\) recorded the finding of human and faunal associations in Oregon, and Russell\(^5\) in 1885 reported a somewhat similar instance from Nevada. With these earlier discoveries we cannot concern ourselves very much, for, in most cases, we lack sufficient facts to determine their value as evidence one way or the other. In the light of present knowledge on the subject, however, it may not be out of place to have called attention to these discoveries. There were many others during the closing decades of the last

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\(^1\) Koch, Mastodon Remains in the State of Missouri.


\(^3\) Abbott, Occurrence of Implements in the River Drift; Spier, New Data on the Trenton Argillite Culture; The Trenton Argillite Culture.

\(^4\) Cope, The Silver Lake of Oregon.

\(^5\) Russell, Geological History of Lake Lahontan.
century of decidedly dubious character, and some statements that have no more to recommend them than the vivid imagination of a so-called antiquarian. In this way man’s origin in the western hemisphere has been connected at one time or other with every part of the world, including Egypt, Assyria, and even the Lost Tribes of Israel have been given credit for being the ancestors of the American Indian.

Nevertheless, the old controversy goes on: did man come to America in comparatively recent times, or was he here before the last glacial period? One school of thought has endeavored to place man as far back into the past as possible, while a small but resolute minority of anthropologists have maintained a balance at the other end of the scale, so that somewhere in between these opposing views one may eventually find the truth. It is not essential to the study of our problem that an absolute chronology be fixed, but it is necessary to strive for a relative chronology that will give us a clearer understanding of the sequences of the prehistoric complexes with which we are concerned.

The evidence as to when man probably arrived in the New World is of so varied a nature that it will be well to consider it under the main heads of anthropology, geology, palaeontology, and archaeology in order to try to give as complete a picture of the problem as possible.

Beginning with the first heading, one important fact stands out clearly, and that is, that no human skeletal remains have been found in America, up to this time, which differ from the Indian as Neanderthal man differs from Cro-Magnon man in Europe. Are we, therefore, justified in the assumption that man is a comparatively recent comer to our shores? When we speak of “early man” in America, what do we mean? Does this necessarily denote a low-browed, thick-skulled individual, with heavy brow-ridges, prognathous jaw, and other Neanderthaloid characteristics? So far, according to physical anthropologists, there have been no skeletal remains with such a combination of primitive characteristics discovered in the western hemisphere, and therefore, while evidence has been offered from time to time to the contrary, there has been no general acceptance of these claims.

On the other hand there seems to be nothing incompatible with the idea that man may have lived in America for a comparatively long time without having undergone radical somatological changes. In other words no human skeletal material has been discovered that has led physical anthropologists to believe that man in the New World was other than a modern type. While the underlying homogeneity of the American aborigines appears not to be questioned, it is, at the same time, recognized that
they show a great diversity of somatological characters. Whether this diversity is due to isolation into groups, where inbreeding occurred, or to adjustments to environment, or to differentiation having taken place before the earliest immigrants arrived, are questions that apparently have been controversial ones for a long time among anthropologists. Along this line Hooton\(^6\) says

I subscribe also to the pragmatic doctrine of the persistence of indifferent physical traits in inheritance rather than to the mystic creed of environmental miracle-making. The American race is a composite race, but I think it is composed of heterogeneous strains welded together by mixture, not of wonderfully adapted types made out of common clay by a creative environment.

Hooton,\(^7\) Dixon and others agree that the first immigrants to the New World were dolichocephals. To Hooton it appears that the earlier dolichocephals of the American population probably had a blend of Mediterranean, Negroid and an archaic white element, subsequently glossed over with Mongoloid traits due to mixture with other immigrants.

In any case we have evidence of dolichocephaly among the Basket Makers' whose remains stratigraphically are earlier than the Pueblos. There are also a number of cases where extreme dolichocephalic skulls have occurred under conditions indicating considerable age. The Lagoa Santa\(^8\) skulls from Brazil represent such a case. Extreme dolichocephals have been found in west central Texas,\(^9\) and along the Texas coast.\(^10\) The incomplete skull from Vero is according to Dr Hrdlička a skull with a cephalic index near the upper limits of dolichocephaly. The Punin skull\(^11\) from Ecuador is also dolichocephalic. The Minnesota skeleton\(^12\) reported in the last few years and an even more recent discovery—that announced by Figgins\(^13\)—are both narrow-headed. So that while the actual dating of these and other similar skeletal material remains doubtful, they do seem to bear out the impression that early man in America was narrow-headed, and that many of the skulls, although they appear to be of the same general type as those of the surviving Indians, show characteristics that are Australoid. Regard-

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\(^6\) Hooton, Racial Types in America, p. 162.
\(^7\) Hooton, Notes on Five Crania, p. 161.
\(^8\) Hrdlička, Early Man in South America.
\(^9\) Hooton, Notes on Five Crania.
\(^10\) Woodbury and Woodbury, Prehistoric Skeletal Remains.
\(^12\) Jenks, Minnesota Pleistocene Homo.
\(^13\) Figgins, New World Man.
ing this question Sir Arthur Keith\(^1\) thinks the Punin skull shows unmistakable Australoid affinities.

However, it seems to be generally agreed that man came to this continent from Asia, and that earlier migrations, at least, came gradually in small waves or groups. To what extent they were already differentiated in appearance, custom, and language seems difficult to say. There is no question that whatever the differentiation, there was further somatological, cultural, and linguistic diversification here at later times. Every kind of environment was encountered in the New World and it is easy to see the important influence this would have in the human adjustments that necessarily would follow, making it difficult to distinguish the original culture traits which have thus become obscured. Some fourteen or fifteen geographical areas have been recognized in the New World and the cultures of these areas intensively studied, so that out of these studies one is able to form an idea of what were the underlying cultural traits of the earlier immigrants. Culture diffusion and culture sequences play an important part in formulating opinions regarding origin and time relationship of the various American tribes. There are those who subscribe to an independent development theory, to which a number of serious objections have been pointed out by Wissler.\(^2\)

Without raising questions regarding theories of culture origins and their relations to migrations and chronology, all of which is ably treated in Wissler's "American Indian"\(^3\) and other works, we can see what a study of this kind will lead us to, so far as it bears upon our problem. First of all, we find that, as with respect to somatic factors, and notwithstanding a great diversity, culture traits of the New World show signs of unity in both a material way and in various conceptions of myth, ritual, and so on. But these cultures of the New World appear to have been developed without direct contact with the higher cultures of the Old World. In other words the higher centers of culture in both continents were isolated, and in between, it appears, were great regions of nomadic hunters. As Wissler\(^4\) points out:

We have found the great centers in Mexico and Peru to be not really unique growths but to possess many of the fundamental traits common to the wilder folk in the marginal areas of both continents. New World culture is thus a kind of pyramid whose base is as broad as the two Americas and whose apex rests over Middle America.

\(^1\) Keith, New Discoveries Relating to the Antiquity of Man.
\(^2\) Wissler, The American Indian, p. 375 et seq.
\(^3\) Wissler, op. cit.
Kroeber\textsuperscript{18} gives a diagram, showing geographical distribution of some of the more important elements of culture, and the probable sequences in each area. He thus pictures the first migrants as a people living on wild products; for weapons he suggests the bow, harpoon, and atlatl, as well as clubs, darts, spears, and perhaps daggers of bone or stone. He further lists chipped and flaked stones and others that were beginning to be ground or rubbed into shape; bone awls for piercing, perhaps eyed needles for sewing; twisted cords; and twined or woven baskets and nets. It also seems likely that the dog was brought to America and not domesticated from any wild species here. This sort of evidence Kroeber\textsuperscript{19} believes indicates that man's entry took place in the closing stages of the Palaeolithic or early in the Neolithic. He further points to the fact that these early migrations were unlikely to have been much later than the beginning of the Neolithic since cattle, pigs, sheep, wheat, barley, rice, millet, and the invention of the wheel would otherwise have been introduced into America. The origin of these traits is considered to be ancient, and therefore, as Wissler\textsuperscript{20} says:

make a strong case for the peopling of the New World either at a very remote period, or by wild tribes only, such as might arise by contact between the historic tribes of Alaska and Siberia.

Wissler\textsuperscript{21} gives the following as some of the probable traits brought in with the original emigrants from the Old World: the fire-drill, stone chipping, twisting of string, the bow, throwing stick, the harpoon, simple basketry and nets, hunting complexes, cooking with stones in vessels of wood, bark, or skin, body painting and perhaps tattooing, and the domestication of the dog. Some of these he points out may have filtered through Alaska from time to time.

Considering that the languages of the New World lack evidence, outside of Eskimo, of any identification with Old World languages, the conclusion appears to be that human contact between the two continents was very remote.

When we bring our focus up to the Basket Makers of the Southwest we find them without pottery, making coiled baskets instead, using the spear-thrower, and with some of the other traits already mentioned, which probably extended back to earlier times. As to their origin, it was given a

\textsuperscript{18} Kroeber, Anthropology, p. 340.
\textsuperscript{19} Idem, p. 344.
\textsuperscript{20} Wissler, The American Indian, p. 392.
\textsuperscript{21} Idem, p. 399.
tentative dating by Kidder\textsuperscript{22} of from 3,500 to 4,000 years ago. Roberts\textsuperscript{23} more recently suggests a time for their appearance in the Southwest nearer the present: 1,000 B.C. to 1,500 B.C. The inclination of late years has been to regard the development of both the Basket Makers and the early Pueblo stages as having taken place more rapidly than was formerly thought to be the case. At any rate, though the tendency is now not to stress too strongly actual dates prior to those which dendrochronology is establishing, there can be little doubt that the Basket Makers were already cultivating corn at the beginning of the Christian era. How long it took to develop the type of maize found in Basket Maker caves in Utah and Arizona is problematical, but several centuries must be allowed. A hunting stage Basket Maker preceding these early agriculturists is logically postulated, so that we may say conservatively that we lose sight of the beginnings of Basket Maker culture somewhere around 1500 B.C. Let us leave the anthropological discussion of our problem at this point and see what light geology and palaeontology can throw on it.

One difficulty in developing any sort of chronological relationship for a period antedating the Basket Makers is that there is a general lack of understanding of the use of such terms as “post-Glacial,” “Recent,” and so on. Unfortunately these words convey different meanings, according to the experience of the individual concerned with their usage, so that, for example, the beginning of post-Glacial time may mean as much as 25,000 years ago to one geologist and 10,000 to another. Whereas “Recent” may apply to a subdivision of post-Glacial time, by others it may be considered to be synonymous with it. A definitive solution of these difficulties will probably not be found till much more work of a co-operative nature is accomplished. However, for purposes of this paper we shall adopt here what we believe to be the best and most generally accepted interpretations, those given by Dr Ernst Antevs.\textsuperscript{24} He makes the “Recent” a part of post-Glacial time, allowing 8,500 years as the time elapsed since the beginning of the post-Glacial, or that time, as he considers it, when the temperature became approximately as it is today, and allowing 4,000 years since the beginning of the Recent.

Obviously these questions are of great importance in relation to any migrations that may have taken place during these early times. We must assume, as we have done, that there is no longer much doubt that man came originally to America from Asia, at the same time realizing that there

\textsuperscript{22} Kidder, An Introduction to . . . Southwestern Archaeology.
\textsuperscript{23} Roberts, Early Pueblo Ruins in the Piedra District.
\textsuperscript{24} Antevs, Late-Glacial Correlations and Ice Recession; Climaxes of the Last Glaciation.
are those who postulate a trans-Pacific migration in large war canoes; still others who account for man in the New World by calling upon the existence of a continent, now sunken; others who call attention to the comparatively easy access to North America by way of Iceland and Greenland. As to the first of these speculations, there is no denying the abilities of the Polynesians as navigators, but the evidence for more than a casual boat-load to have made a passage of such extreme length across the Pacific appears to be very unlikely. Furthermore, Polynesian diffusion seems to have been too recent to account for migrations in late Glacial or early post-Glacial times. As for the second, there is geological evidence that a sunken land mass once connected South America and Africa, but there is no evidence that this connection was not broken long before even the Java man came upon the scene.

We believe that some Bering Strait route furnishes the most readily acceptable theory of a route of migration. There is evidence from many different angles that man came to this country from Asia by this route. not the least of which is the geological evidence. The Yukon\(^{25}\) seems not to have been glaciated during the last glacial period, an important thing to note in connection with the movement of peoples, since ice would act as a barrier to man and some of the animals he hunted. So far as northeastern North America is concerned conditions could not have been as favorable until later times, since the ice sheet lasted much longer than in the western part of the country, and would therefore have acted as a formidable barrier to migration. To return to the more likely route in the northwest, it should also be noted that recent investigations in northeastern Siberia\(^{26}\) reveal a condition there similar to that in Alaska during the last glacial advance. The evidence points to the Anadyr River having been unglaciated, and to there having been no continuous ice sheet, only mountain glaciers in that part of Siberia. Thus the geological factors place no serious obstacles in man’s path from the northwest to establish himself in a new continent at the close of the last glacial period. By the beginning of post-Glacial times he would have found it possible to have reached our Great Plains regions from a northwesterly direction through ever-widening corridors, the receding ice sheet on one side and mountain glaciers on the other. At a somewhat earlier time, during the maximum of the Wisconsin glaciation, the ice sheet in Canada would have discouraged migration, since we have no proof that these early people were adapted to an arctic environment as

\(^{25}\) Johnston, Quaternary Geology in North America; Smith, Geographic and Geological Evidence.

\(^{26}\) Obruchev and Salishchev, The Mountain Systems of Northeastern Asia.
were the later Eskimo. In regard to such a route Antevs\(^{27}\) says in part:

He [man] came from Northeastern Asia to Alaska and probably spread along the eastern foot of the Rocky Mountains where an ice-free corridor had formed some 20,000–15,000 years ago. He seems to have reached the Southwest at the age of transition between the pluvial and post-pluvial epochs, roughly 12,000 years ago.

He further notes:\(^{28}\)

As was pointed out by Johnston, the earliest possible, i.e. late-Glacial, land route from unglaciated Alaska to Central North America led eastward to the Mackenzie and thence southward along this river and the eastern foot of the Rockies. This explains why most of the significant finds of earliest man in the United States have been made on the Great Plains. The trail was probably opened 20,000–15,000 years ago. Possibly this route was also open for a short time between the first and second expansion of the Keewatin ice sheet 45,000, 40,000, or 35,000 years ago.

One of the chief factors in differentiating earlier sites from Basket Maker or later ones has been the association of extinct animal bones with artifacts unlike Basket Maker types. Discoveries of this kind have been made in too many well-authenticated cases in recent years not to be accepted as valid. This, however, has not, up to this time, helped us very much to date these occurrences with any degree of accuracy, since we do not know when the animals became extinct. Did they survive the last glaciation, and live on into post-Glacial times, or did they die out during the Pleistocene? It is not known whether the types of animals reported in associations with human remains and artifacts became extinct simultaneously in various regions of the New World, or whether some lingered on, in certain places, longer than in others: it seems reasonable to suppose that the latter was the case.

What caused the extinction of the larger forms: was it due to changed climatic conditions, food shortage, disease, dust, man, or one or more of a number of other causes that may have upset the natural equilibrium existing previously? Whether correct, or not, climatic change has been most generally thought to have played the chief rôle in this extinction, but until we are more familiar with the stratigraphical relationships of these extinct animals in geologically known deposits, it will be difficult to make use of their associated remains as chronological criteria. However, one thing that seems to be definite is that no extinct animal remains have been found associated with the remains of humans as late as the Basket Makers.

\(^{27}\) Antevs, The Spread of Aboriginal Man to North America, p. 302.

\(^{28}\) *Idem*, p. 306.
Romer\textsuperscript{29} makes the statement that the association in America of man with certain mammalian types no longer living is unquestioned, but that such contemporaneity does not indicate any remote antiquity. He finds almost no palaeontological evidence that man was here at a time earlier than the withdrawal of the last Pleistocene ice sheet.

We must now consider the archaeological factors that enter into the problem, but before doing this it will be well to look into what is meant by the use of the words "Folsom point," "Yuma point," and "Folsom-like point"—words that have come into use in describing early types of American stone points. There is general agreement among those informed as to what characteristics are combined in the typical Folsom point, but there has been no clear understanding regarding the Yuma and Folsom-like points. This may be due to the fact that it is hard to say what might be chosen as the type in each case because of the number of variants. For example, in the case of the latter, the writer has called them "Folsom-like points" for the reason that it seemed important to show that in some way they are related to the true Folsom point (though not necessarily in point of time). The longitudinally flaked grooving of the true Folsom blade is a specialized technique, which, as it will appear later, has a distribution limited so far to the United States and Canada. There are Folsom-like points from most of the United States, varying in size, shape and form, but with the specialized grooves. There is little evidence as to their age, and though in the west they have been found with extinct animals, in the rest of the country they represent surface finds. Therefore, it seemed to the writer that in our present state of knowledge regarding all these types it would be best to form three family groups, as mentioned, without trying to refine the classification till more is known of the relationships. Otherwise, in attempting to describe the various subtypes, it will be found that there are variant types indistinguishable from those found in recent campsites. For example, there have been found both at Clovis and the Lindenmeier site true leaf-shaped blades without any grooving that undoubtedly belong to the Folsom complex, but if we call these Folsom points, then the whole classification falls down. It would appear better not to anticipate too much, but to be satisfied with three main groups recognized in a tentative way. When there is more information and more evidence as to what are contemporaneous, or what the stratigraphic relationship is, it will be time enough to try to pigeon-hole all the variations. In the meantime a "Folsom complex" is beginning to be recognized, but it is important to understand that

\textsuperscript{29} Romer, Pleistocene Vertebrates and . . . the Problems of Human Antiquity.
some of the types of artifacts composing this "complex," such as the snub-nosed scrapers and the small, leaf-shaped, flaked points without grooves, are hard to distinguish from similar types belonging to different horizons in other parts of America and other parts of the world.

We refer to the first group,\(^{20}\) then, when the points have all the characteristics of the type found near Folsom, New Mexico. These are thin leaf-shaped points with a longitudinal grooving of each face (sometimes only one, depending upon the original thickness of the flake) and with very fine secondary chipping and a concave base, with ear-like projections. The Folsom-like\(^{21}\) types also exhibit the grooving technique, made by the removal of a spall from a prepared base, but these types are usually larger and the flaking cruder. Usually the base differs somewhat in not being so deeply concave in relation to its length. There seem to be a number of variants of this type, both as to size and outline; one having the sides constricted above the base and then flaring to form a sort of fish-tail. The distribution of the true Folsom point seems to be more or less restricted to southeastern Wyoming, southwestern Nebraska, southwestern Kansas, western panhandle of Oklahoma and Texas, eastern half of Colorado, and eastern New Mexico. There are isolated finds from other places in these states and one or two adjacent states, but most of them are from the High Plains region. The distribution of the Folsom-like point, on the other hand, is very widespread, covering most of the United States and parts of Canada. As already stated, these represent, with few exceptions, surface finds. Whether they represent a later, degenerate type of point than the true Folsom or an earlier one has not yet been established satisfactorily, but the two types will undoubtedly be found to be related in some way to each other. The third type or Yuma point\(^{22}\) has a distribution that seems to coincide with that of the true Folsom, the greater number having been found in Nebraska and in the vicinity of Yuma, Colorado. This type exhibits in its best examples the same control of the flaking tool as the Folsom, though the shape and cross-section are different. It occasionally has a narrow spall removed longitudinally along each face; the sides are parallel for some distance from the base, which is squared; the maximum width is either at the base or slightly above, where a shoulder exists. Often this shoulder is only a slight indentation, below which, to the base, the edges appear to have been purposely smoothed for hafting. There seem to

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\(^{20}\) Howard, Evidence of Early Man in North America.


\(^{22}\) Op. cit.
be a number of variations, particularly as to length and to the fineness of the chipping. Some of the best types exhibit narrow, ribbon-like flutings that extend diagonally clear across the faces. In cross-section, instead of being bi-concave like the Folsom point, they are either elliptical, or where the flaking does not extend all across the face, the cross-section will be more diamond-shaped, because of medial ridges on the faces. These particular types have been described in detail by Renaud,23 and more recently by Figgins.24 There seems to be some disagreement as to just what constitute the distinguishing characteristics of the cruder forms, some of which are hard to differentiate from other types found in the same general region. It should be noted here that no Yuma types were found at the Folsom site, nor have any been reported from the Lindenmeier site by Roberts.25 From the Clovis site26 the writer has figured several specimens that were called Yuma points but at the same time it was suggested that, in his opinion, most of these were probably knives of the "Folsom complex" and not really Yuma points. This conclusion can be more confidently expressed at this time. However, one point found at Clovis27 resembles a Yuma type, but has a narrow longitudinal groove from the base along the face.

It may be significant that so far the distribution of the true Folsom and Yuma artifacts occurs in the Great Plains province. In fact, with some exceptions, the distribution can be narrowed down to that part of the province in which are the headwaters of the larger river systems draining east into the Missouri and Mississippi Rivers and southeast to the Gulf, such as the White, the North Platte, the South Platte, the Arkansas, the Cimarron, the Canadian, the Red, and the Brazos. There is geological evidence that the latter, at least, headed much farther west towards the mountains at a not very remote period.28

The Folsom-like points on the other hand are not only found in the same region but have a very much wider distribution, extending all the way to the Atlantic seaboard and apparently centering in the Mississippi Valley—Ohio, Illinois, Indiana, Missouri, Kentucky. They seem, so far, to be exceptionally rare from the Pacific coast and are for the most part from the eastern side of the Rockies, with a limited number coming from the Basin and Range province, in Nevada, Idaho, and other places. Outside of

23 Renaud, Yuma and Folsom Artifacts; The First Thousand Yuma-Folsom Artifacts.
24 Figgins, Folsom and Yuma Artifacts.
26 Howard, Evidence of Early Man in North America.
27 Idem, Pl. 34, No. 4.
North America and Canada, none has been reported from the rest of the western hemisphere up to this time. There are apparently none in the collections in Mexico City and none have been found in Central America. No grooved points of the Folsom and Folsom-like types have been discovered in Europe or Africa, nor are there any in collections from Siberia so far examined. Points of slate with rubbed grooves occur in Alaska among recent peoples. Similar types have been found in Manchuria dated as of the pre-Han period, but these exhibit an altogether different technique in manufacture, though the purpose may have been similar. Therefore this looks like a North American development. If Wissler’s map of the Plains Indian culture area be contracted somewhat on the east, west, and north, it will coincide well with what is, at present, the distribution of the Folsom and Yuma points. Can it be inferred that, because the Folsom-like point has a wider distribution than the true Folsom, it is, therefore, relatively earlier? It is certainly not so well flaked as the Folsom, but there seem to be more variations in size and shape.

We may now consider some of the more recent discoveries relating to man’s antiquity in America. We list these under the heads “cave finds” and those at “open sites.” We are including here only those caves with indicated associations with man, and not the larger number of caves in which Pleistocene faunal assemblages alone have occurred. In the former category we may include: (1) Conkling Cave, New Mexico; (2) Gypsum Cave, Nevada; (3) Burnet Cave, New Mexico.

In 1930 as the result of preliminary work on the part of Roscoe P. Conkling an expedition from the Los Angeles Museum excavated a cave in a spur of the Organ Mountains about fourteen and a half miles southeast of Las Cruces, New Mexico. The mouth of the cave is approximately fifteen to eighteen feet above the present valley floor and after excavating was completed the walls extended down almost vertically to a floor level with the valley floor outside. About twenty-four feet below this was a layer of cemented sand that sealed off the cave completely from above. Below this bones of camel, horse, ground-sloth, antelope, wolf, and California condor were found, as well as parts of human skeletons. Though there is no evidence

39 Hrdlička, personal letter.
40 Photographs sent to the author by A. S. Loukashkin, Museum of North Manchuria, Harbin.
41 Wissler, The American Indian, p. 221.
42 Conkling, The Discoveries in the Bone Cave.
43 Harrington, Gypsum Cave, Nevada.
44 Howard, Evidence of Early Man in North America.
of another entrance, it is probable that the fractured limestone slightly above the "sandstone seal" conceals a horizontal opening now below the valley floor.

The human skull found here appears to be narrow-headed, but otherwise not noteworthy. Human bones were apparently found at several levels, the lowest roughly fifty feet below the floor level at the time of discovery. Human bones as well as those of camel, horse, bear, and ground-sloth were found above the "sandstone seal" as well as below. Some of the sloth skeletons were found in articulated positions. It seems difficult to escape the conviction that these associations furnish evidence of man's contemporaneity with extinct animals. The dating of these discoveries, however, can be only a guess at the present time.

Gypsum cave, located about sixteen miles east of Las Vegas, Nevada, was excavated by M. R. Harrington of the Southwest Museum of Los Angeles in 1930, though preliminary investigations had been made by him previously. The principal association here was evidence of man with remains of the ground-sloth, though other animals, including camel, horse, dire wolf, and the California condor, were also discovered. No human skeletal remains were found, but much evidence of human occupation, such as charcoal and ash, burnt sticks and cane, besides stone artifacts. No question has been raised as to these associations.

The third cave mentioned, Burnet Cave, is located about fifty miles west of Carlsbad, New Mexico, in the south fork of Rocky Arroyo. This cave furnished evidence of use as a burial chamber by a people probably related in some way to the Basket Makers, since the objects recovered showed striking similarities to those of the San Juan region, except for the sandal types and the fact that the burials represented cremations. No corn and no pottery were found. Below this burial level, which did not extend over three feet, lens-shaped deposits of charcoal and ashes were found at many different places and various levels throughout the cave as deep as eight feet six inches. Also below the burial levels and in some cases associated with the "hearth" were bones of animals, including camel, horse, antelope, bear, bison, caribou, and musk-ox-like animals. Of man-made objects recovered, a Folsom-like point was found at approximately four feet below the burial levels. This grooved point was directly associated with bones of one of the musk-ox-like animals, and with a hearth. No ground-sloth was found in this cave.

These three caves are the most recently investigated, but earlier ones are equally important. To mention one, the evidence brought forth by the ex-
cavation of Potter Creek Cave, California, may well be reconsidered in the light of results of the caves briefly described.

Dr John C. Merriam very properly pointed out, shortly after the discovery of Potter Creek Cave, the difficulties in the way of pronouncing as man-made certain bone objects recovered there. Later, however, these bone objects were submitted to a number of anatomists who agreed that certain peculiarities were not natural. In a statement by Matthew and Gidley the view was endorsed without question that the specimen submitted showed the handiwork of man. In view of the confirmation of such an association by evidence of a similar nature in recent years, is there any reason why this discovery should not be accepted along with the rest?

Now to turn to the second group of recent finds — those at open sites as distinguished from caves — we may mention first the site near Folsom, New Mexico, which deserves to rank at the head of the list of recent discoveries, though it is already over ten years since residents of Raton, New Mexico, first brought their discovery to the attention of J. D. Figgins of the Colorado Museum of Natural History, Denver. This led to investigations during the next few years on the part of this museum and of Barnum Brown of the American Museum of Natural History, New York. Not far from the little town of Folsom some thirty skeletons of an extinct bison were found under eight to twelve feet of restratified deposits, associated with nineteen or so spear points of a specialized type that has come to be called the "Folsom point," as already described. This discovery has come to be generally accepted, and has been responsible for other discoveries of a similar nature in the general region.

Next may be mentioned the Clovis site where not only Folsom spear points were found, but also knives, end and side scrapers, and small flakes, probably for use as gravers, though not of the "burin" type. These, found first in 1932 by A. W. Anderson of Clovis, are from basins that at one time may have represented part of the Brazos River system. The wind has exposed these old basin deposits by blowing the overlying sands into dunes along the edges. Weathering out of the top of these beds, and below the surface where cuts have exposed them, are found bones of extinct bison, hearths containing charcoal and burned animal bones, and in a number of places, in

46 Merriam, Recent Cave Exploration in California; Sinclair, The Exploration of the Potter Creek Cave.
47 Putnam, Evidence of the Work of Man.
48 Cook, Glacial-Age Man in New Mexico.
direct association, flake knives, points, and side scrapers. In the same de-
posits also, at levels above the hearth and bison bones, were found elephant
remains. The site was carefully examined by a number of well-known scien-
tists, among others Dr John C. Merriam⁴⁹ and Sir Arthur Smith Woodward
before much of the evidence was removed from place.⁵⁰

The deposits also contain invertebrates and diatoms, studies of which
evidence climatic conditions for the time of deposition differing greatly
from those existing at present. Dr Ernst Antevs, who made a study of the
site, dates the deposition at from 13,000 to 12,000 years ago.

A third site, excavated last summer by Dr F. H. H. Roberts, Jr.,⁵¹ is
that near Ft. Collins, Colorado, known as the Lindenmeier site. Here again
the bones of an extinct bison were discovered in association with artifacts.
The stone artifacts included Folsom points and, in addition, knives,
scrapers, and gravers. They were found along an arroyo down to fifteen feet
below the surface. The work, carried on in most able fashion, produced con-
clusive evidence of these associations. No human bones were discovered
either here, at Clovis, or at Folsom.

It is not possible here, nor does it seem to be essential, to give detailed
accounts of all of the recent discoveries of a somewhat similar nature. Re-
ports have been made of these or are in process of preparation, so that we
shall do no more than mention them.

In Dallam County,⁵² Texas, W. E. Baker has discovered Folsom and Folsom-
like artifacts in basin deposits exposed by the wind, together with extinct animal
bones.

In Yuma County,⁵³ Colorado, Perry Anderson has made a large collection of
artifacts under more or less similar conditions. This site has given the name to the
Yuma type point.

Near Scottsbluff, Nebraska, C. B. Schultz,⁵⁴ under the direction of E. H.
Barbour of the Nebraska State Museum, has made some important finds of extinct
bison remains associated with artifacts under deep loess deposits.

In Weld County, Colorado,⁵⁵ near Dent, J. D. Figgins has reported on the
discovery of what appear to be Folsom-like artifacts in association with mammoth
remains at five and a half to seven feet below the surface.

Floyd V. Studer⁵⁶ of Amarillo, Texas, has reported on the association of an arti-

⁴⁹ Woodward, Recent Progress in the Study of Early Man
⁵⁰ Merriam, Early Man in North America.
⁵² Howard, Evidence of Early Man in North America.
⁵³ Renaud, Yuma and Folsom Artifacts.
⁵⁴ Barbour and Schultz, The Scottsbluff Bison Quarry.
⁵⁵ Figgins, A Further Contribution to the Antiquity of Man.
fact (ungrooved) with a mammoth found by J. A. Mead near Miami, Roberts County, Texas.

In the north A. E. Jenks, in the University of Minnesota, has given preliminary accounts of discoveries of what also appear to be Folsom-like points found among skeletal remains in a gravel pit.

The work being carried on in the Pinto Basin by Mr and Mrs W. H. Campbell, in connection with the Southwest Museum of Los Angeles, that at Scottsbluff, Nebraska, by W. D. Strong, and that by E. B. Sayles near Abilene, Texas, under the direction of Harold Gladwin of Globe, Arizona, is furnishing evidence of great importance regarding early man in America.

The discoveries at Vero, Florida, reported by E. H. Sellards, and those at Melbourne, Florida, by J. W. Gidley, although somewhat earlier than those mentioned, must not be overlooked. In the light of more recent work the evidence furnished at these two sites deserves reinvestigation, particularly since these discoveries furnished a most interesting fauna apparently similar to that from the west, where associations have been established with objects of human manufacture.

The latest report, as this is being written, is from E. B. Renaud of the University of Denver, to the effect that crude stone tools resembling Chellean, Acheulian, and Mousterian tools of Europe have been found on what appear to be ancient river terraces. What this will do to throw light on early man in America cannot be determined; but anthropologists and archaeologists will await with interest the geological report upon the terraces.

Discoveries near Abilene made by Ray and Sayles, and tentative reports from MacClintock, Barbour, Schultz and Lugn as to artifacts in Nebraska, under clays and loess indicating pre-Glacial deposits, are focusing attention upon the possibilities of man having been in America considerably longer than the time assigned to those who made the Folsom artifacts.

Thus we begin to see a pattern gradually taking form, as though we were endeavoring to fit together the pieces of a gigantic picture puzzle, one end of which rests in Siberia and the other in the Great Plains of North America. As we put the picture together as far as it now goes, and considering that some of the pieces have probably been lost, let us see what we can make of what is left.

57 Science News Letter, Bones and Dart Points.
58 Campbell and Campbell, The Pinto Basin Site.
59 Strong, Signal Butte, A Prehistoric Narrative.
60 Sayles, An Archaeological Survey of Texas.
61 Sellards, Human Remains and Associated Fossils.
63 Science News Letter, When Did Early Man First Reach America's Shores?
64 Ray, Archaeological Research in Central West Texas.
65 Sayles, An Archaeological Survey of Texas.
66 Paper read at the St. Louis meeting of the American Association for the Advancement of Science, 1936.
First we see that there is depicted a basic hunting people, and while we cannot tell exactly what these early people looked like, as there is a gap here, it will probably turn out that they were narrow-headed, with Australoid and other non-Indian characteristics but still recognizable as American Indians.

Second, we see further a group in our Great Plains, particularly along the east side of the Rockies near the head-waters, or what once were the head-waters, of some of the larger rivers, who left there beautifully flaked implements that have come to be called Folsom points. The flaking of these and the so-called Yuma points is unequaled anywhere, except, perhaps, in the Danish and Egyptian Neolithic daggers. They have also been compared to the European Solutrean laurel-leaf points, but it is doubtful whether the comparison is valid since the American points exhibit a finer chipping, as though made by a people who had been familiar with the technique for a long time, but by comparison were in a later stage of development.

Third, the people whose skeletal remains have not yet been found, or at least not yet recognized as belonging to the makers of these artifacts, evidently hunted animals that have been extinct in that region for some time.

Fourth, we cannot see, in our picture, when these animals died out, but it looks as though it were some time after the last glacial advance. Some ininterpret this, in years, at from 10,000 to 15,000 years ago. Geological evidence points to no obstacle at this period in the way of these early hunters migrating from Siberia into our Great Plains region.

Fifth, we see further that we must not narrow down too much the margin of time for the development of our native cultures or else it will be difficult to account for such diversity of language, culture, and somatological characters as existed.

Sixth, we can make out that our "Folsom man" lived at an earlier time than the Basket Makers of Utah and Arizona, and that the Basket Makers lived before the Pueblos, so that we have this much of a sequence. It seems likely that some of the more recent finds, such as those at Gypsum Cave, will fit into the gap between the first two mentioned, but our picture is somewhat blurred as to this gap. It is decidedly blurred with regard to those people who may have existed prior to our so-called "Folsom Man," who is recognized, so far, only by the tools he made. These he left scattered about old river banks and lake beds of our Great Plains when in search of animals who came there to seek water.

In brief, therefore, we may conclude, taking all the evidence from

67 Martin, Études sur le Solutréen.
68 For about the best summary of the problem, see Nelson, The Antiquity of Man in America.
anthropology, geology, palaeontology, and archaeology, that man lived in America at least as long ago as the closing stages of the last Glacial epoch or the beginnings of post-Glacial times, which conservatively may be taken as an antiquity of ten thousand years. That he may have lived here earlier cannot be denied, but the evidence is not yet ample enough to prove the case, so we rest it here.

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A SURVEY OF THE WOOLEN TEXTILES
IN THE SIR AUREL STEIN COLLECTIONS  By LILA M. O'NEALE

SIR MARK AUREL STEIN spent the years 1900–01, 1906–08, and 1913–15 in Chinese Turkestan. On the first expedition he investigated the Khotan region over an area which stretches in a direct line for more than three hundred miles from west to east. He published a detailed report, "Ancient Khotan" (Oxford, 1907), in two quarto volumes in addition to a personal narrative, "Sandburied Ruins of Khotan" (London, 1903).

Stein’s second expedition explored from the uppermost Oxus to the western boundary of China with the Tarim basin as the main field of operations. "Serindia" (Oxford, 1921), the detailed report of this undertaking which covered two and a half years and ten thousand marching miles, fills three quarto volumes of text, almost 1600 pages, one volume of plates, and one of plans and maps. Prior to the publication of "Serindia," Stein finished his second personal narrative, "Ruins of Desert Cathay" (London, 1912).

The third time Stein set out from Kashgar, he crossed six hundred miles of country including the whole length of the Taklamakan desert to reach the district around the extreme easternmost depression of the Tarim basin. Results of his explorations of ruined sites and ancient trade routes in the Lop area are embodied in four quarto volumes, "Innermost Asia" (Oxford, 1928), two of text, one each of plates and maps. The preliminary account of these explorations appeared in the paper, "A Third Journey of Exploration in Central Asia," 1913–1915.¹

The Stein Collections of Central Asian antiquities include a wide variety of specimens, thousands of ancient documents, textiles, household furniture, and implements of unknown use. The finds comprise examples of arts and crafts from the first century B.C. to the eleventh century A.D. A very brief paper on the silk relics recovered from a Lou-lan site in the Lop desert has appeared.² It is natural that these earliest surviving examples of a great industry should excite the most enthusiasm and first interest, since the materials other than silk were generally put to homely uses, and were less frequently patterned. Under any circumstances analyses of the Stein Collection objects will be difficult. The Indian Government arranged for the division of the finds between the Imperial Museum of Delhi and the British Museum. It is all the more fortunate, in view of this fact, that Stein should have interested Mr. Fred. H. Andrews in compiling the detailed Descriptive List.


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of antiquities which regularly follows each record of an excavated site. Although English textile terminology differs in minor matters from our own, there is little doubt that his analyses provide a sufficiently safe basis for a general survey of Central Asian textiles. I propose in this paper to summarize what may be known of the early wools of Chinese Turkestan from the fabrics in the Stein Collections. In so doing I shall organize the available material upon a chronological basis.

Wool is now and must always have been of utmost importance to the ever-shifting peoples of Central Asia. Felted material is both a necessity and a medium through which the aesthetics of its makers are expressed. At Khotan there are great stores of felt rugs, or numdahs. The making of numdahs is an ancient industry as evidence from third and seventh century sites proved. Khotan and Kashgar have long been famous for their products according to the Annals of the Former Han dynasty. Chinese supremacy in the form of political control was effective in Central Asia during the Han dynasty (206 B.C.—220 A.D.), and later was reasserted during the T'ang dynasty (618—907 A.D.). Khotan, situated in an important position on the ancient trade route from China to the Oxus basin, was destined to receive influences from both East and West. Kashgar, referred to as a “market for goods,” was on the Chinese route to the West through Sogdiana and Bactria.

The “Travels” of Fa-hsien is the earliest extant record of a journey to India. The Buddhist pilgrim, who visited the Khotan region about 400 A.D., mentioned the materials of dress at Kashgar in speaking of the different kinds of fine woolen cloth (some translators render the word as “felt”) and serge. “Apart from these,” he wrote, “the dress of the common people is of coarse materials, as in our country of Ch'in.” Stein's excavations brought to light actual fabrics which antedate by centuries those mentioned in the traveler's few sentences. Yet even these finds cannot be said to represent the early developments of wool as a textile fibre in Asia, since the silk fragments found in the same sites with the wools are achievements demanding highly specialized knowledge and experience in pattern weaving. Whether remnants of ancient materials more nearly comparable to archaic types will be found cannot be prophesied. Stein evidently doubts the possibility when he says,

... it is true, we can scarcely hope ever to obtain a sufficiently close knowledge of the development of ancient weaving industries in Central Asia and China to derive chronological indications from the manifold fragments in silk, wool, and felt which the Descriptive List below shows.  

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3 A special index in the reports lists every mention of each specimen found.
4 Serindia, Vol. 1, p. 221.
ANCIENT BURIALS

At most of the sites excavation brought to light inscribed and dated tablets, but at other sites Stein estimated the relative chronological position of his finds. Outside the enclosing wall of a small fort built of clay slabs on the Mesa L.F. near Lou-lan he discovered an undisturbed cemetery containing three graves. The finds even lacking exact dates are of considerable interest because this small and seemingly unimportant site produced the first well-preserved body unearthed during Stein’s several journeys into Central Asia. Secondly, nothing about the appearance of the bodies, of a racial type “distinctly non-Mongolian,” the manner of shrouding them for burial, nor the actual materials used suggests that Chinese influence, as typified by the fortified post, had made any real impression upon the people living there. Stein says the appearance of the men’s bodies corresponds to the descriptions of the people of Lou-lan to be found in the Han Annals. They were undoubtedly the “common people” of the region at the beginning of our era, of whom centuries later Fa-hsien wrote. One coffin contained the body of a man, bare except for a kind of loin cloth made up of dark brown tassels of wool. The shroud was of coarse but strongly woven woolen material. The head was covered with a brown felt cap with angular ear-flaps. The whole body, except for face and feet, was enveloped in coarse canvas, apparently of hemp.

The graves found at Site L.S. in the Singer district are believed to represent burials coeval with those at Mesa Site L.F. Coarse cloths of wool and goat’s hair together with felts were recovered. One wool is dyed terra cotta (L.S. 1.01); and a coarse dark brown goat’s hair fabric, plain weave, is “ornamented with occasional inwoven threads of red and yellow” (L.S. 2.06). Grave L.S. 3 contained the body of a woman.

A plain close-fitting felt cap tied under the chin with strings. The face cloth was made of strong plain-weave woolen fabric, yellowish, with a twisted fringe and narrow ornamental line of brown wool inwoven along the top of the fringe. For a width of 1 1/4" within fringed border, double shoots of weft. Selvage along one side. Well made. The body was shrouded in hemp (?).

DATED FABRICS

An account of datable woolen fabrics in Central Asia might begin in either the west or east portions of Turkestan. An abundance of textile remains was recovered from the rubbish heaps found at the long chain of ruined watch stations along the ancient Chinese frontier wall in the exceptionally arid desert of Tun-huang, in the east. The wall, or Limes, was constructed during the closing decade of the second century B.C. to protect
caravans against the raids of the Huns, and to safeguard the greatest of the ancient trade routes from China. Small detachments were sent out to man the towers. Documents and records of all sorts in the form of wooden slips were recovered from the *Limes* ruins and rubbish heaps. The earliest of these records, the oldest extant Chinese manuscripts known, have clearly written dates corresponding to 98, 96, and 94 B.C. Since no documents dated later than the middle of the second century A.D. have been found, the whole line of wall and watch towers must have been abandoned to the desert soon after that date.

Silk scraps predominate among the numerous fragments found in the deserted soldiers' quarters and within the rubbish heaps usually present at the base of each tower. The general Chinese style and treatment of the silks is to be expected of examples from the Han dynasty industrial centres. Materials are described by Andrews as being "finely woven." Fabrics other than silks are less often represented although the Administration of the *Limes* issued tunics, vests, and shoes to the men.

The woolen textiles, few by comparison with the silk and linen finds, are, too, less noticeable because rarely patterned. Andrews' descriptions of them are brief. He characterises the various qualities of woolen fabrics as "fine canvas" or "coarse canvas," presumably meaning plain weave materials of more or less standard texture. He lists one fragment as "fine," four as "coarse." Almost all the plain woolen fabrics found at the towers are natural buff or yellowish. One piece is dark brown, and the very simple patterning of another has been described as "a stringlike woof on double warp; buff, with occasional lines of dark brown" (T. XXII. c. 002). One fragment (T. XLI. b. 018) is woven in twill.

Two fragments of woven wool rugs were discovered. One (T. XLIII. a. 06) is dyed green, the other, a tapestry (T. XXIII. b. 03), is in "rainbow stripes" in red, green, and brown, a conventional arrangement occurring frequently among the textiles of Lou-lan. The single example of pile weave carpet from the watch towers was constructed of goat's hair warp, natural color wool weft, and pile wefts of red and yellow wool. A curious technical feature of the ancient Central Asian carpets is the formation of a pile surface on both sides of the piece. An interesting variant of pile tufting is used in an object which Stein suggests may be the tongue of a saddle or harness strap (T.VI. c. ii. 002). It is made of three to five strips of leather superimposed and sewn together. One side is ornamented with rows of upstanding tufts in crimson, dark blue, and yellow wool, the wool being couched down with buff thread on the top strip of leather, and the tuft ends cut off short. This superstructural decoration comes nearer to being embroidery than any other of the earliest finds from the *Limes*.
Goat’s hair fabrics, although Andrews often questions their composition, are almost always “coarse and loosely woven” of dark brown, buff (natural), and yellowish materials. Goat’s hair was plaited for rope and horse hair was plaited for strainers. The many entries mentioning “woolen string” may mean yarn prepared for weaving.

Felt occurs in combination with other materials, cotton, hemp, silk, or woven wools, in slippers and shoes. Because almost all of the specimens of footgear have several materials and techniques in their makeup, they present an unusually complicated technological problem. Felt ordinarily forms an inner sole or an interlining between fabrics used for the uppers. For example, one slipper type has a leather sole and uppers of felt covered with corded woolen cloth, red, purple, or blue-green; a more elegant Limes specimen (T. XXIII. b. 09) is made with felt foundation covered on one side with layers of silk, a pale blue one between two of white; the three layers quilted together with parallel rows of stitchery.

Truly, the above wools from the oldest of the extant fabrics of Central Asia do not form a very impressive list taken by themselves, but there is every reason to believe that the wools were local products.

Stein announces in his introduction to the monumental “Serindia” that he left on his second expedition into Turkestan with the same hope of recovering from ruins long ago abandoned to the desert sands more relics of that ancient Civilization which the joint influences of Buddhist India, China, and the Hellenized Near East had fostered in the scattered oases of those remote Central Asian passage lands.

And again he followed the old trade route to Lop-nor, a group of homesteads and townlets near the marshes of the lower Tarim river.

The Lop desert has been a region to dread from ancient times, but the usual conception of a desert is strangely at variance with the arid region surrounding the old Lou-lan sites. Deep trenches and sharply cut ridge formations of hard clay create an “utterably desolate” landscape which Stein compares to a “sea... frozen hard and buckled.” And yet, from such an unlikely area have come the quantities of relics of the ancient silk trade between China and the “Western Regions.” Undoubtedly, the silks were importations. Like the watch stations along the ancient Limes, the Lop-nor tract lies on the route which marks the first expansion of Chinese political influence westward into Central Asia.

From many literary references and from evidences found at various sites, Stein concludes that the old route, opened during the last years of the second century B.C., and always difficult because of the surrounding waterless wastes, was superseded by a new northern route and finally
abandoned by the beginning of the fourth century A.D. The fortified station at Lou-lan became inactive at approximately 270 A.D. The justly famous figured silks of the Collections came from a small cemetery on top of a mesa rising thirty-five feet above the surrounding ground. These silks must be treated as a unit in contrast to the other Lou-lan textiles.

Stein assumes from the rooms and the documents found in them that the dwellings at Lou-lan were those of resident officials of Chinese administrative headquarters. In most cases the last occupants had stripped the place bare, leaving only discarded objects and fragments. The majority of the textiles recovered are plain weave cloths, presumably of coarse or medium quality. Andrews has commented upon some “fine” textures, but in no instance has indicated the degree of fineness by yarn counts. The most interesting feature of these plain weaves is the color range employed. Buff, brown, and red are mentioned an almost equal number of times by Andrews; yellow and blue woolens are occasionally found, and green is noted in a single fragment. This rather limited use of colors must have been motivated, for, as the Mesa cemetery finds prove, many other dyes and combinations were known. Oddly enough, because they are considered by us as fundamental color variations of plain weaving, stripes, checks, plaids, and changeable or two-tone effects are conspicuously lacking. Only two of the thirty woolens described, L.A. V. xi. 02, a coarse woolen canvas in broad buff and brown stripes, and L.A. VI. ii. 0042, a two-tone effect gained by crossing buff warps with red weft, come under this classification.

Felts form the most numerous class after the plain weaves. About half of the specimens are dyed yellow. The remainder are brown, buff, scarlet, and white. The presence of felt fragments in quantity is to be expected since, according to the fort records, a “felt dress” was part of the individual soldier’s kit. Two specimen numbers (L.B. IV. v. 0032 and 0033) consist of small fragments of thin yellow felt painted with tempora. The patterns are apparently geometric and floral. The designs of one are dark gray, buff, and pale blue; those in the second piece, which has a conventional floral design of lotus flowers and spots, are red, buff, brown, gray, blue, and white. Andrews believes that the pieces were imitations of embroidery. Whether they are or are not imitations, these painted felts are unique in the Collection.

Yarn and cords of goat’s hair are found among the Lou-lan textiles. There is a single example of a hair fabric, described as “coarse,” and made after the manner of tapestry, with thick warps and close-pressed weft elements. The other examples are plaited handles and bands, coarse brown matting woven of goat’s hair string, and a four-strand rope for hobbling a
horse. The List includes the usual fabrications into which goat’s hair entered in early times although often a larger proportion of woven cloths is present than appears here. Horsehair, on the other hand, seems almost always to be found in fabrics for specialized uses. Whenever listed, the material is described as gauze or sieve cloth, open in texture, reinforced at the edge with felt, and made by “wrapped-twined weave” corresponding to the plain twining technique of basketry. Stein comments upon the “carefully woven horsehair gauze” peculiar to the Lou-lan region.

Although found in much greater profusion at the Lou-lan Mesa cemetery, there are several characteristic examples of pile carpet from the big rubbish heap L.A. VI. ii. A pile-knot fabric is admittedly of ancient origin, but Oriental rug authorities have usually stated that there are no extant examples antedating the fifteenth century, that none is to be hoped for, and that our knowledge of early carpets must come from paintings. Yet here at this modest Chinese station situated in the heart of the dread Lop desert were found fragments of pile-knot weave. Andrews gives a detailed analysis of a coarse but well-preserved specimen (L.A. VI. ii. 0046).

The length of pile is sometimes more than the 3". There are about 4 rows to the inch (vertical), and about 8 knots to the inch (transverse) equalling 32 to 1 inch square. On back, at intervals of 5 picks of weft are rows of long woollen tufts. The object of these may have been to prevent the carpet slipping on a smooth floor. The technique closely resembles that of the modern cheap Japanese rug. The colors used (in the Lou-lan specimen) are black, dull white, red, pink, buff, yellow, bright blue. A fine green occurs, but may be caused by damp and heat affecting the blue accidentally. The ground color is red and pink with indistinguishable pattern in lines, sometimes straight, sometimes meandering, of the other colors. In none of the Lou-lan fragments is the pattern wholly distinguishable, but all the colors are still bright. By contrast with the limited color range present in the plain-weave wools, the pile fabrics show designs in claret, red, pink, bright blue, dark brown (natural), light brown, buff, yellow, green, black, white.

Andrews makes several references to the “darri,” the true indigenous cotton carpet of India. It is usually stripped in two colors, often blue and white, although geometrical and floral designs appear in the more ambitious examples. Technically speaking, the weft face makes the cotton darri similar to wool tapestry. The darries in the Stein Collection apparently are similar in all respects to the familiar Indian weavings. Lou-lan sites furnish three rather elaborately patterned fragments. L.A. IV. v. 002 is a woolen darri; L.A. IV. v. 004 is woven in “satin twill,” but termed a darri. It has bands of color in regular order: deep red, dark green, yellow-green, bright yellow, saffron, violet on pale blue warp. L.B. IV. ii. 0013, according to
Andrews, resembles the ordinary Indian darri. Its warp is tightly twisted hemp string; the weave is "satin." Perhaps a satin effect is meant since the method of weaving the darri is in no way related to the usual satin technique. Andrews says, "the colors do not run right across the fabric, but are inwoven with each other at their junctions where they are cut off." He describes the addition of tufts at the back in rows about fifteen picks apart by a pile weave technique. The pattern in red and blue is the "stepped battlement" motive.

As was the case at the early *Limes* sites, Lou-lan sites furnished a number of most interesting slippers involving complicated construction features. Several descriptions state that the fabric uppers are woven to shape in one piece, a feat of no small skill. In addition, the tapestry-like fabric, which Andrews often refers to as a "silk weave," probably because of its ribbed effect, is elaborately patterned.

By comparison with any textiles found elsewhere at Lou-lan, the celebrated fragments rescued from reburials at the Mesa cemetery are of greater interest. At Lou-lan, and later at Turfan, Stein found evidences of an old custom of bandaging the dead in strips and rags of discarded or partly worn clothing. Due to this fortunate circumstance, the Mesa cemetery finds offer the opportunity to study a tantalizing array of fabrics in actual use during Han times. Among the 129 specimens from the Mesa, 74 are silk, and by way of indicating their historical importance, 45 of these are figured silks. Their undoubted Chinese origin does not in any measure preclude seeing resemblances to Sassanian, Coptic, and Byzantine silks of two to three centuries later. Technically they are products of weaving methods far removed from those of so-called "primitive" looms. The whole problem of warp manipulation as evident in figured stuffs is immeasurably expanded by these finds from Central Asia.

Wools, hemp fabrics, and felts are much less exciting. The Lou-lan wools are mostly plain weaves or canvas-like materials. But even an assumption that the fabrics were of local manufacture admits considerable knowledge of weaving techniques. In addition to fine and coarse plain weaves, Andrews lists a half dozen examples of reps and cord effects, the results of combining warp and weft yarns of appreciably unbalanced sizes. Several twills also appear in the List. Finds from the *Limes* watch towers include one example, but the Lou-lan sites, if I interpret the analyses correctly, yielded five twills. Two others (L.B. IV. ii. 0013 and 0016), a darri carpet and the upper of a slipper, are called "satin" weaves, but the illustrations seem to show what is, strictly speaking, tapestry technique, a plain weave variant. It is quite possible to err in judging from a reproduc-
tion, but the fact remains that the satin and the darri ordinarily have nothing in common. The five fabrics above mentioned are assumed to be twills. One, a "satin" according to the Descriptive List, has an allover pattern of lozenge motives, each lozenge centered by a small floral element (L.A. IV. 004). Floral motives are not usual twill designs, but the term twill may somewhat broadly be extended to classify any technique in which the weft or warp yarns float over more than one yarn in forming a pattern. Andrews calls L.C. 037 a "fine twill," L.C. 038 a "serge weave," L.C. V. 02b and 026 woolen damasks, adding that they are "monochrome, woven in the manner of L.C. i. O11." The last specimen is a "cotton damask . . . ground woven in small rib; pattern in large broken twill giving the effect of chequer. Lozenges, chevrons, rondels the pattern motives."

Although it is permissible to classify satins and damasks and figure weaves under the general heading of serges or twills, such usage extends terms which are more often applied to diagonal wale effects than to freely patterned fabrics. Literally, the term twill describes weaves in which the right or left progression of the successive picks of weft over the warps is regular and clearly defined. Regular progression, imperative in building up a pattern by means of twilling, may be varied in a number of ways, but each of the variations normally results in motives of insistent angularity such as chevrons or lozenges. Fabrics which we term damasks, satins, and figure weaves depend for their effect not so much upon regular and systematic progression as upon the manipulation of yarns to produce design elements of different shapes. Sometimes this involves standard twilling, but the amount is governed entirely by the size and shape of the motive.

The place of origin of the twill is unknown, but its use in wool fabrics, and the lack of a single early silk example in twill technique leads Stein to the belief that the weave is not of Chinese origin and may have been locally developed.

Another technique unrepresented among the 129 Lou-lan silks is tapestry. Whatever may be the reason, the woolen tapestry pieces provide the surprise element: their designs are unmistakably Hellenic in character. Stein suggests that the technique as well as the motives might have been acquired from the West. Tapestry weaving is an old art in Asia Minor and Greece, a fact proved by fifth century B.C. finds at Greek colonial sites in Crimea. The designs of the Central Asian wool tapestries from Lou-lan present the strongest possible contrast to the essentially Chinese motives characteristic of the silk fabrics. Among the ten tapestry fragments in the List there is nothing distinctively Chinese in style, and much that is reminiscent of Greek and Coptic designs. The most unexpected, and at the same
time the most typical among them is in specimen L.C. iii. 010a which shows
a portion of a human head of Western type rendered realistically even to
the flesh tones. Other Central Asian tapestries of approximately the third
century A.D. contain palmette motives similar to sixth century Western
examples in combination with cloud scrolls and horse-legged birds of the
Han art tradition. Andrews states of some examples that the weave is
“of Coptic tapestries,” equivalent to saying of the slot or Kelim type.

The Lou-lan Descriptive Lists contain about a dozen fragments of
different pile-weave fabrics similar in construction to the example already
noted (L.A. VI. ii. 0046). The patterns are indistinguishable in their en-
tirety, but elements such as shaded or “rainbow” bands, latch hooks,
zigzags, lozenges, dots, and stylized flowers may be traced. There are from
two to eight colors in each.

Andrews makes a number of references to a material rare (?) among
primitive weavings. He calls it “box cloth,” implying its similarity to the
modern thick fabric which is fulled until it is virtually waterproof. The
cemetery at Lou-lan yielded four examples, all two-tone, in crimson and
buff yarns. They are described as “fine cord textures, velvety to the touch
and apparently teased up in parts to a surface somewhat like box cloth.”
If teasing was done in Turkestan as long ago as the beginning of our era,
was it also done elsewhere? Logically, the technique would be developed
in an area where wool was used since teasing is a process essentially depend-
ent upon a thick loosely-spun yarn. Finishes subsequent to actual weaving
of fabrics are comparatively rare among primitive materials with the excep-
tion of the application of color by means of dyeing or printing.

The cemetery site at Lou-lan furnished a very old fragment of an em-
broidered comb case (L.C. 003) in chain-stitch work, a technique typical of
the Chinese, which has come down to us through the centuries. To judge
from the illustrations, the standard for extreme precision of execution
distinguishing the Oriental embroideries of modern times was set by the
first centuries of our era. The design is “the latch hook pattern, much
scrolled . . . within which are scrolls of naturalistic grape vine with fruit
and leaves. Chain stitch in dark blue (?), green, yellow.”

Felt fragments, abundant here as at other sites, are often padding layers
or linings for silks and other wools. Yellow is the favored color, although
brown and natural buff are fairly frequent. A fragment of buff felt with an
appliqué four-petalled flower of thin blue felt is an interesting specimen
of superstructural decoration.

The definite contrast between the textiles of the fort and dwellings and
those excavated at the Mesa cemetery is to be expected, since the first are
relics from soldiers’ quarters and homestead rubbish heaps, and the second finds represent grave wrappings. Although strips and rags of partly-worn garments were put to that ceremonial use, the initial quality of the fabrics was undeniably finer.

About midway between the Lou-lan sites and the present day Korla is Ying P’an site dating from Former Han times. The usual assortment of miscellaneous woolen fragments came from the refuse heap at the base of the stupa, but in addition to the plain weaves and the familiar modifications, rep, cord effects, tapestry, and darri, and the twills, some of the wool fabrics were in warp-rib weave. The importance attaching to these finds lies in the fact that all the Chinese figured silks from the Lou-lan and also from the Ying P’an sites are in this weave. We may have in the use of the warp-rib an example of the transference of the typical method of weaving silks to the more commonplace wools. Some centuries later, the twill, a technique for wools, was taken over by the silk weavers.

Stein visited the Niya site during each of his three expeditions. To the east of the Niya river he found a group of residences marking an ancient agricultural settlement mentioned by Hsuan-Tsang, the Buddhist pilgrim of the seventh century. At that time there was a town which “the king of Khotan makes the guard of his eastern frontier.” The small oasis was even than in danger of being conquered by the desert. Stein reasons that the site must have been abandoned prior to the fourth century A.D. The dozen or so dwellings had been stripped of all objects of value, but a close resemblance was recognized between the carved and painted patterns on remnants of furniture and frescoes and the familiar motives found in Gandhara reliefs. Dominance of Indian art forms can also be traced in the fragments of an ancient rug (N. VII. 3) discovered in the open fireplace in the centre of a great hall. The material, the technique, the design of transverse stripes and simple geometric units of svastika and stupa-like motives, and the buff, gray-blue, yellow-green, yellow-red colors are all Indian. The illustration of this rug from a period at the beginning of the Christian era might just as well be a reproduction of a typical cotton darri of modern India.

The plain-weave woolen fabrics found at Niya, to judge from the plates, show considerable variation in texture, but the terms “coarse” and “canvas-like” are the usual descriptive words used. Colors are brick red, dark pink, crimson, “dark gray and dirty white,” yellow, and buff. Two wools illustrate variations through color choice: N. XLV. i. 010, a stripe, and N. XLV. iii. 004, a check.

Several carpet fragments are listed. Two are specifically stated to be of wool woven after the manner of the cotton darri. N. X. I. has a fret pattern
in narrow bands at intervals dividing the larger bands from each other. These narrow bands are blue and yellow, the two colors counterchanging within successive repeats. The warp is of undyed wool varying from dark brown to dirty white. N. XIII. ii. 001b is a fragment of firm thick wool (?) darri, in stripes of buff and dark brown "interspersed with thin lines of red."

The one example of pile weaving from the Niya site is similar in construction to the Lou-lan carpet L.A. VI. ii. 0046 described above, but without the reverse-side tufts. The relative qualities of the two may be judged by the number of knots per square inch: Niya example, 50 knots; Lou-lan example, 32 knots.

In one of the "dust bins," as Stein calls them, was found an unusual specimen. Andrews believes it is a button from which a metal centre and shank are lost (N. XIV. iii. 0033). It is a disc of leather with square central hole (like a Chinese coin) on one side of which wool strips are sewn, three of red, three of blue, and three of yellow, each group radiating from the centre. Fragments of light red plaisted woollen braid one and three-eighths inches wide (N. XIV. iii. 006e) also came from the same rubbish heap. The Niya specimens include several examples of goat's hair fabrications: "rope," "brown and white string," and "a flat string band. Plaited of 5 strands, 2 dark brown, 3 yellow" (N. XIV. iii. 007d). The only hair fabric mentioned, apparently a monochrome tapestry type, is "very coarse." It is woven of dark brown and buff yarns (N. III. x. 012).

Felt fragments were found in numbers, but the felts at Niya present some interesting aspects. There were the ordinary light buff and brown rags suggesting use as shoe materials, and the ubiquitous yellow scraps. The plates show variations in quality: N. XV. 014 is a compactly felted, rather thick stuff, while N. XV. 015 is loosely felted. N. XII. 0018 is a sieve for clearing milk. It is a "network of coarse vegetable fibre, yellow and brown, set within a circular frame, c. 10" in diameter, made of withies bound with string, and covered with yellow felt sewn." A "conical headgear like a Phrygian cap made from carefully gored yellow felt" came to light in clearing the great hall N. XIV. Although in fragmentary condition the five segmental strips about four inches wide composing the upper part are well preserved, and also enough of the lower five-inch portion to reconstruct both size and shape of the cap. The height is about fourteen inches, the lower diameter about eight.

Two felt fragments, examples of surface decoration, came from a rubbish heap. N. XIV. iii. 006c is a fragment of "natural" buff felt on which is appliqué a scroll wave pattern in thin crimson felt. N. XIV. iii. 007
is composed of "two strips of felt, light buff and dark blue. The blue is held to the buff with stitchery in red thread." Andrews says, "On blue side stitches are scattered; on buff, irregular cross work." The piece may be a bit of an edge decoration.

But of the various textile finds at Niya, none is more interesting than a definite reference to felt rugs in the early documents. One of the wedge-shaped tablets found in such profusion in the rooms and refuse heaps recorded a transaction involving household goods, sheep, vessels, wool weaving (?) appliances, implements, and "Namadis." Stein comments, "We may well recognize here the earliest mention of the Numdahs so familiar to Anglo-Indian use." Fortunately, the tablet was associated with a Chinese document bearing a date corresponding to 269 A.D.

Among Andrews' descriptions of the finds from Kara Dong, near Niya, appears the following for Ka. 1. 0014: "One fragment fine twill woollen fabric printed with repeating pattern of dot rosettes alternating with plain round spots; ground light red, pattern buff." Is the piece printed, or is it tie-dyed? Buff and "buff, natural" are the names given by Andrews to the natural color of wool. If the ground of this textile was really printed light red leaving the pattern the presumably natural buff, we have an example of negative patterning. But "dot rosettes" and "plain round spots" are the most typical design motives resulting from the tie-dye process. The only specimen in that technique in the Descriptive Lists of the Turkestan finds is one from Endere, an eighth century site. E. i. 029 is a "blue cotton, knot-dyed' fabric."

LATER FINDS

The finds so far mentioned are earlier by from three to seven centuries than those from Turfan sites dated relatively or definitely within a period from the seventh to the tenth centuries A.D. Yet, for a number of reasons, the Astana site in particular is notable from the textile aspect. The period between the latter half of the sixth century and the first half of the eighth was a period of Chinese influence heightened by the renewal of political control and increased facilities for trade between the East and Central Asia. Turfan lay on the northern route to the West, and had special advantages by reason of its position as a place of convergence of the main and smaller routes.

The Astana burial customs were not unlike the much simpler ones found at L.F. site near Lou-lan. Beneath the body shroud Stein found wrappings of rags and strips torn from old, apparently valueless garments. Tomb chamber after tomb chamber was cleared without finding a trace of woollen cloth. Practically all of the hundred fabrics were figured and plain silks
and cottons. The Descriptive List contains only two mentions of woolen pieces, both canvas-like, and both used in combination with other shoe cloths. But if actual wool finds are almost totally lacking, the regular and abundant supply of silks from Chinese centres explains the fact. That wool had it place in everyday life even if it furnished none of the customary burial garments is shown through another of the usages in that region. Among the objects placed in a tomb chamber were varying types and sizes of clay images such as camels and horses with men or women riders. Each of the horse images recovered shows a carefully rendered numdah saddle blanket. The painted "embroidery," characteristic of the horse trappings in use during the sixth to the eighth centuries, is also the typical ornamentation of the felt rugs sent out from Turkestan today. One saddle blanket (Ast. III. 2. 058) is described by Andrews thus: "... Numdah has red ground and is gaily decorated with elaborate palmette and flower pattern representing embroidery, in orange, red, emerald green, pink, dull blue, picked out with black and white."

Dandand Uulid, a monastic (?) site yielded only one find which bears any relation to wool fabrics. According to the Detailed List, not a scrap of wool cloth came to light during the excavation of fourteen shrines and houses. The unexplained absence of wool finds seems most peculiar in a region famous for its weaving. Document dates, between 782 and 787 A.D. are assumed for a painted wooden panel (D. VII. 5), important to any historical treatment of felt. The panel has been described by both Andrews and Stein.

It shows two figures, both mounted and manifestly of high rank, one above the other. The upper figure is seen riding on a high-stepping horse. . . . The horse . . . carries a deep and narrow saddle over a large Numdah or felt-cloth, and shows elaborate trappings. . . . We could not have wished for a more accurate picture of that horse 'millinery' which in the eighth century evidently flourished throughout Turkestan as much as it does nowadays.⁶

At Khadalik, a minor site dating from the eighth century, were found small fragments of silk paintings, and a part of a "small pennon of a coarse woolen (?) fabric. With it was a piece of similar cloth still bearing traces of a painted design." This is the first painted wool mentioned, and more detail would be appreciated. Another Khadalik piece is described as a "brown woolen fabric, 'braid texture,' rather fine."

The felt fragments, red, yellow, and natural buff, appear both as portions of garments, and as padding materials. Kha. IX. 0030 is a "circular

mat (diam. $3\frac{1}{4}''$) of coarse linen material, two thicknesses with felt between, the whole carefully bound round with a narrow strip of red linen fabric. All edges turned in and neatly finished.

From the refuse heaps in a string of hamlets in the valley of Kara-tash River east and south of Khotan, Stein found ragged woolen fabrics with an unusual color range: buff, sulphur, sienna, brown-red, and crimson. At Sampula in the Khotan district, also an eighth century site, he found pieces of coarse red woolen serge (a twill), and fragments of wool tapestry border (Kelim). Andrews calls the bold pattern a "rather badly balanced modified anthemion with base in symmetrical scrolls." The colors are red, yellow, buff, purple, blue, and brown. An ancient rubbish heap at Mazar Toghrak in the same general region contained a wool rug fragment (MT. 81). It is patterned in red, blue, and green bands on which are white circular spots, a blue serpentine scroll, and simple geometric forms. Andrews does not mention the technique.

It is assumed that Andrews means a woven fabric, not a felt when he lists a fabric as "wool." In that case, finds from Endere, near Niya, illustrate a fairly wide range of colors: pink, crimson, dull carmine, blue, dark blue, yellow, brown. The felts were buff (natural) and yellow in the proportion of three natural colored felts to one dyed piece. A T'ang period fort with surrounding ramparts was cleared at Endere. More important than the fort was the discovery that it had been built over the site of an old rubbish heap, thus preserving its contents. A miscellaneous group of cloths from the refuse are listed under the number E. Fort 0012, and represent specimens of "plain cloth, plain corded, plain twill, and fancy twill forming lozenge pattern." Such an enumeration, although the first two weaves are technically the same, testifies to considerable knowledge of the loom. There are also a "woollen braid" and "two plaits of hair string" (E. Fort 0014 and 0016).

The Miran ruins are south of Abdal, the small hamlet from which Stein made his start for the Lop desert sites. He has determined that Miran, like Niya, Endere, and Lou-lan, must have been deserted at approximately the end of the third century A.D. Later the Tibetans built the fort and its rabbit-warren quarters on the site. The small rooms and hovels proved a mine of relics. Rooms nine feet in height were filled to the roof with rubbish, and evidence was not lacking that they were in use during the accumulation of the refuse.

"The relics of the Miran fort," says Stein, "show a distinct falling off from the standards of technical skill exhibited by the remains at the Niya and Lou-lan sites." Most of the wool textiles in the List are "coarse."
Andrews states that "the variety of texture is considerable in these fabrics. ... But all are woven strongly and skillfully, and colors are well preserved. ... It is significant that not a rag had found its way into the rubbish until it had become hopeless for use."

The plain-weave wools are in the majority. Besides being "strong" and "coarse," Andrews writes that they are a "loose and somewhat irregular texture," and that they are the "texture of English hopsack tweed," a basket weave. Buff (natural) and a brick red appear in an almost equal number of fragments; salmon and crimson each occurs once.

The fragments referred to as daries are both "fine" and "coarse." The patterns represent three rather distinct types. Two fragments are banded with stripes in a variety of colors, and in widths ranging from one eighth inch to two and a quarter. M.I. 0084 is banded in terra cotta, dull myrtle, green, buff, and brown; M.I. XXVII. 009, a finely woven coin bag, has fourth-inch stripes of red, green, buff, and indigo in regular order. The second type of pattern is an adaptation of the key motive. In the three fragments showing the key motive the colors include red, dark green, buff, yellow, blue, and red-purple. The third type illustrates a pattern decidedly different from any other darris pattern so far encountered among the examples from other sites: animal motives. The following is an excerpt from Andrews' description: M.I. XXV. 001. "... The border attached to one edge is 1" wide and woven in blue and yellow; the pattern consisting of conventional running animals, such as may be seen in ancient Cretan, Greek, and Persian art."

Fragments in twill weave are found in greater proportion among the Miran finds than among those from any other site. The types are the regular two-and-two and herringbone.

The Miran felts are like those from other sites in quality and color, but they illustrate several unusual uses to which the fabric was put in the eighth century. M.I. 0080 is a felt pad four by three inches, buff color, of double thicknesses of fabric stitched together. The pad is filled with powdered charcoal, suggesting that it may have been a "pounce bag (?) used for transferring designs to fabrics before painting or embroidery." There are two round felt cords. One, M.I. 0086, is "salmon colored, made by felting together a number of loosely spun strands of yarn." The other M.I. XX. 006, is finer, "made of two or three buff strands laid side by side and joined by a coating of red felt." These are the only recorded examples of cords composed of felted material. M.I. ii. 0038 is a part of a gourd vessel which has been broken and mended with felt. A strip of thick buff felt 8½" long sewn to gourd. ... Along sides of original break holes have been
made at regular intervals of $1^{1/4}$" and the strip of felt carefully and closely sewn with goat's hair strings. These holes were then plugged with short wooden pegs to prevent leakage.

A very simple sling of goat's hair came to light in the refuse at Miran. It consisted of an elliptical felt pouch for holding the stone, and a seven-strand plaited cord which divides into two parts to form the edge of the felt, then reunites (M.I. 0081).

The ruined fort of Mazar Tagh lies on the shortest route between Khotan and the oasis on the northern rim of the Tarim basin. Here as at Miran Stein found manifold signs of Tibetan occupation. A unique specimen among the felts is the small ring about three inches in diameter which may have been used on the head when carrying weights (M. Tagh. a. 001). The woolen fabrics were found fastened by stitchery to felts, a combination implying use as shoe materials. Buff and dark brown predominate; carmine and dark blue are mentioned once. A checked blue and white woolen material is noted because of the rarity of color combinations among plain weaves (M. Tagh. Fort. 02d). A fine canvas (M. Tagh. 0654), which Andrews calls wool, shows remains of paint on each side. If it is a painted banner, it is the single representative of a wool fabric put to that purpose.

The most spectacular finds made by Stein came from the cave temples of the "Thousand Buddhas" southeast of Tun-huang, to be dated by their Chinese inscriptions between the years 864 and 983 A.D. The outstanding fact about the textile finds from these caves is that for the investigator of wool fabrics in Central Asia, the Descriptive Lists are total blanks: among 230 woven specimens, there is not one mention of wool.

CONCLUSIONS

The sketchy survey of Sir Aurel Stein's published reports made in this paper does no more than indicate obvious possibilities for the textile analyst. Recovered Turkestan wools of the period between the first century B.C. and the ninth century A.D. permit one to make generalizations with confidence concerning the types of wool fibres used, the weaves, patterns, and color range. Qualities of cloth as indicated by warp-weft counts per inch are not known to date. The omission of such details is unfortunate. How fine is "fine," and does it mean the same for silks and wools?

Three fibres, sheep's wool, goat's hair, and horse hair, were staples. The first is found in by far the greatest number of fabrics of all qualities. It is spun into yarns, doubled for cords, and worked under moisture and pressure for felts.

About weaves we can know a good deal. No question of origins is in-
volved. The earliest sites excavated yielded weaves which are a long way from the starting point of simple interlacing. Of the eight standard types known to the modern textile industry, Turkestan artisans in the third century A.D. knew four: plain and its variations, twill, figure (similar to our Jacquard), and pile-knot. The twill occurs among the early wools, but it is not found among the Chinese silks until later. To solve the problem of its origin requires more source material concerning Western weaving. Brocade types, usually considered simple, gauzes, double cloths, and true satins are lacking although Andrews often refers to a "satin twill," and the warp-rib weave of the early Chinese silks occurs in the wools at Ying P’an.

The pile weave appears to be the most outstanding technical achievement of these early Turkestan weavers. The specimens illustrate the fully developed technique with basic weft and independent pile weft, which although "knotted" differently from those in the familiar Oriental types, is nevertheless the result of a twist given a short length of yarn.

The single-element fabrications are conspicuously lacking. There are no examples of knitting, and but few of plaiting. The failure to record the number of strands in the listed braids makes inventorying impossible. Five-strand and seven-strand specimens are mentioned, and the phrase "braid texture" is used several times. Any textile analyst will appreciate the welcome refuge offered by the phrase.

Superstructural decorations, embroidery and appliqué, are infrequently found. Single examples occur, but apparently needlework was mainly utilitarian. The very beautiful comb case from the Lou-lan Mesa cemetery is the outstanding piece of chain-stitch embroidery.

Andrews’ patient descriptions of the aesthetic aspects of the Central Asian fabrics are marvels of accuracy, to judge from the identity of the notes and the photographic reproductions of specimens. The textile patterns in wools range from stripes to complex geometric designs of the darri and pile rugs, and the realistic motives in the tapestries. Any list of elements must include bands, crosses, svastikas, latch hooks, scrolls, leaf and flower forms. It is only in the descriptions of running animals, human heads, composite creatures, and cloud motives that we realize how truly the regions of Stein’s explorations were “passage lands.” In arbitrarily limiting considerations in this paper to the wool textiles, only a comparatively faint reflection of the influences at work forces recognition.

Color range in the early Central Asian textiles offers several surprises. One is the limited color repertory of the felted materials. With few exceptions they are natural buff, brown, and yellow; the fragments from some sites being predominantly one or the other color. The woven wools, by con-
trast, are often brown or buff, rarely yellow. The reason does not appear. As for the other colors in the gamut, reds are numerically in the lead. Andrews calls them dark red, pink, crimson, scarlet, claret, carmine, brick, and terra cotta. Blues come next in number: dark, pale, bright, and indigo. Greens and white occur about half as many times as blue; black and gray about equally; purple least often. Apparently Tyrian purple did not travel eastward. This color order is only relative; no exact count was made. A natural inquiry concerns the dyes used. Neither Stein nor Andrews offer any information on the subject, which fact indicates that nothing suggestive of materials or processes was found by either of them.

The wool fabrics made during a period of ten centuries in Central Asia show little change. The plain-weave wools and the goat's hair cloth are "coarse," whatever the century; there are no new weaves, and there is no appreciable amplification of the color range. Perhaps there were localities where greater skill was in evidence, or where far Western and Eastern influences were more pervading, circumstances which would have been reflected in the weavings. In the areas Stein visited wool fabrics seem to have been relegated to commonplace uses. With Chinese silks coming regularly, there was no particular incentive to develop the possibilities of a sturdy, reliable, homely material. Even the rubbish from the soldiers' quarters at the watch towers (third century) contained figured silks. Wool was essential and fundamental to the lives of its wearers, but, except in the tapestries and pile weaves, it did not inspire the development of techniques or devices which would make of it a beautiful fabric.

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ASSOCIATIONS AND THE STATE IN THE PLAINS

By E. ADAMSON HOEBEL

PROFESSOR LOWIE has focused attention on the rôle of military societies among the Plains tribes as effective territorial unifiers. The policing of the buffalo hunt was emphasized as the specific cohesive function which acted to counterbalance the centrifugal forces residing in inter-club rivalry and frictions. The importance of such police activity is notable because it so sharply contravened general Plains notions of individual freedom of action and lack of centralized authority. Still, in harmony with the general culture patterns of the area, the police functions are said to have been limited to the vital and short period of the hunt.

Two paragraphs sum up Lowie's exposition.

With amazing uniformity as to detail the police functions just described have been recorded for a dozen and more tribes and for a period of two hundred years. The personnel of the constabulary varies with the tribe; the duties may be linked with a particular society (Mandan, Hidatsa), or be assumed by various military societies in turn (Crow), or fall to the lot of distinguished men without reference to associational affiliations (Kansas). But everywhere the basic idea is that during the hunt a group is vested with the power forcibly to prevent premature attacks on the herd and to punish offenders by corporal punishment, by confiscation of the game illegally secured, by destruction of their property generally, and in extreme cases by killing them.

In other words, for the brief period of the hunt the unchallenged supremacy of the police unified the entire population and created a state "towering immeasurably above single individuals," but which disappeared again as rapidly as it had come into being (italics mine).

Here is a point which calls for careful study by the student of political and legal origins. Here is a potent germ of the state. One wants to know what lines would have been taken had this germinal instrument been given opportunity for expansion and development. Into what phases of civil life would the police authority tend to penetrate? But is it not possible that police authority had gotten beyond the bounds of the buffalo hunt?

We need to know more about this than is allowed us in the literature.

1 Association is here defined as a social group specifically organized for the pursuit of some common interest or interests in so far as such interests are not primarily based on either the blood or the territorial tie. Cf. R. H. Lowie, The Origin of the State (New York, 1927), p. 74.
2 Ibid., pp. 94-107.
3 Ibid., p. 103.
4 Ibid., p. 104.
Consequently, when I went to the Northern Cheyenne in the summer of 1935 to see what more could be discovered pertaining to matters of law and control, I was on the watch for civil activities of the military clubs. Some of the results are presented herewith.

Authority to assume the police functions was given to a society by the tribal council, except in the case of the Sun Dance, when the society of the dance pledger was in charge. The responsible club retained its position so long as the tribe was drifting in a certain direction, to relinquish its control when the direction of movement was changed or the bands separated.

It is in the situations which arose when a hunter was suspected of having brought in buffalo taken by stealth, when the "no hunting" rule was in force, that we first began to note the extension of powers of the Cheyenne soldiers. The set-up is, to be sure, still a part of the hunt complex, but it is an important step to allow a searching party to enter a man's lodge to look for evidence. This is a social policy of a much more deliberate nature than that of whipping a man on the immediate field of his crime at the instant of detection, as it has hitherto been always reported.

Consider the case of Man-lying-on-his-back-with-his-legs-flexed and that of Low Forehead. The Shield Soldiers were having a dance. They had given the order that no one was to go out to hunt buffalo. Man-lying-on-his-back-with-his-legs-flexed let it be known he was going out to hunt small game for his family needs. There was no objection. But when he unintentionally got into a herd of bison, he simply could not restrain himself. He shot a fat cow. He was observed. When the Shields had stopped dancing, their chiefs were informed. When the soldiers appeared, Man-lying-on-his-back-with-his-legs-flexed did not rush out to protest his innocence as he saw them coming, so they took the next step. They ripped a gash down the back of his tipi. In spite of this warning, he sat inside saying nothing. Then they knew he was guilty, and destroyed his lodge about him.

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6 It would be ungrateful not to bear witness that Karl N. Llewellyn of the Columbia University School of Law had a large hand in awakening me to this point. Acknowledgment should also be made to the American Council of Learned Societies for the grant which made this study possible.

7 This seems to have had nothing to do with the necessity of preserving the food supply, but impresses me as a form of ceremonial idleness imposed by a group which had the power given it to a different end. If so, it indicates in another way how the Cheyenne associations were curbing the individual's personal rights to liberty of action.

8 It was stated that though sentries were not posted against enemies, there were always soldiers lurking about the fringe of the camp when the "no-hunting" rule was invoked. They observed who left the camp. If the absent ones were not back by early evening, they began to tell each other that he must have killed a buffalo. A special watch was kept for the return. Also, women and non-soldiers might observe infringement and report.
Not so Low Forehead. He was informed on one night. When the soldiers approached, he rushed from the lodge holding high his hands, calling upon them to stop. He denied the charge and invited a search. Last Bull and Wrapped, two chiefs, entered and hunted about. They emerged and said it was all right. At that the soldiers withdrew.9

The pattern was clearly established. If innocent, one rushed out holding up his hands in protest. A search followed. If guilty, one made no move, and the penalty followed. Overharsh action on the part of the soldiers, however, was not well taken. The case of Last Bull in his treatment of Grasshopper shows this.

The Fox Soldiers had issued the "no-hunting" decree. Grasshopper ignored it, and was informed on. Blood was on his horse before his tipi. Last Bull as chief of the Foxes rode up and shot the horse without further inquiry. This fanned Grasshopper's mother to such a state of indignation that she cut several gashes in the chief's tipi. At this Last Bull called out the Foxes.

"Friends," he said, "We're going over there and ruin everything those people have."

On learning this intent, the father of Grasshopper loaded his gun and took vigil. It was his purpose to shoot the first man to touch a thing. The Foxes drew bead on him, telling Last Bull to destroy the lodge while they covered the old man. Then one soldier spoke up with soothing counsel, saying that the incident was not worth all that trouble. (They would have killed the whole family—a half war, according to my informant.) The advice was accepted and the crisis averted. The old man announced he was going to move off to one side to camp and hunt by himself. They let him do it.

People said Last Bull did just right, that he had a right to "wipe out the whole family to enforce the law." That it was for the commonweal he acted was recognized by the informant, Calf Woman. When asked if she would have informed if she had seen Grasshopper coming in, the answer was, "Yes. The soldiers were not working for themselves, they were working for the people."

When questioned, "If he had been a relative?" the answer was, "I'd lie low. Some one else would tell anyhow." The conflict between kin and community ties resolved!

In another field of civil action, too, the soldiers had penetrated deep as the police arm of the community. Murder was both a crime and a tort to the Cheyenne. Crime first and tort second. Tribal welfare rested in the

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9 These cases were given by Stump Horn, aged 84, who was a Shield Soldier at the time.
holiness of the Medicine Arrows and the Medicine Hat. The death of a Cheyenne at the hands of a Cheyenne "bloodied" the arrows. So long as the blood remained there could be no successful war parties, and game shunned the country of the Cheyenne. "It made the tribe lonesome." The ritual of renewing the arrows effected the purification.

A stigma attached to the murderer forever. He became rotten inside so that his very breath was putrid. His body gave off a bad smell. This, even though the arrows had been renewed, was enough to drive off the game. Hence, a sentence of banishment was imposed by the tribal council and enforced by the soldiers. Actually, no enforcement seems to have been necessary. The doom was recognized. The exile went usually to the Dakota or Arapaho where his evil odor did not apparently bother the herds. The many cases in which blood vengeance was visited during the exile, or before it could be imposed, show that the community law had not completely downed the private law.

Calf Woman brought out a crucial case which tests the position of the soldier police in the murder situation. An aborted foetus was found in the vicinity of the Cheyenne camp. The discovery was made known to the chiefs. The presumption was entertained that the foetus was that of a Cheyenne. Yet nothing was known about it. The soldier chiefs were consulted; by them a plan of investigation was produced. The two chief chiefs of a society convened their group and the society announcer was sent out to broadcast the order of the soldiers for all women to assemble in public. When it was ascertained that all were at hand, the women were ordered to expose their breasts for inspection. Each was then scrutinized by the soldier chiefs to note lactation enlargements of the breasts as a sign of recent pregnancy. One girl showed symptoms and was charged with the crime, judged guilty, and banished from the tribe until after the arrows had been renewed. Calf Woman claims to have been present, but to have escaped the inquisition because she was too young to have been pregnant. This would place the event in the 1860's.

Three salient points are to be noted here. First, that the unborn child had tribal status and insofar a legal personality. Abortion, too, was murder which tainted the tribal medicine. Second, this was a situation wholly criminal, for the violence had been done within the most intimate family unit possible. Blood feud was precluded. Third, the murder was a secret crime demanding detection of the criminal. The technique was ingeniously invented to meet the situation.

Clearly, the military associations were well along towards establishment as a civil power.
One last instance will suffice to complete the demonstration of police penetration of civil life by the soldier groups. The people were moving camp through a deep snow in dead winter. Bird Face, a Dog soldier, came upon his niece, Elk Woman, struggling nearly exhausted through the drifts. He sang a Dog soldier song, "In any fight I protect the people." To this he added, "This is a cold day; I'll do the same thing." Whereupon he had Elk Woman mount his horse behind him.

Riding on, they overtook Sleeping Rabbit, husband of Elk Woman.

"There is one thing I don't like," Bird Face upbraided him, "That is leaving a woman in the deep snow. I don't know what would become of my nieces if I were dead. How would they get through the deep snow?"

He made no threats, according to Calf Woman, but Sleeping Rabbit's rejoinder was to draw his bow and drive an arrow into the hero's left elbow. Bird Face pulled the shaft loose, but the head stuck fast in the wound. Red Eagle, son-in-law to Bird Face, came up and took Bird Face home. Then he set after Sleeping Rabbit with his gun. When he could not find the culprit, he turned back.

Meantime, Bird Face was having difficulties. Attempts to extract the arrowhead failed. The lower arm gradually blackened until amputation was thought necessary. The situation was serious, for, as Grinnell categorically states, the Cheyenne never practiced amputation, that no man was willing to lose a limb, nor would any doctor undertake the responsibility. In the face of this Red Eagle called the Fox Soldiers together to see what they would do about it. (Note that this was not Bird Face's own society: it is not a case of a society avenging a harm to one of its own. It is community responsibility at work.) The decision was that they would apprehend Sleeping Rabbit and "beat him until he was as sore as Bird Face's arm." (A poetical nicety, it seems.)

Though Sleeping Rabbit was informed by a friend, the Foxes caught him. They punched him; they kicked him; they beat him with sticks. They threw snowballs in his face; they pushed him in the snow. But they did not kill him. During the mêlée a Fox called out that any who failed to beat him would have to give a Contrary Dance. Everyone spared the expense with a vim.

This accomplished, the arrowhead was nevertheless still in Bird Face's arm. So the Foxes told Sleeping Rabbit he had to remove it. Compulsion gave him strength where others failed. He managed it. The arm was now so bad that they ordered him next to amputate it. And he was condemned to sit up with the patient every night until he was well.

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Public feeling ran high against Sleeping Rabbit. The fact that he was a tribal chief at the time probably heightened it, for a chief of that order was custom-bound to generous and noble behavior. The fact that Bird Face was beyond the philandering age made the violence wholly unjustifiable in the public view. Other soldier bands, according to Calf Woman, were agitating for driving Sleeping Rabbit from the camp. Contrition saved him. He had five good horses which he presented to the Fox Soldiers with a statement of repentance. These worthies took them to a trader in a deal for a barrel of whiskey. They set up a feast and invited all the military associations to a drinking bout. They had a good time. "And no harm was done."

The whiskey is said to have pacified the warriors, and the matter was considered settled. Sleeping Rabbit served his chieftainship until the expiration of his term, attending the council meetings. He lost his wife, however, for Bird Face declared her divorced from him. Though Calf Woman was in the camp through all of this, she cannot remember that Sleeping Rabbit gave gifts to Bird Face. It appears that there was no composition. The penance did not ensure to Bird Face's benefit. The "fine" was paid to the government. And with it its functionaries were socially rewarded for their arduous labors.

Thus it is that for the Cheyenne, at least, the military societies had become an ever-ready arm of a state "towering immeasurably above single individuals," not only on the occasion of the buffalo hunt, but in internal civil affairs as well.¹¹

¹¹ I would not maintain that parallel features will be found in all Plains societies. From my own field studies I have found that the Comanche, for instance, who had established a system of associations (see R. H. Lowie, Dances and Societies of the Plains Shoshone [Anthropological Papers, American Museum of Natural History, Vol. 11, pp. 803-35, 1915], pp. 812-13), had no buffalo police whatever. The Idaho Shoshone, on the other hand, had no military societies, yet I find that they organized an association of police to function when they went into the plains to hunt. But I do suspect that a good deal more of what I have reported here for the Cheyenne would be discovered among the more developed Plains tribes, if there were more searching in this particular field by interested students.
THE CAVE OF PUY-DE-LACAN: A MAGDALENIAN SITE IN SOUTH-CENTRAL FRANCE

By LILIA AND HOMER H. KIDDER

On the slope of a wooded ridge overlooking the valley of the Corrèze near Brive, the Cave of Puy-de-Lacan opens to the south in a ledge of Triassic sandstone nearly 100 feet above the river at a distance of 273 yards from the right bank. A natural pillar divides the entrance into two arches. The interior, supplied with water that seeps through the coarse rock, measures thirty-two feet in width by forty-two in depth, including a low cavity at the inner extremity. From a rock-terrace in front, a sandy talus drops at an average angle of 15° to the foot of the hill.

This is the only site in the region of Brive that is known to have been occupied by folk of Magdalenian culture. The case is surprising for a locality counting many Solutrean and Aurignacian sites, the more so as some forty miles downstream beyond the point where the Corrèze flows into the Vézère, the neighborhood of Les Eyzies was, in Magdalenian times, as throughout the Upper Palaeolithic, the most frequented center of human life known in the prehistoric world. It may well be that with the advance of ice to within fifty miles northeast of the location of Brive in the recrudescence of intense cold that prevailed during the Bühl glaciation, most of the inhabitants, following perhaps a movement of game to the southwest and west, abandoned the Corrèze for the lower valleys of Périgord, where, in fact, as along the Dordogne, the Isle, and the Charente, Magdalenian sites are considerably more numerous than those of preceding epochs. Whatever the cause of its isolation, Lacan was on the fore-front of man's range in the direction of the ice-covered highlands of central France. No Palaeolithic site has been found farther up the valley or in Upper Corrèze stretching northeastward across the bleak Plateau des Mille Vaches towards the Monts d'Auvergne.

The site was discovered and named some seventy years ago by two pioneer prehistorians of the Corrèze, Elie Massénat and Philibert Lalande, who, digging through a meter of sand on the talus, found a bed of Magdalenian culture. Lalande published an interesting account of their work. About the year 1900, when a sand pit was opened at the foot of the slope, the shovel men collected on their screens some 700 flaked flints which they turned over to Abbés L. Bardon and A. and J. Bouyssonie, then beginning

1 The last statement is based on an unpublished list indicating the location of prehistoric sites in Périgord by M. D. Peyrony, which the author kindly permitted us to consult.

a notable series of studies of the Stone Age in Corrèze. Nearly thirty years later, without knowledge of former discoveries at Lacan, we ourselves dug the deposit lying just in front of the cave. Today a new exploitation of the sand pit, which is stripping the talus to the rock, has exposed further archaeological deposits, these being closely observed by two competent prehistorians, Abbés L. Lejeune and J. Bouyssone, aided by M J.-F. Pérol. They have reported a Solutrean bed at the foot of the hill and above it an important Magdalenian deposit which runs up the talus towards the levels dug by us. When the relation of these deposits to ours has been established, MM Lejeune and Bouyssone propose that we collaborate with them and with M Pérol in an article on the site as a whole. The rest of the present communication is therefore concerned only with our own work at Lacan. For the same reason, Figure 1 shows, in cross section, only the archaeological beds dug by us at the top of the talus.

We first visited the cave, then concealed by woods, in the course of a walk in June, 1929. Poking with a stick in the sand in the interior, one of us unearthed a couple of flaked flints, a small saw-toothed blade, and a borer (fig. 3, nos. 1 and 2). This find led to the reopening of the site.

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2 Two papers by these authors that contain passages on Lacan are (1) "De la Succession des Niveaux Archéologiques dans les Stations Préhistoriques des Environs de Brive" (Compte Rendu du Congrès Préhistorique de France, Perigueux, 1905); (2) "Sur les Stations Préhistoriques des Environs de Brive" (Compte Rendu du Congrès Préhistorique de France, Angoulême, 1912).

FIG. 2. UPPER, The Cave of Puy-de-Lacan (Corrèze). The shadow below the entrance marks the overhang of the little cliff, with hearth beneath (X, fig. 1). LOWER, Fragment of reindeer antler from Magdalenian III at Laugèrie Haute (Dordogne) showing method of cutting elongated pieces to be made into implements or arms.
Through the kindness of M Albert Dejean, of Brive, to whom we were strangers, we obtained from his cousin, Baron Docteur Charles Le Clere, of Payzac, owner of the land, the generous permission to excavate. We wish again to express our thankfulness to these gentlemen.

Having dug to bed rock in the cave without striking an expected archaeological deposit, although we found a good many flaked flints, we ran our trench on to the talus and twenty-five meters down the slope. The cross-section (fig. 1) shows three archaeological beds there brought to light: A, B, C, as designated from the bottom upwards. They abutted against a low cliff in front of the entrance, beneath a layer of sand, D, which extended down the talus from inside the cave. At eighteen to twenty meters from the rock, A, B, and C gradually merged with the sand of the talus and were no longer distinguishable. Maximum thickness: A, 45 mm.; B, 54 mm.; C, 65 mm.; D, 1 m. 15 mm.

These four beds we excavated systematically in the summer of 1929, one layer at a time, working uphill. The debris was shovelled down the slope to right and left of an oak which we hoped to spare, near the foot of our digging, but which died when the moisture had drained from the block of earth in which it was rooted. Near the rock, a hearth or oven (X in fig. 1), lined with blackened river stones sunk in the sand beneath our bottom layer, contained disintegrated charcoal and a granite lamp whose underside had been fashioned into broad facets, apparently by rubbing or grinding.

Rapid decay of organic matter in the sandy soil of our archaeological beds had resulted in total disappearance of the admirable artifacts of bone, antler, and ivory (tools, weapons, engravings, and sculptured forms) such as at sites in calcareous soil, like that of Dordogne, serve as a means of distinguishing between the different epochs of the Upper Palaeolithic and also between the different levels of those epochs. Of the remains of fauna, too, we consequently found almost nothing—a few fragmentary molars of horse and bison. The culture brought to light was, however, recognizable as Magdalenian by the evolved style of the animal engravings on stone, here-with reproduced, and by the presence of flint implements of certain types peculiar to that horizon. The most characteristic of these are the short, flat scraper, with abrupt, retouched edges, called raclette (fig. 4, nos. 3, 4); the dentate blade, with or without retouched margin (fig. 3, no. 1; fig. 4, no. 1) and the parrot beak graver, of which we found only one, an undeveloped specimen. The other types of the Magdalenian flint industry

6 A drawing of a good specimen of the parrot beak graver may be seen on p. 192, Vol. 1, of Dr G. G. MacCurdy's useful "Human Origins" (New York and London, 1926).

The drawings of flints reproduced in our Figures 3 and 4 (except fig. 4, no. 4) are by the skilled hand of Abbé J. Bouyssonie.
Fig. 3. Flint implements from the three upper levels, Cave of Puy-de-Lacan (Corrèze). Level D: 1, dentate blade; 2, 3, borers. Level C: 4, spalled graver (burin bec-de-flute); 5, 6, end scraper combined with graver; 7, 8, borers; 9, double nucleiform scraper; 10, end scraper. Level B: 11, carinate graver (burin caréné); 12, 13, blades with retouched margin; 14, end scraper combined with graver; 15, nucleiform scraper, called “plane” (rabot); 16, core (nucléus); 17, retouched flake; 18, multiple graver.
had already appeared in the Aurignacian and most of them in the Solutrean, which latter is distinguished from both by peculiar forms showing a special

![Flint implements from lowest level, Cave of Puy-de-Lacan (Corrèze). Level A: 1, dentate blade with retouched margin; 2, blade with retouched margin; 3, 4, flat scrapers with abrupt retouched edges (raclette); 5, scaled graver (burin à troncature retouchée); 6, Mousterian side scraper (râdoir); 7, 8, blades with retouched margin; 9, double end scraper; 10, oval scraper; 11, retouched flake; 12, unretouched flake.](image)

The general resemblance of our flint material to that of Aurignacian sites in the region of Brive is frequently striking. One could hardly
escape the impression that the artisans of Lacan were heirs of the Aurignacian tradition of flint work, although they no longer produced certain tools characteristic of the typical Aurignacian, such as the so-called "busked" graver with a notch, the "strangled" blade, and the true Tarté scraper.

With a few exceptions, flake tools of the Upper Paleolithic are smooth and unretouched on one face, the same being the inner face as fractured from the core, with a bulb of percussion often showing near the end where struck with a hammer stone (percuteur); while the other or outer face usually shows one or more longitudinal ridges produced by primary flaking before removal from the core (fig. 4, no. 12).

**TABLE 1. TOTALS OF FLINTS FROM ALL LEVELS EXCAVATED**

<table>
<thead>
<tr>
<th>Description</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Gravers (burins)</td>
<td>2893</td>
</tr>
<tr>
<td>2. End scrapers (grattoirs sur bout de lame)</td>
<td>1081</td>
</tr>
<tr>
<td>3. Scrapers with abrupt edges (raclettes)</td>
<td>5</td>
</tr>
<tr>
<td>4. Thick scrapers with bulb (grattoirs épais avec bulbe)</td>
<td>7</td>
</tr>
<tr>
<td>5. Nucleiform pieces (pièces nucléiformes)</td>
<td>1073</td>
</tr>
<tr>
<td>6. Borers (perçoirs)</td>
<td>195</td>
</tr>
<tr>
<td>7. Blades with retouched margin (lamelles à bord abattu)</td>
<td>172</td>
</tr>
<tr>
<td>8. Retouched flakes (lames retouchées)</td>
<td>328</td>
</tr>
<tr>
<td>9. Unretouched flakes (lames sans retouches)</td>
<td>818</td>
</tr>
<tr>
<td>10. Flakes with crimped crest (pièces à crête écrasée)</td>
<td>120</td>
</tr>
<tr>
<td>11. Flakes with truncated, splintered ends (pièces esquillées)</td>
<td>17</td>
</tr>
<tr>
<td>12. Burin spalls (lamelles de coup-de-burin)</td>
<td>1302</td>
</tr>
<tr>
<td>13. Tablets (tablettes)</td>
<td>62</td>
</tr>
<tr>
<td>14. Moustierian pieces (pièces moustériennes)</td>
<td>5</td>
</tr>
<tr>
<td><strong>Total without chips (éclats)</strong></td>
<td>8078</td>
</tr>
<tr>
<td><strong>15. Chips (éclats)</strong></td>
<td>16743</td>
</tr>
<tr>
<td><strong>Grand total</strong></td>
<td>24821</td>
</tr>
</tbody>
</table>

The principal flint tool of the Magdalenian, brought to a high degree of perfection in Aurignacian times but less used by the Solutreans, was the graver or burin. This implement, long supposed to have served especially for engraving, was also and no doubt chiefly employed for working bone and probably wood. The essential feature of the graver is a short, robust cutting edge made across the thickness of the flake at one of the extremities or angles. Gravers may be grouped in three general categories, of which characteristic forms are illustrated in our drawings as follows (mentioned here in order of their frequency at Lacan): I, the spalled graver (burin bec-de-flûte), Fig. 3, Nos. 4, 5 (upper end, angle at left), and 6 (lower end); II, the scaled graver (burin à troncature retouchée), Fig. 3, Nos. 14 (upper end) and 18 (upper end), also Fig. 4, No. 5 (upper end); III, the fluted graver (carinate type, burin caréné), Fig. 3, No. 11. These three general categories
of gravers, as classified by Mr H. V. V. Noone, comprise in all sixteen different types found in the Upper Palaeolithic.  

The Magdalenians seem to have been the first to make general use of the graver for cutting elongated pieces from bone or antler for the production of harpoons, spear points, needles, etc., by working the short transverse edge back and forth in parallel grooves. The photograph (fig. 2, lower) shows the process uncompleted on a piece of reindeer antler unearthed in Magdalenian III at Laugèrie Haute, Dordogne.  

At Lacan, gravers run to 35.8 percent of the flint industry, without counting the chips, and to 52 percent of the total number of tools, without counting burin spalls, tablets, flakes with crimped ridge, etc.  

In 1930, we dug three remaining portions of the deposit (one about the dead oak, two along the sides) which we had intended to leave as "witnesses" (témoins) of the stratigraphy, but which had already begun to give way under the rains of the preceding winter, so that we saw they would not endure. Finally, we emptied the cave of sand varying in depth up to a little over a meter.  

On the rock floor within, thus exposed, we found to our surprise unquestionable traces of metal work, where apparently quarry men had tried the quality of the sandstone and had grooved channels to drain off seepage water. There could be no doubt that the ground in the cave had been turned over to the bottom. Both in the cave, however, and also outside, in the same bed, D, extending on to the talus, we found numerous Magdalenian flint implements mixed with others of Mesolithic character and with potsherds of historic age, Gallo-Roman and Merovingian. Two inferences seemed clear: that a Magdalenian deposit formerly existing in the cave had been partly but never entirely emptied on to the talus, and that this bed, D, had, strictly speaking, no stratigraphic significance. In other words, the

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8 A standard French work on gravers is Captain M. Bourlon's "Essai de Classification des Burins" (Revue de l'Ecole d'Anthropologie, No. 7, Paris, 1911). An even more comprehensive study is H. V. V. Noone's "Classification of Burins or Gravers" (Journal, Royal Anthropological Institute, Vol. 64, Jan.–June, 1934). Mr Noone's essay has also appeared in French, in Compte Rendu du Congrès Préhistorique de France (Périgueux, 1934).  

We have borrowed the English terms "spalled" and "scaled" from Noone, the French terms from Bourlon and from Bardon and Bouyssonie.  

7 By Miss Barbara Kiddier, who has kindly allowed us to reproduce the picture. To obtain elongated pieces for making tools and arms, the Aurignacians and Solutreans split bones, apparently with flint wedges. Two instances have, however, been recorded where Aurignacians (Perigordians) used the graver to remove longitudinal cuttings from reindeer antler by making parallel grooves. Each case was that of a single specimen, one observed by Denis Peyrony in the top layer, Upper Aurignacian (Périgordien V) at La Ferrassie; the other by his son, Elie Peyrony, in the same level at La Forêt, both in Dordogne.
age of objects found in this layer could be determined only by comparison with types of known age.

The underlying beds, C, B, and A, were undoubtedly in place. Notwithstanding the mobility of their sandy composition on a talus washed by rain and by water from the cave, these three layers had retained a stratigraphic character that appeared in the homogeneous nature of their archaeological contents and in progressive changes, from level to level, in the dimensions, relative frequency, and to some extent in the form of certain flint implements—changes that run more or less regularly through A, B, and C, but are usually arrested in D. An example is seen in the similarity of variation in the average lengths of gravers and of end scrapers, shown graphically in

![Graph showing average lengths of gravers and end scrapers]

Fig. 5. Average lengths, in millimeters, of gravers (unbroken vertical lines) and of end scrapers (dotted vertical lines) in the successive levels.

Figure 5. As an example of progressive changes in relative frequency, the percentage of multiple gravers (those with more than one working member, fig. 3, no. 18) decreased with the lapse of time: A, 34.9; B, 27.5; C, 24; while that of broken gravers (fig. 3, no. 4) increased: A, 33.7; B, 43.5; C, 44.2. Similarly, the percentage of double end scrapers (fig. 4, nos. 9, 10; characteristic rather of the Aurignacian) diminished rapidly: A, 13.3; B, 8.8; C, 4; while that of end scrapers associated with gravers (fig. 3, nos. 5, 6, 14; fig. 4, no. 5; a favorite combination in Magdalenian times) kept augmenting: A, 23.3; B, 39; C, 43.9. Not to multiply instances, the foregoing may suffice to illustrate the stratigraphic individuality of the three lower beds and the continuity of cultural changes that developed during their formation.
Similar phenomena, which could be expressed by curves, may be traced in the percentage of most of the items in Table 2.

**TABLE 2. PERCENTAGES OF THE SEVERAL CATEGORIES MAKING UP THE TOTAL OF FLINTS FROM EACH LEVEL**

<table>
<thead>
<tr>
<th>Category</th>
<th>A</th>
<th>B</th>
<th>C</th>
<th>D</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Gravers</td>
<td>22.5</td>
<td>31.1</td>
<td>38.9</td>
<td>32.4</td>
</tr>
<tr>
<td>2. End scrapers</td>
<td>13.4</td>
<td>9.2</td>
<td>9.1</td>
<td>10.5</td>
</tr>
<tr>
<td>3. Scrapers with abrupt edges</td>
<td>0.3</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>4. Thick scrapers with bulb</td>
<td>0.0</td>
<td>0.2</td>
<td>0.0</td>
<td>0.1</td>
</tr>
<tr>
<td>5. Nucleiform pieces</td>
<td>9.7</td>
<td>10.1</td>
<td>11.0</td>
<td>16.1</td>
</tr>
<tr>
<td>6. Borers</td>
<td>1.1</td>
<td>1.5</td>
<td>1.8</td>
<td>2.4</td>
</tr>
<tr>
<td>7. Blades with retouched margins</td>
<td>4.9</td>
<td>1.5</td>
<td>0.8</td>
<td>1.5</td>
</tr>
<tr>
<td>8. Retouched flakes</td>
<td>5.7</td>
<td>4.2</td>
<td>2.6</td>
<td>3.4</td>
</tr>
<tr>
<td>9. Unretouched flakes</td>
<td>29.5</td>
<td>26.0</td>
<td>16.8</td>
<td>24.6</td>
</tr>
<tr>
<td>10. Flakes with crimped crest</td>
<td>1.9</td>
<td>1.0</td>
<td>1.6</td>
<td>0.8</td>
</tr>
<tr>
<td>11. Flakes with truncated, splintered ends</td>
<td>0.5</td>
<td>0.1</td>
<td>0.0</td>
<td>0.2</td>
</tr>
<tr>
<td>12. Burin spalls</td>
<td>7.9</td>
<td>14.1</td>
<td>16.8</td>
<td>6.8</td>
</tr>
<tr>
<td>13. Tablets</td>
<td>2.5</td>
<td>1.0</td>
<td>0.6</td>
<td>0.9</td>
</tr>
<tr>
<td>14. Mousterian pieces</td>
<td>0.1</td>
<td>0.0</td>
<td>0.0</td>
<td>0.3</td>
</tr>
</tbody>
</table>

The two engravings here reproduced, one representing bison and a bird, the other three young bison, are lightly traced on small slabs of fine-grained sandstone (pls. 12 and 13). Unearthed just below the surface of layer C, near the rock and not far from each other, they were possibly by the same hand but are evidently of the same school, that of the great period of Magdalenian art. Abbé Breuil attributes them to Magdalenian V–VI. In contrast to the mastery shown in these drawings is the immaturity of a horse's head carved in relief on a small block of sandstone picked up in earth that had fallen from the side of our exploring trench. It is in the characteristic primitive style of sculptured horses' heads from Magdalenian III–IV, found in the Valley of the Vézère by M Peyrony and others and at Isturitz by Count de Saint-Périer, who considers the profile on our block to be of approximately the same age. Though not found in place, it would seem to have come from our lowest bed, A. Its presence at Lacan, in relation to the developed technique of the two engravings, is of interest as apparently bearing out the supposition that in the region of Brive Magdalenian art evolved along lines analogous to those already known in Périgord and in the French and Spanish Pyrenees.

Apart from engravings unquestionably Palaeolithic, one of us (L.K.) unearthed some fifty stones and a potsherd bearing incised signs different
from anything previously signalized in the country. Among them are characters or letters, some of these forming apparently lineal inscriptions; two stones are engraved with conventionalized human visages, others with cupules in various arrangements. The publication of the signs, illustrated by line drawings in the Revue Archéologique, gave rise to the expression of very different opinions regarding their age. They were variously ascribed to Glozelian culture in late Magdalenian or early Neolithic times, to Gallo-Roman magic (supposed according to this view to have been practised in the Cave of Lacan), and to a survival in the Bronze Age or even later of conventionalized anthropomorphic figures of Megalithic origin. As the age of these signs is a problem outside of the field of our own studies, we limited ourselves, in our article, to describing the specimens and the stratigraphic conditions in which each piece came to light. The impression has grown upon us, however, that one has here not the product of a single epoch but of each of the periods to which the signs have been attributed. Observers are agreed that there is no doubt of their authenticity. We found them apparently in place in all levels, the largest number being in D.

The question to which of the six levels of the Magdalenian the three dug by us belong—a complex question, made none the simpler by the disappearance of bone artifacts, which characterize the different levels—will be taken up again when the work at the sand pit has brought to light the remaining vestiges of prehistoric deposits on the talus. Meantime, indications observed in the course of our work may here be stated.

The finding of raclettes (flat scrapers with abrupt edges, characteristic of the old Magdalenian) in our lowest bed, A, seems to show that the site was inhabited at that level before the introduction of the primitive bone harpoon in Magdalenian IV, and may possibly have been occupied even earlier than Magdalenian III. On the other hand, the presence of examples of fully evolved art in level C shows that Lacan was inhabited up to Magdalenian V—we should say also in VI were it not that the site has yielded us but one, under-developed specimen of the parrot beak graver, a type particularly characteristic of that level. With a good deal of doubt regarding occupation in VI, we should therefore place our three undisturbed levels as approximately contemporaneous with two well known Magdalenian sites in

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9 Dr A. Morlet in Le Mercure de France (Paris, June 1, 1932).
10 M. C. B. in Man (Nov., 1933).
11 Commandant F. C. E. Octobon in “Homenagem a Martins Sarmento” (Guimares, Portugal, 1933).
Dordogne, Laugerie Basse (III–VI) and La Madeleine (IV–VI), as also with Magdalenian III, and possibly II, at Laugerie Haute. Mesolithic types, including microliths, in the sand of level D suggest an occupation, perhaps after an interval, by Tardenoisians. And finally, historic pottery shows that Lacan was frequented in Gallo-Roman and in Merovingian times.

The material excavated at Lacan is in the Musée Ernest-Rupin at Brive, except the engraving of the three young bison (plate 13), which is in the possession of Baron Le Clère, at Payzac (Dordogne).

Besides much help given us by French prehistorians, particularly by Chanoines A. and J. Bouyssonie and M D. Peyrony, in the study of this material, as also by Professor A. S. Barnes in the analysis of our statistical observations, we wish to acknowledge generous financial aid given unasked by Mr William North Duane.

Les Eyzies, Dordogne
France
Engraving of bison and bird on a small slab of fine-grained, reddish-gray sandstone. Magdalenian V–VI. From the prehistoric site of Puy-de-Lacan, Corrèze, France.
Engraving of three young bison on a small slab of fine-grained, gray sandstone. Magdalenian V–VI. From the prehistoric site of Puy-de-Lacan, Corrèze, France.
Fig. 7. Line drawing of engraving of three young bison shown in Plate 13.
ETHNOLOGICAL AND HISTORICAL IMPLICATIONS
OF CERTAIN PHASES OF MAYA POTTERY DECORATION

By MARY BUTLER

NEGATIVE PAINTING

FROM the ethnological point of view, the most striking decorative feature of Piedras Negras pottery is the prevalence of negative painting in polychrome wares. This consists of painting a design on a vessel in a resistant substance such as hot wax, covering the vessel with a darker coat of color, and subsequently melting off the resistant material, revealing the design in the lighter color. Such a process has been called "true" negative painting to distinguish it from the "false" method of painting in directly a darker background around an undisturbed lighter design. It is used today in Guatemala and Salvador to decorate gourds. Piedras Negras is the only Old Empire site where it appears as a characteristic of polychrome pottery. None of the published material from the Peten shows more than an occasional occurrence of it. An unpublished collection from the Atlantic Highland area, now in the University Museum, Philadelphia, is characterized by negative-painted pottery, but we cannot be sure of the time levels represented. The negative-painted wares from this district differ in style and coloring from the Piedras Negras ware. What specimens we have of negative painting from the rest of the Maya area tie into either the Piedras Negras or the Highland group.

As far as we know, its principal use in this area in prehistoric times was in the Old Empire time level and Highland derivations from it. It is not a characteristic of "Archaic" pottery, since Usulutan ware, which superficially bears it some resemblance, owes its negative stripes to painting of a pattern in a fugitive color. It is not a characteristic of Yucatecan pottery.

Although in Mexico, too, it has been thought not to occur in archaic levels, Dr Vaillant reports a few examples in "Archaic" material from Ticoman and Gualupita. However, we can no longer think of archaeological

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1 The various points discussed here were brought out by a study of the pottery found in 1931-32 at the Maya Old Empire site of Piedras Negras, Peten, Guatemala, but were not included in the report on that material (M. Butler, Piedras Negras Pottery, Piedras Negras Preliminary Papers, University [of Pennsylvania] Museum, No. 4, 1935), since they were theoretical matters, of general rather than specific interest. They were discussed in an unpublished, philosophical, Introduction which served to make the report acceptable to the Department of Anthropology of the University of Pennsylvania as the writer's doctoral dissertation. Another section of this Introduction has been published as Pottery Study in the Maya Area (American Antiquity, Vol. 2, No. 2, 1936, in press).


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objects as occurring throughout the Valley of Mexico in three neatly superimposed layers, with “Archaic” at the bottom, Toltec above, and Aztec on top. We know the approximate time range of Aztec material, we know that Toltec began before Aztec, and “Archaic” before Toltec, but we cannot tell yet how long “Archaic” and Toltec may have persisted from site to site. According to tentative dating suggested for the “Archaic” sites referred to, the negative-painted sherds found there may well be considered as no earlier than the second half of the first millenium A.D. Negative painting occurs frequently on pre-Toltec pottery at Teotihuacan, infrequently on Toltec pottery. It is not a characteristic of typical Aztec wares. The Mexican sites where it has been found in any quantity are outside the Valley of Mexico, in the states of Mexico, Jalisco, and Michoacan, and the time relationships of wares found there are not yet fully determined.

While there will be definite historical significance to the distribution of negative painting, as further archaeological work enables us to trace it, the appearance of this type of pottery painting is exceedingly interesting from the technical side alone. It is a solution of the kind of problem that fascinates engineers today; the successful application of a technique usual in one process to material to which it is naturally foreign. It is an instance of invention changing pottery-making, but any conclusions reached as to its origin must at present be purely hypothetical.

Painting pottery with hot wax or a similar resistant substance is not a process native to pottery decoration, nor one that would have evolved naturally from it. It could have had one of two origins: observation and utilization of the properties of hot wax or resin, or application to pottery painting of a technique used in some other craft. We know that the early Maya were great bee-raisers, but we do not know their methods, nor what use, if any, they made of beeswax. They are not supposed to have used candles. Thompson, however, points out that they may have had them and that wax must have been used ceremonially in Mexico, since it has been found in candeleros, small pottery objects of unknown purpose presumably

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5 E. Noguera, Antecedentes y Relaciones de la Cultura Teotihuacana (El Mexico Antiguo, Vol. 3, Nos. 5-8, Mexico, 1935).
ceremonial. It may be, then, that negative pottery painting originated in Mexico through observation there of some use of hot wax, or of the potentialities of its properties. On the other hand, we cannot ignore other possibilities.

We do not know what arrangements the Maya had for artificial lighting. They may have used resinous torches, although these are hardly practical for giving steady light in a small house. If so, drops from one of these torches may have fallen on a vessel, before it was painted; the potter, observing the result after painting and firing, might see the possibility of turning such an error into a process for pottery decoration. The use of hot wax as a medium, however, is unusual, and when there is near at hand a craft to which it is necessary, the possibility of some connection between the two uses of the medium seems more plausible than independent invention.

The cire perdue process of metal-casting was known in South America, Central America, and Mexico at the time the Spaniards came. We do not know how long before that it had been in use. Metal-casting has been considered as spreading from a center of origin in northern South America. We do not know when or how it reached Mexico, if it spread by land, since there is so far no evidence of metal-working in the Maya Old Empire, and only two or three gold ornaments have been found at sites of that period. What metal there is from Yucatan, apart from Central American importations, may well be due to Mexican influence. Landa refers to the Mexican origin of the small copper axes used as one form of currency there in the sixteenth century. The knowledge of metal-working may have been transmitted into Mexico from Central America along the cordilleras, after Maya power had shifted north into the geographically peripheral area of Yucatan. The occurrence of metal objects in the Highlands of Guatemala would bear this out.

The lack of metal and the appearance of negative painting in the Maya Old Empire mean that the second is not a corollary of the first. Some one in Central or South America, where metal was being worked and the cire perdue method invented, may have had the wit to apply this unusual medium to pottery decoration. Its use would then have spread independently of metal or metal-working, since it had now become a phase of pottery decoration and had no further connection with metal.

The whole question of derivation hinges on the date of the origin of metal-casting, and this cannot be known until more work has been done on South American archaeology. Nordenskiöld thinks copper and bronze were characteristic of the later cultures in Peru, but implies that these cultures

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may have covered a long period of time.7 According to Rivet,8 the gold work which flourished in Colombia, as a result of trade knowledge brought by Caribs from the hinterland of Guiana, is a good deal older, since examples of it are found on the Peruvian coast in the Proto-Chimu period. Tello reports gold objects from his first epoch, the archaic culture, which consist of ornaments in the Chavin style from tombs at Chongoyape, Lambayeque, on the northern coast of Peru.9

Negative painting is found in the Mexican-Maya area during the first half of the first millenium A.D., and if the tentative dating previously referred to should hold, may have reached the "Archaic" Mexican levels by way of the Maya Old Empire.

Dr Linné has suggested the derivation of negative pottery painting from resist-dyeing.10 This technique was known in pre-Columbian America only in its tie-dyeing phases: tie-dyeing of material already woven, and ikat, the application of the same process to the threads before weaving. In this method, it is necessary only to wrap sections of the cloth or thread with some material, such as fibre, that will resist dye. It does not require the use of hot wax or some similar resistant substance in temporarily liquid form such as is used in the batik method. In fact, Dr Linné says that he has no information of wax ever having been used in preparation of the binding thread for tie-dyeing, but considers it not impossible that it was so used. It seems to me that a technique, such as metal-casting, in which hot wax, or a similar substance, was a constant factor, is a more likely source of derivation for negative painting than a technique in which it may never have been used. Tie-dyed designs may have furnished the inspiration; tie-dyed technique does not seem apt to have furnished the means.

NEGATIVE DESIGN

An interesting corollary to the appearance of negative painting among the highest civilizations of pre-Columbian America is the possible significance of the distribution of negative design on pottery throughout pre-Columbian America. To work the background rather than the design, so that the latter stands out lighter than the former, is an inverted sort of idea which may have originated independently in different areas, but which is striking enough to warrant consideration of the possibility of its diffusion

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7 E. Nordenskiöld, The Copper and Bronze Ages in South America (Comparative Ethnographical Studies, Vol. 4, Göteborg, 1921).
9 J. Tello, Antiguo Peru, Primera Epoca (Lima, 1929), Fig. 107.
10 Linné, op. cit., Appendix 1.
from the negative painting belt, which stretches from Jalisco, Mexico, to Paracas, Peru.\(^1\)

We can find in the Maya area the whole range of negative design on pottery. In painted wares, we find negative painting, "true" and "false;" in carved wares, we have champ-levé, a raised design with incised details against a flat background; in incised wares, we have figures outlined against punctate and hatched backgrounds. All these techniques use both geometric and naturalistic designs, and all furnish examples of negative design. The champ-levé pottery which bears naturalistic designs may here be taken as a derivative of carved pottery with details in low relief, since a steady development can be traced from one to the other. Pottery carved or stamped with figures in low relief may be due to imitation of stone sculpture or painted walls or vessels. Whatever the inspiration, the technique is the same as that of stone carving and is probably derived from it. Champ-levé, in the Maya area at least, seems to be a modification of this technique. It is infrequent in the Old Empire, and cannot be an original source for negative design.

Toltec champ-levé, where the design is usually a dark one that stands out from a red-painted background, and cloisonné ware, a specialized Mexican development of the champ-levé technique, imitate painted pottery. While the technique is a negative one, the design produced, in Middle America outside the Maya area, tends to be positive. In some parts of the Maya area, even today, negative painting and champ-levé are used on gourds to produce negative designs. These techniques apparently did not spread north of Mexico, although a few vessels and a piece of textile from southeastern United States suggest decoration by negative painting, probably "false."

The derivation of negative design on incised wares may lie either in negative-painted or in champ-levé design. The first possibility would make it a direct development of negative painting, the second a parallel development.

The interesting thing about negative design is its distribution in North America. In the Maya area it appears in the Highlands, Chiapas, and Yucatan, in designs, that, when naturalistic, are definitely decadent and suggest a relatively late time level. A jar from Tzimin Kax with such decoration is assigned by Thompson to the local Holmul V period.\(^2\) It is not a characteristic of Mexican pottery nor of that of southwestern United States, but it

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\(^1\) *Ibid.*, Map 1, p. 166.

does appear on the pottery of the important Hopewell culture of the mound region of the Mississippi Valley where there is a complex of traits showing artistic connections with Mexico. It is again evident in the underlying archaeological layer in southeastern United States that is considered by Setzler to be related to the Hopewell culture. This is an area that is steadily growing in archaeological importance, and that also contains material that suggests contact with Mexico.

POLYCHROME DECORATION

Considering in some detail the historical implications of various phases of Maya pottery decoration, it is obvious that polychrome wares are the significant phase of painted decoration for diagnosis. Piedras Negras polychrome may be roughly divided into a red group and an orange group, according to the background color used. Negative painting is, with two exceptions, confined to red polychrome, which comprises two-thirds of the polychrome group and may at present be considered local, in contrast to the orange polychrome that is found in varying styles throughout the Maya area. The negative-painted ware that forms a large part of Piedras Negras red polychrome is at present unique in style. It consists of white and orange-yellow designs against a red-orange background, painted by the "true" negative technique, with, sometimes, an apparent use of the "false" technique for the final red coat. The designs in this are simple silhouettes, chiefly variations of the circle. There is an elaboration of this type where black line decoration is inserted in the light areas, which have become fewer and simpler. There is a small group, entirely negative-painted, with white and orange disks against a brown or brown-black background, which appears elsewhere. A sherd from Hochob (in the collections of the American Museum of Natural History), and the exterior of a bowl, with polychrome interior, from Copan (Peabody Museum), have the same type of decoration.

The only other collection of Maya pottery in which negative painting is an outstanding feature is that from the Alta Vera Paz region, now in the University Museum, Philadelphia. The designs here are mainly white against black, although there are one or two crude examples where the under-slip is red. The designs are more sophisticated than the Piedras Negras ones cited and include birds as well as fairly elaborate geometric

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13 F. M. Setzler, Pottery of the Hopewell Type from Louisiana (Proceedings, United States National Museum, Vol. 82, Article 22, 1933).
14 Butler, op. cit., Pl. 1, Fig. 1, 2, 6.
15 Ibid., Pl. 1, Fig. 8, 14, 15, 18.
16 Ibid., Pl. 1, Fig. 7.
motifs such as a fret-and-dagger. The two examples of "true" negative painting from Holmul III are in this white-and-black style, one vessel having a geometric design, the other a monkey. Since such decoration is frequent in the Alta Vera Paz Highland region, and unusual in the Peten, it seems probable that these two Holmul examples are due to influence from the Alta Vera Paz.

The inferences to be drawn at present about "true" negative painting in the Maya area are that there are two main types: a dichrome, centering in the Alta Vera Paz and a trichrome, centering in Piedras Negras. While there is little more of the distributed trichrome type at Piedras Negras than at the other two sites mentioned, it is at that city one phase of a well-known technique, while at the other cities it forms the only example of that technique. Almost any digging done in Old Empire sites may change the emphasis of this distribution, but it is possible that the type division indicated will persist.

Illustrations of straight-sided bowls from Nakum, Holmul, and Uaxactun suggest that their decoration is done by "false" negative painting. The Nakum and Uaxactun bowls may represent a Peten black-and-white style in this technique; one Uaxactun bowl suggests a development from Piedras Negras Polychrome A.

An outstanding feature of Maya polychrome pottery is what may be called the Chamá style of figure painting. It might be said to represent the highest development of pre-Columbian American painted pottery because of the feeling shown for line and color, and the skill, vigor, and conciseness of execution of the figures drawn. While this style has been considered as a presumably late Highland development from Old Empire pottery, three examples of it, undoubtedly trade pieces, have occurred at Piedras Negras, where they definitely belong to the time of the city's main occupation. Two

18 G. C. Vaillant, The Chronological Significance of Maya Ceramics (MS., Thesis submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Harvard University, 1927), Figs. 245–47.
19 Merwin and Vaillant, op. cit., Pl. 31,a.
20 E. B. Ricketson, Notes on the Pottery of the House Mounds of Uaxactun (in House Mounds of Uaxactun, Guatemala, by Robert Wauchope: Contributions to American Archaeology, 7, Preprint from Publication 436 of the Carnegie Institution of Washington, 1934), Fig. 31,c; A. L. Smith, Two Recent Ceramic Finds at Uaxactun (Contributions to American Archaeology, 5, Preprint from Publication 436 of the Carnegie Institution of Washington, 1932), Figs. 3,b, 4,e.
21 Smith, op. cit., Fig. 4,e.
22 Merwin and Vaillant, op. cit., pp. 81–82.
of these sherds show human figures, another a parrot wing. Both motifs are rare on Piedras Negras painted pottery, where there is relatively little naturalistic design, due, possibly to the geometric cast given to pottery decoration by the type of negative painting used there.

A variant of this parrot design occurs on a characteristic tetrapod bowl from Holmul I, and another on a shallow plaque from a Rio Hondo mound in British Honduras, where polychrome painting is thought to have derived from Holmul V. There seem to be two types of polychrome design running side by side through the first four Holmul periods; a geometric one, and a naturalistic one, that is markedly different in style from the naturalistic decoration characteristic of Holmul V, Yalloch, and the Rio Hondo. The parrot wing design from Rio Hondo suggests a survival there of the earlier as well as the later Holmul polychrome style. Although the Chamá trade pieces at Piedras Negras suggest an earlier date for Chamá polychrome than that referred to above, elements of style and shape preclude its preceding Holmul I, and supplying the source for the Holmul I parrot. The Holmul polychrome style to which this design seems to belong may perhaps show another line of development from the same source that inspired Chamá polychrome. It may possibly have supplied that source.

The Copan polychrome style, divided by Dr Vaillant into three stylistic groups that seem to have chronological sequence, marks the western edge of the southeastern Maya polychrome pottery group. A few trade pieces of Copan I appear at Piedras Negras, and three bowls that suggest local copies of Copan polychrome.

We have, then, in the very nature of the polychrome decoration at Piedras Negras, evidence of a specialized local development of a technique of widespread historical significance, negative painting. While its significance within the Maya area can at present be only suggested, it already indicates contact between Piedras Negras, Copan, and Hochob. The contact with Copan is supported by the evidence of polychrome style, which also serves to show trade with the Alta Vera Paz.

Negative-painted pottery appears also in Mexico, though no quantity

23 Ibid., Pl. 18.f.
24 T. Gann, The Maya Indians of Southern Yucatan and Northern British Honduras (Bulletin, Bureau of American Ethnology, 64, 1918), Fig. 52.e.
26 Ibid., Pls. 18,b, d; 19,c; 28,h.
27 Ibid., Pls. 18,c, f; 20,e; 28,g.
28 Ibid., Pls. 29,a, c; 30.
30 Vaillant, MS., 1927.
comparable to that from Piedras Negras has been found at a site that can be dated as early as the Maya city. It seems possible that its earliest appearance in Mexico is no earlier than, if as early as that at Piedras Negras, and that its presence at that city may show it to have spread in the Mexican-Maya area from the Maya Old Empire. If that is the case, its appearance there, seen in relation to pre-Columbian America, shows that the technique spread north through the Maya Old Empire in its diffusion north and south from a probable place of origin in northern South America or in Central America, to the limits of Jalisco and Paracas as indicated by Linné, and possibly beyond.

Any significance in the variations indicated in polychrome style is not capable of being pushed beyond the Maya area. Polychrome pottery painting is a widespread trait that is almost bound to appear where good pottery is made. The styles that developed in the various ceramic areas in Middle, Central, and South America are well defined and distinct from each other, and the local variations within each area are important only for that region, unless we discover them to have covered a far greater range through trade communications. This has not as yet been shown to be the case with any of the styles cited.

CUT DECORATION

The most interesting phase of cut pottery decoration at Piedras Negras is the low-relief carving of scenes containing human figures that occurs on several sherds of Orange 3 bowls. This is a type of pottery decoration that may well have derived from the stone and wood carving so highly developed during the Old Empire. The earliest examples bear a definite resemblance in style and subject matter to Old Empire stelae and lintels; later ones show the same loss in skill that is apparent in Yucatecan stone figure carving. The sherds from Piedras Negras are carved in the Old Empire style, like sherds from Nakum, Copan, and Palenque, and are made of a fine orange paste. Similar carved bowls, made of similar paste, come from Yaxchilan (in the Peabody Museum), Kixpek in the Highlands (University Museum), and the Toltec level at Xolalpan near Teotihuacan.21 Associated with these sherds at Xolalpan was similar ware carved in a style very like that of distinctive black bowls found by Gann in British Honduras.22

Champ-levé, a cutting away to an even depth of the background of a design, occurs occasionally at Piedras Negras in fragments of geometric design. This technique does not really come into its own in the Maya area until the Yucatecan period, when it is used for fine reproductions on pottery

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21 Linné, op. cit., Fig. 130.
22 Gann, op. cit., Pl. 17–18.
of figures of men and gods. It seems at present as though fine polychrome wares were replaced to a large extent among the later Maya by carved wares, and one can trace a definite sequence from such vessels, with details carved in fairly high relief, through vessels on which the detail relief grows lower and lower, to vessels on which the detail is all incised, and the decoration a champ-levé design on only two planes. The suggestion would be that in the Maya area champ-levé decoration on pottery was an outgrowth of relief carving in the same medium.

The particular type of carved ware found at Piedras Negras seems to belong to the Usuchacinta drainage. Its appearance at Xolalpan associated with similar ware carved in a style characteristic of a British Honduras site suggests that the two may perhaps be contemporaneous, and may serve to date the latter site.

Relief carving of scenes on pottery may very well have been a specialized development in the Maya part of Middle America, and have been a result of Maya influence when found elsewhere. When it appears at Teotihuacan, it is obviously exotic. A few specimens have been found in southern Mexico, but their style suggests the Maya even when the figures shown wear Toltec costume. The one from Monte Alvan is very possibly a trade piece of Yucatecan Carved Slateware.

Relief carving on pottery seems to be lacking in Central America south of the Maya area. It occurs in Peru, but usually in the modified form of relief on mouldmade vessels. There is probably no connection between this and Maya pottery carving.

Champ-levé, on the other hand, flourished in Mexico. It is a characteristic of the Toltec pottery of Teotihuacan, appears in Oaxaca, and is the basic technique that produced the cloisonné pottery of the Tarascan area. It is interesting to note, however, that Toltec champ-levé and the Mexican cloisonné usually imitated, in their coloring, positive-painted design. It seems possible that the technique may have been introduced into Mexico from the Maya area, since it does not occur in Mexican “Archaic” levels. Champ-levé design occurs from Mexico to Peru and Bolivia, and even in Amazonian material from Marajó. It may possibly have diffused from one source, and if this should be relief carving on pottery, that source may have been in the Maya Old Empire.

University Museum

Merwin and Vaillant, op. cit., p. 79.
ADDITIONAL NOTES ON THE ISLAND
CARIB OF DOMINICA, B.W.I.

By DOUGLAS TAYLOR

An earlier article published in the AMERICAN ANTHROPOLOGIST\(^1\) was written rather hurriedly after several cursory visits to the Carib Reserve of Dominica, British West Indies. Having since then been able to spend some consecutive weeks among this interesting group, I now take the liberty of offering further comments and corrections together with an additional vocabulary.

Physically, the Dominican Caribs are a small though sturdy race—the men averaging around five feet four inches and the women about five feet one inch in height. They have sleek black hair, high foreheads, broad cheek-bones, straight nostrils, and rounded chins. Their eyes are long and narrow—though usually not oblique—with long silky lashes. Their ears are very long and often lobeless, their feet small and extraordinarily high-arched. The girls are broad-shouldered, tend to be plump, and have powerful but almost straight loins. Men and women alike have little or no body-hair. The full-blooded Indian's color is quite distinct from that of a mulatto, and is best described as resembling the dried bark of cinnamon. The shade varies with the occupation followed—fishermen are darker than woodsmen—and does not always indicate the purity of the strain. Like all others of their race, the Indians of Dominica are naturally shy and reticent, sensitive to an extreme, quick to take offense, and given to occasional moods of melancholy or unreasonableness.

Perhaps the most typical of their present-day products is the pagàra or double lined Carib pannier. These are made by a number of the men, some of whom show almost as much skill as their more primitive cousins of the Guiana mainland. The commonest variety is the rectangular "toilet-basket" measuring about twenty-four by thirty by twenty-four inches, with lid of almost the same depth as the body and very slightly larger in order to fit over the latter. Other forms are the "satchel," a flat handbag-shaped affair some ten inches long by two inches wide by eighteen inches deep. The sides taper slightly toward the top, which is some two inches shorter than the base. The "grip" is a modern adaptation introduced to suit the tastes of town-negro and tourists. All these baskets are made in the same manner, and each consists of four pieces: a body and a lid, plaited in a more or less complicated design with strands of two or three colors, duplicated by linings of the same material in close plain white weave. Between

the outside basket (body and lid alike) and the lining is placed an overlapping layer of cachibou leaves to ensure watertightness.

Hébichets or cassava-sifters, matûtu or Carib tables (a sort of low tabouret), and màtapi or cassava-squeezer (the typical stocking-like affair known here as “couleuvre”) are also made yet by a few of the older men.

All of the above are made from the stem of the luàrouman. This reed-like plant (the itirite of the Guiana Indians ?) is found in the high woods, cut to a length of about eight feet, and brought back in big bundles to the Reserve. Here it is dried in the sun until it acquires a reddish-brown tinge, or steeped in “mud-holes” to dye it black. The stems are then prepared for use by splitting them in four and removing the pith with the aid of a knife. The white inner side of the strands is usually placed uppermost in either the warp or the weft of most products.

The women weave simple round wicker baskets in a variety of shapes and sizes out of a liana called mibi. The strands are often dyed yellow and mauve with the fruit and leaves respectively of plants known locally as “saffron” and “tan.”

Other products are the catãoli or shoulder-carryall, made here of lattanier; kali, a kind of fish-pot used for catching flying-fish, and the sleeping-mat, no longer common, made from the ribs of balizier leaves.

Roucou (anatto) and cotton still abound in the reserve, but the women of today can neither dye nor spin, and the latter is now mainly used for caulking defective canoes.

The men still make their fishing-lines (nets are not used) in the old way out of what they call la pitte (kùrawa or silk-grass). A length of bush-rope (tressed strips of mahoe-bark) is attached to the fork of a tree and a slip-knot passed round the leaf, which is then folded over a thick round stick held in the hand. By an even pull, the leaf is made to pass through the knot, exposing the long fibre and leaving the waste matter on the rope. As the process continues the stick is twisted and the disengaged fibre wound around it. The latter, when dried and bleached, is spun into twine by rolling it on the naked thigh in a particular manner.

The tree Carapa guiensis is still found in the Dominica forests. Its fruit was formerly used to make the oil Caribs put on their hair and, mixed with roucou, on their bodies. Curiously, its name (carapat oil) is now given to the oil of the palma christi which the women still extract in a primitive way for anointing their hair. The men sometimes make shark oil for use in primitive lamps as a substitute for kerosene. The flambeau, now seldom seen, is a torch made from strips of a wood known as bois chandelle bound up in dried leaves together with lumps of white gum of the gommier tree. Such a
torch burns steadily for six or seven hours, and gives off a very pleasant aroma.

A few surviving customs are worth noting. Though not altogether confined to the Carib population today, they are, at any rate, of purely Indian origin.

At least two fish-poisons are known and commonly used in river-pools whenever the sea is too rough for fishing. That known as nivrage (from the French “énivrer”) is prepared by crushing the leaves of a shrub fairly common in the Reserve and probably identical with Carib kunâmi. The other and more powerful comes from the fruit of a tree called babarà. It may even, on occasion, be used in rock-pools of seawater.

Sorcery is not unknown, but has fallen into disrepute and become inextricably mixed with imported African and pseudo-Christian varieties. At the present time it is concerned chiefly with petty jealousies, love spells, the recovery of lost objects, and the like. The word piafe is used throughout Dominica in the sense of “spell” or “charm,” whereas obea (supposed to be African) and quimois (said to come from French “tiens bois!”) serve indifferently to denote the science and practice of sorcery. It is curious, though perhaps a coincidence, that these last two words so nearly resemble the Island Carib words (women’s and men’s languages respectively) for “spirit,” “understanding:” opoya and akàmbué.

Some Carib are reluctant to tell their names to strangers—or at least they retain a private name they will divulge only to their family. Though Carib names have long been dropped for “Christian” names (usually incomprehensible corruptions of French names), family surnames are not used. Thus, a man whose father was John Jules may call himself Norbert John, and his son Siméon Norbert. This practice—in a country where most families already consist of “mine,” “thine,” and “ours”—makes it exceedingly difficult to trace genealogies. “Outside children” always remain with the mother, whether or not she has more legitimate responsibilities. A newly married girl brings her husband back to her parents’ home until such time as he can build a house himself; she never goes to live with his parents.

Death and sickness are caused, not by disease, but by evil spirits in search of mischief. An old Carib friend once told me in confidence of the “death-spirits,” against whom neither priest nor doctor could avail. His last words before he died—of a pleurisy contracted through wearing wet clothes—were: Yo ka pren’ la vie au soè-à, “Tonight they are taking life.” Nine days after a death, a sort of second wake is held in the house of the deceased “pour faire la prière.” It begins with the singing of French hymns by the women, becomes—refreshments helping—quite a merry party, and
Island Carib types, Dominica, British West Indies.
Island Carib types, Dominica, British West Indies.
ends around daybreak with games, conundrums (to tirer contes in patois), and ring-dances. All the priests I asked denied that this ceremony had any Christian origin. A Carib finally told me that the object of this prayer-evening, frowned on by the church, is to rid the house of any death-spirits that may still be lurking there—a sort of spiritual fumigation!

In writing the following supplementary vocabulary I have used a different orthography from that adopted in the previous article, as more suited to the genius of the language.²

*People*

1’haru woman
n-iani my wife

*Animals, Birds, Fish*

wayàmakà iguana lizard
wanàçaé tête-chien snake
hēhwē² snake (generic)
kàturi screech-owl
kùrapiàò* small song-bird (patois: merle)
çèçè* small grass-bird
çicérù* Dominica or Imperial parrot
bàlau* small common fish
katàrù edible turtle (caret)
kùhirù* “Jack,” small fish
haçùlali fish (vive)

² In general, the vowels have been given their Latin values, while the consonants are as in English. Some modifications have, however, been necessary.

  au, eu, aì are pronounced in two syllables.
  ao is almost like English “how,” aì almost like “I.”
  è is the indefinite vowel in “mother.”
  ué, ui, ua replace wé, etc., where that sound results from an inflected u sound.

The tilde is used as in Portuguese to denote a nasalization of the vowel, in the same way as in French n final or before a consonant.

Accents denote stress, not quality.

ç is a sound intermediate between “ss” and “ch” (French).

hw is a strongly aspirated “wh” sound.

gh and kh are soft though distinct gutturals.

p, k, and t are softened so as to become confused with b, g, and d. There is similar confusion between long ò and u, ç and ch, l and r.

n and r seem to be sometimes aspirated.

Asterisks denote that the word is now used in Créole patois.

Where the English for a Carib word is unknown, its patois equivalent is given in parentheses.

² Used in patois in the expression hēhwē-congre, the conger-eel.
hànnao fish (bourse)
hépi "parrot-fish"
makùba river mud-fish (têtard)
móbëi fish, cardinal
čibuli fish (nègre) of dark color
titiri fry of certain fish caught in sheets at river-mouths
wàtèribi fish, variety of snapper (tanche)
içulu fresh water crayfish

Elements
parànna sea
konòbu rain
karèbali the breeze or trade-wind
iwàyuhurù storm

Various
écùbaraté knife or cultlass
hébichet* cassava-sifter
kàbuiya* noose
kàli* kind of fish-pot of cotton or bamboo
kanàri* big earthenware cook-pot
katàoli* shoulder container-basket
kuriàla* dug-out canoe
matùtu* Carib table (small basket-work tabouret)
muinà palm-thatch dwelling

Native Trees whose Carib names have remained in general use in Dominica.
bàlata, pàpài (papaya), kàrapà (crabwood tree), kàchibù, kwàchin,
ikàku (corrupted to z’ikak), luàrumà, mìbi, mòbel, çìma-ùba, wawa,
yàttaghu.

Carib Place Names in Reserve, forgotten by all but oldest inhabitants.
Ataori, Akàoyu (Raymond River), Kùçàrawà (Ravine Gros Rochers),
Kùçàrakwa, Kuànarà (Ravine Viville), Wàicàima, Kuérek, Ìçulàkitì
(Crayfish River), Bàrààiçì (Bataca), Wàiniàka, Kuària (Big River, now
a dried bed forming northern boundary), Wàraka (district between the
foregoing and River Pegoua).

Phrases and Verb-Forms
bìrhàli arhyàbu rìcha lightning streaks the night
kàimà w-atàbura tùnà let us go and draw water

* Heard once in patois, but very unusual.
tabù-búka tùnā  go and draw water!
ènni tābu nákū  I'm going to sleep (lit., close my eyes)
ènn ay-átakwa  here is for you to drink!
àla kuābutu  I want (or let's have) a drink!
ènn kāi amulāi  Here's how! (lit., here's let us appease)
aohwééli  he is dead
abínaka  to dance
chírakwa  to split, pierce

And some corrected forms of phrases given in the earlier article under Nos. 7, 8, 10, 12, and 14:

rùbài yètè nùnì—ni-lamāhàtina  Give here food—I am hungry
mékèru k'hiìnçi  The negro smells!
makrùbhùhåtina  I am thirsty
yùrek'-hào kàtu kàrrahí  Are you well?
rùbài pàipatè pùman iùtti kòmulåkha  Go and get me a smoke!

nùnì may be Breton's "no-i, pittance;" kàrrahí probably has to do with his "karréf, strength;" pàipatè is, I think, undoubtedly his "baibatí, go thou;" pùman his "bòman, from thee" (a form used in asking); iùtti, "a portion;" kòmulåkha, "to smoke."

The following song was taught me by Chief Jolly John, who learnt it from Tanaze, now dead.

They make war, O lazy one! They make war, O lazy one!

Unwilling thou to flee, thou my son

Come let us take the lead! Come let us awake!
As some of the words may be corrupt, I cannot vouch for the accuracy of the translation; for although words and air are known to several living Caribs, the meaning has been forgotten!

The remaining Indians of Dominica are today the only direct descendants of the two groups first encountered by Columbus—the Island Arawak and the Island Carib. They have, within the last seventy years, lost practically the whole of their language, tradition, and culture, and the greater part of their customs and individual way of living. This falling-off is not due to lack of stamina in them—the Dominicans having outlived by some two hundred years their cousins of the other islands—but is the outcome of a long, persistent, and stupid policy of interference and absorption that has remained unchanged since the days when the Indians were a real menace to white settlers of their islands. If something to preserve them is not undertaken soon—and it is doubtful whether that would still be possible—they in their turn will have disappeared in another generation. If that must be, it is high time to put on record in a more scientific way than has here been possible what yet may be ascertained of their physical characteristics, their customs, and their crafts; to etymologize the old place-names; and to make a representative collection of the ancient weapons and implements with which the soil of the island abounds.

Easton, Maryland
AN URN FROM THE RIO AGUARICO, EASTERN ECUADOR

By JOHN GILLIN

THE urn pictured herewith was collected by the writer in the course of an ethnological reconnaissance on the Rio Aguarico in eastern Ecuador in March, 1935, on behalf of the Peabody Museum of Harvard University. The specimen was presented to me by two Aguarico Indians who happened to share our camping place on a playa one night. These men informed me that they had found the urn during the preceding day while hunting for turtle eggs. It was protruding from the bank of a right tributary of the Aguarico, apparently having been exposed by erosion of the creek bank during recent high water. Inasmuch as none of the Indians living in the vicinity make or use such articles at the present time, I went next morning to the spot where the specimen was alleged to have been found. A hole was found in the side of a mud bank with evidences of considerable digging by sticks and cutlasses. The writer is convinced that the urn was found by the Indians in situ, four feet six inches deep in alluvial, gray clay. The surface of the ground was covered to a depth of about nine inches with vegetable mould and a matting of roots from the forest growing there. Two trees more than fifty feet high were found growing within a radius of fifteen feet inland of the spot where the urn was lying. We had no tools for digging, but a day spent in working with cutlasses revealed several sherds of gray, undecorated pottery and a hearth of charcoal at a depth of five feet nine inches from the surface. The creek in question, which is nameless, is a right tributary of the Aguarico which joins the latter an estimated forty-five miles above its mouth. The site is located on the left bank of the creek about one and a half miles above its mouth. The country is at present uninhabited by Indians for a distance of forty miles upstream, although about six miles downstream from its mouth Señor Abraham Duque's small plantation is situated, the most upstream outpost of white settlement.

Measurements: diameter at lip, 14½ inches; diameter at bulge, 19 inches; diameter at bottom, 6 inches; total height, 15½ inches; width of decorative band, 10 inches; circumference at lip, 46½ inches; circumference at bulge, 57½ inches; circumference at bottom, 19 inches.

The color of the exterior background surface is brownish red and the design has been applied with white paint. Over the whole a gum glaze seems to have been applied. The surface of the interior is grayish, with a few pinkish marks of firing barely discernible. Since there are no marks of smoke or indications that the urn was used over a fire after manufacture, the conclusion that it was a ceremonial container seems valid.

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The design is interesting, although the writer has been unable to identify it positively either in detail or in style with any previously published archaeological or ethnological material from the Amazon basin. Apparently two conventionalized faces are represented, the eyes of which can be distinguished by two oblongs placed side by side, a motive which occurs twice. Suggestive analogies are found in pictures of funeral urns from the Island of Marajó, published by Netto. The design may be said to have a "generally Amazonian feeling" without corresponding in many details to designs so far published from other sites in the area. The general treatment of broad lines contrasted with very light ones, of straight lines, sharp curves, and sharp angles is also suggested by an urn of anthropomorphc shape reported by Uhle from the Rio Napo, found at a point twenty leagues above the mouth of the Aguarico. Faint analogies may also be seen in some of the decorative art of certain present-day Indians of the upper Marañon and lower Ucayali, reported by Tessmann.

University of Utah
Salt Lake City, Utah

1 L. Netto, Investigacões sobre a arqueologia brasileira (Archivos do Museu Nacional do Rio de Janeiro, Vol. 6, pp. 261 ff.). See particularly Plate 19, Fig. 4, p. 309, and figures on pages 339, 345, and 468.

2 Max Uhle, Los principios de la civilización en la sierra peruana (Boletín de la Academia Nacional de Historia, Vol. 1, Quito, 1920); also reported in E. Nordenstiöhl, L'archéologie du bassin de l'Amazone (Paris, 1930).

3 G. Tessmann, Indianer Nord-ost Perus (Berlin, 1930): drinking cups of the Panabo, Plate 6, Nos. 1, 2, 3-11, especially Nos. 1 and 2; pottery vessel of the Kokama, Plate 5.
Urns from the Rio Aguarico, eastern Ecuador. Below, The developed design.
Walter Hough: An Appreciation

By Neil M. Judd


Walter Hough

Doctor Hough was born in Morgantown, West Virginia, April 23, 1859, the son of Lycurgus S. and Anna Fairchild Hough. He received his schooling at the Morgantown Academy and the Preparatory School of West Virginia Agricultural College; from West Virginia University he obtained his B.A. degree in 1883, his M.A. in 1884, and his Ph.D. in 1894. Following graduation, he taught for a year in a boys' school at Alton, Illinois, and
then began his professional career by accepting appointment to a minor position in the division of ethnology at the National Museum.

If we may judge correctly from a few autobiographical notes written in his seventy-fifth year, Walter Hough merely followed a natural inclination into the field that became his life's work. As a boy at his mother's knee, he was thrilled by the accounts she read to him of explorations in Palestine, the Near East, and elsewhere throughout the world.

I was fascinated [he writes] with the story of antiquity but it was sometime before I realized that near our home town there were traces of antiquity in the former camps of our Indians. Then I located and collected from the surface the various arrowpoints, worked stones, etc., which began my archeological collection.

Like every other of its kind, this boyhood collection was not restricted to a single theme. Young Walter was fertile minded; he was energetic and possessed the average boy's insatiable curiosity. The rugged valley of the Monongahela and the wooded hills on either side provided endless mysteries and hidden corners for exploration. His father's library, obviously an exceptional one for the time and place, partially satisfied the youth's desire for information and it also suggested additional subjects for out-of-school study, such as botany and the other branches of natural history. Thus the collection which had its origin in a pocketful of arrowheads came to include a sampling of the flora, fauna, and fossils from the Carboniferous of the Morgantown district.

The interest awakened by the latter discoveries appears to have been the dominant one throughout Hough's preparatory and college days. He devoted more attention to geology than to any other subject in the curriculum. In his spare time he collected fossils for Dr J. J. Stevenson, of New York, then a recognized authority on the geology of the coal measures of Pennsylvania and West Virginia, and received fossils from other areas in exchange. During this same impressionable period Hough came also under the stimulating influence both of William Maury Fontaine, long-time professor of geology at the University of Virginia, and I. C. White, state geologist and foremost authority of his time on the economic resources of West Virginia. Although Hough's enthusiasm for geology and paleontology was soon to be supplanted by a deeper interest in primitive peoples and the rise of civilization in various parts of the world, he treasured his Carboniferous plants and invertebrates until 1897 when they were presented to the National Museum.

As a copyist in the division of ethnology, then under the curatorship of Dr Otis T. Mason, Hough joined the Museum staff in January, 1886. A
year later he was appointed Aid in the same division and in October, 1894, Assistant Curator. These were merely the first of a succession of appointments and temporary assignments that were eventually to lead, however slowly, to the highest office within the department of anthropology. There can be but little doubt, however, that throughout this early period Hough's ingrained studiousness attracted the attention of, and was encouraged by, Professor Mason whose passion for accuracy and whose contagious enthusiasm for ethnological research were well known. At this time the division staff was wholly occupied with the cataloguing of extensive collections presented to the United States by foreign governments and other exhibitors at the Philadelphia Centennial Exposition of 1876—collections which, of necessity, had been placed in storage pending completion of the museum building in 1881. Among these new accessions Hough found themes for a number of short papers each year and the inspiration for endless reading. His amazing memory for what he read never ceased to be the envy and admiration of his colleagues.

Following the death of Professor Mason on November 5, 1908, Assistant Curator Hough was designated both acting Curator of the division of ethnology and acting Head Curator of the department of anthropology. On January 1, 1910, he was appointed Curator of Ethnology and ten years later, upon the resignation of Prof William H. Holmes, he was again placed temporarily in charge of the department. Responsibilities may multiply but reward comes slowly in Washington! Not until March 1, 1923, did Doctor Hough actually receive the title and emoluments of Head Curator of Anthropology, an office he continued to occupy with distinction, while remaining at least nominal Curator of Ethnology, until the end. Indeed, chiefly through his extraordinary fund of information, he had become so indispensable to the Museum that he was twice continued in office for two-year periods beyond the customary age for retirement of Federal employees before an Executive order of June 30, 1932 exempted him from compulsory retirement. He had been in the public service for fifty years, lacking only three months, when his career was brought to a sudden close September 20, 1935, after a fortnight's rest from the effects of a rather strenuous summer in Washington and on the very eve of his expected return to duty.

By far the greater portion of Doctor Hough's half century with the National Museum was spent in the laboratory. His was the chore, first of cataloguing specimens, then of preparing data for the copyists, next of supervising these and other routine tasks. He joined the staff at the height of the exposition era, when nations, states, and cities were vying with each other in commemorating historical events of greater or lesser moment. For
these expositions the Congress usually provided a building and authority for the commission in charge to draw heavily upon the national collections for exhibition material. Casual inspection of its annual reports indicates that the Museum participated in no fewer than sixteen expositions between 1892 and 1926, and in the preparation of exhibits for these Doctor Hough had an active part. He attended the Columbian Historical Exposition at Madrid in 1892 both as a representative of the Smithsonian Institution and as a member of the United States commission, helped install the Museum exhibits, and remained in charge of them. In recognition of his efforts on this occasion, the Queen Regent made Doctor Hough a knight of the Royal Order of Ysabel la Católica. Certificates, diplomas, and medals evidence his contribution to other expositions, national and international.

So far as the writer can learn, Doctor Hough’s first professional fieldwork was in New Mexico and Arizona, as assistant to the late Dr J. Walter Fewkes during the summer of 1896. Three years later he accompanied Dr J. N. Rose on an ethno-botanical expedition to Mexico. Subsequently, he returned to Arizona and New Mexico a number of times in connection with his own researches, both ethnological and archaeological, the nature and extent of which will be apparent from his bibliography.

Although his contributions to the literature of the Southwest well merit specific reference, Doctor Hough was perhaps best known among anthropologists for his studies of fire as an agent in human culture. Beginning with a modest article on the “Distribution of the Fire-Syringe,” published in 1888 in the first volume of the American Anthropologist, his bibliography includes no fewer than twenty-one items on fire, heating and lighting apparatus, etc.¹ To the 148 titles listed, there should rightfully be added several thousand letters mailed in answer to inquiries addressed to the Museum, for these often entailed considerable research and invariably went thoroughly into the subject raised even though this latter was a mere passing thought with the original correspondent.

But the wide range of his interests is only partially illustrated by his published works. He was an authority on old English, French, Italian, and American china, on old lace, on violins and pianos. His interest in violins and pianos came naturally from an inherent love of music (although without musical training, he often sat down at the piano, even in later years, to play by ear selections he had heard at afternoon concerts) while his interest in china was awakened by an early study of aboriginal American pottery.

¹ The number was erroneously given as eighteen in Science, Vol. 82, pp. 541–42, December 6, 1935.
the methods of its manufacture, and his efforts, in 1895–96, to reproduce pieces in kind. Few of his friends knew that Doctor Hough's two hobbies were wood carving and painting; that several very creditable Indian portraits, modeled by him in 1889, still hang in his home. Few knew that he possessed more than passing knowledge of the history of art and the distinguishing characteristics of the old masters. So broad was his reading and so retentive his memory, that he frequently astonished his subordinates by a remarkable ability immediately to name the culture and place of origin of almost any curio brought in for identification by chance visitors.

Doctor Hough was a founder of the American Anthropological Association (president in 1924) and a member of the American Association for the Advancement of Science (Member, 1889; Fellow, 1890; vice-president of Section H, 1904); the Anthropological Society of Washington (general secretary, 1906–07; president, 1908–09); the Washington Academy of Sciences; Archaeological Society of Washington; American Museums Association; Phi Beta Kappa (W. Va., 1914); the International Society of American Artists; the Washington Water Color Club; and the Sons of the American Revolution. He was a corresponding member of the Société d'Anthropologie de Paris and the Svenska Sällskapet för Antropologi och Geografi. He represented the Smithsonian Institution at four sessions of the International Congress of Americanists: at Huelva, in 1892; at Quebec, in 1906; at Rio de Janeiro, in 1922; and at New York City, in 1928.

Among his neighbors, his co-workers, and the thousands who came to his desk at the Museum, Doctor Hough exerted an unique influence. A more considerate, more helpful man never lived. He was the personification of kindness. Sympathetic at all times, of rare understanding and unfailing courtesy, he was a magnet that drew people to him with their little troubles and their immature ideas regarding aboriginal peoples the world over. The charm with which he received such visitors, the gentle, convincing manner in which he sought to present accepted scientific facts without open contradiction endeared him to all. He would exclaim with admiration over bits of arrowheads and pottery fragments gleaned from the hills about Washington by successive groups of school boys and proudly brought in for his inspection—fragments that invariably reminded him of those he had himself found in some far-away valley and thus provided opportunity for an informal lecture on the tribes of other areas. As one of his many admirers has written: "He was the symbol of everything that was fine and noble, kindly and good."

He married Myrtle Zuck, of Holbrook, Arizona, on December 29, 1897. Mrs Hough, two sons and a daughter, and seven grandchildren survive him.
BIBLIOGRAPHY

Many of the items here listed were noted in the annual reports of the National Museum at the time of publication; others were taken from Dr Hough's own incomplete record of his papers.

1886 Thumb Marks (Science, Vol. 8, pp. 166–67).
1887 A Bayanzi Execution (Science, Vol. 9, p. 615).
   Notes on the Bernadou Corean Collection (New Dominion, Aug. 7, 1887, Morgantown, West Virginia).
1889 An Interesting Collection from Thibet (The American, Vol. 17, p. 73, Philadelphia).
   The George Catlin Indian Gallery (The American, Vol. 17, p. 185, Philadelphia).
   Catálogo de los Objectos Expuestos por la Comisión de los Estados Unidos de América en la Exposición Historica-Americana de Madrid, 1892 (120 pp. Madrid).
   A Rare Form of Polished Stone Implements and Their Probable Use (Science, Vol. 21, p. 5).
   Balances of the Peruvians and Mexicans (Science, Vol. 21, p. 30).
   The Ancient Central and South American Pottery in the Columbian Historical Ex-


The Moki Snake Dance (Passenger Department, Santa Fé Route, Chicago).


1903 Shields from Western Sumatra (Smithsonian Miscellaneous Collections, Vol. 45, pp. 450–51).


1904 Kava Drinking as Practiced by the Papuans and Polynesians (Smithsonian Miscellaneous Collections, Vol. 47, pp. 85–92).


1907 Antiquities of the Upper Gila and Salt River Valleys in Arizona and New Mexico (Bulletin, Bureau of American Ethnology, 35).
The Palm and Agave as Culture Plants (Congrès International des Américanistes, XV Session, Quebec, 1906 [1907], Vol. 1, pp. 215–21).


Articles on Altar, awls, bags and pouches, black drink, blanket, bolas, boxes and chests, bullroarer, cements, clothing, clubs, collecting, cotton, dishes, dyes and pigments, eagle, fermentation, fire making, fishhooks, food, gourds, grass work, hair work, lance, mescal, moccasins. In Handbook of the American Indians North of Mexico (Bulletin, Bureau of American Ethnology, 30, Pt. 1).


1915 The Hopi Indians (Cedar Rapids, Iowa).


1924 Fire Origin Myths of the New World (Annales do XX Congresso Internacional de Americanistas, Rio de Janeiro, 1922 [1924], pp. 179–84).
The Story of Fire (New York).
Indian Village Sites and Quarries in Tennessee (Explorations and Field-Work of the Smithsonian Institution in 1927 [1928], pp. 119–20).
1930 The Bison as a Factor in Ancient American Culture History (Scientific Monthly, Vol. 30, pp. 315–19).
   How Did We Come by Art? (Scientific Monthly, Vol. 31, pp. 434–41).
   Tribal Communications (Commercial Standards Monthly, Vol. 8, p. 188, Washington).
   Seminoles of the Florida Swamps (Home Geographic Monthly, Vol. 2, pp. 7–12, Worcester, Mass.).
1934 Investigations on Ancient Canals in Southern Arizona (Explorations and Field-Work of the Smithsonian Institution in 1933 [1934], pp. 32–34).
Also thirty-one biographies for the Dictionary of American Biography; numerous reviews, notes, obituaries, and reports.

UNITED STATES NATIONAL MUSEUM
WASHINGTON, D. C.
REPORTS

ANTHROPOLOGICAL SOCIETY OF WASHINGTON

The Anthropological Society of Washington at its annual meeting held on January 21, 1936, elected the following officers for the ensuing year: President, Frank H. H. Roberts, Jr.; Vice-President, George S. Duncan; Secretary, Frank M. Setzler; Treasurer, T. Dale Stewart; Vice-President of the Washington Academy of Sciences, Frank H. H. Roberts, Jr.; Members of the Board of Managers, C. W. Bishop, D. I. Bushnell, Jr., H. W. Krieger, J. H. Steward, and W. D. Strong.

A report of the membership and activities of the Society since the annual meeting held on January 15, 1935, follows:

Membership:

Life members .................................................. 3
Active members ............................................. 37
Associate members ......................................... 12
Honorary members ........................................... 18
Corresponding members ................................. 18

Total ...................................................... 88

Deceased:

Active members ............................................... 2
Associate members .......................................... 1

Resigned or Transferred:

Active members ............................................... 4
Associate members ......................................... 2

Total ...................................................... 9

New Members:

Active Members ............................................... 3
Associate members .......................................... 2

Total ...................................................... 5

The Society lost one of its most devoted and active members through the death of Dr Walter Hough, Head Curator, Department of Anthropology, U. S. National Museum, on September 20, 1935. Another active member lost through death was Mr Charles L. Sturtevant, January 11, 1935. On August 31, 1935, the Society lost through death an associate member, Mrs H. L. Ickes.

Four active and two associate members resigned or were transferred from our Society: Dr Biren Bonnerjea, Mr R. A. Elmquist, Dr J. S. Wall, Mr W. M. Walker, Miss J. J. Glass, Mr Marcus Goldstein.

Members elected during the year were: Mrs H. L. Ickes, Mr Verne E. Chatelain, Mr C. I. Kephart, Mrs Elna N. Smith, and Miss Emma Reh.

The financial statement (Treasurer's report) is as follows:
Funds invested in Perpetual Building Ass'n. $1247.05
21 shares Washington Sanitary Improvement Co., par value $10 per share 210.00
2 shares Washington Sanitary Housing Co., par value $100 per share 200.00
Cash in bank 255.03
Total $1902.08

Bills outstanding:
- To American Anthropological Association $75.00
- To printer 3.75
- To Secretary 1.24
- To Treasurer 1.63
  Total $81.62
  Net balance $1820.46

Papers presented before the regular meetings of the Society were as follows:
- April 16, 1935, 659th regular meeting, Curses in Ancient Egypt, Babylonia, and Palestine, by Dr George S. Duncan, Professor of Egyptology and Assyriology at American University.
- November 19, 1935, 661st regular meeting, An Unknown African Negro Civilization, by Dr Hans Himmelheber, of Karlsruhe, Baden, Germany.

All regular meetings of the Society were held in Room 43 of the U. S. National Museum.

F. M. Setzler, Secretary

AMERICAN ETHNOLOGICAL SOCIETY
REPORT OF THE SECRETARY-TREASURER

The annual meeting of the American Ethnological Society was held January 27, 1936 at the American Museum of Natural History, New York City. Summary
statements of activities and finances were presented to the membership, officers were reelected, and new members were approved. The following reports were referred to the Executive Council for acceptance.

**Report of the Secretary**

*Membership:*

Increase in total membership and changes in the composition of the membership of the American Ethnological Society may be summarized in the records for 1934, 1935, and 1936:

1934—Life members 13, members 50, fellows 118, affiliates 6. ............ 187
1935—Life members 13, members 76, fellows 119, affiliates 5. ............ 213
1936—Life members 14, members 95, fellows 121, affiliates 3. ............. 233

The figures for 1936 are the present status of membership (February 1936).

The increase in total membership is the result of a continuing membership drive. Change in composition has resulted from the addition of many new members, and from many voluntary shifts of status from fellow to member. As a result, it has been possible to lower the dues rate for members to $9.00 for the current year.

The Society lost through death in 1935 Mr George C. Fraser, a member since 1920.

*Meetings:*

At the regular meetings of the American Ethnological Society, which were held on the fourth Monday of each month at the American Museum of Natural History in conjunction with meetings of the Section in Anthropology of the New York Academy of Sciences, the following programs were presented:

February 25, 1935. Suicidal reactions and culture. Dr A. A. Brill.
April 22, 1935. The principles of clanship in human society. Dr Paul Kirchoff.
May 27, 1935. Mountain chant of the Navaho. Mrs Laura A. Armer.

*Publications:*

Cree Texts, by Leonard Bloomfield, was distributed during the year as the 1934 volume (Volume 16 of the Publications). The 1935 volume, Caddoan Texts, by Gene Weltfish (Volume 17 of the Publications) is in press to be issued shortly.

Sales of the Publications, though not as extensive as the year before, continued high as a result of continuing circularization. Total sales to December 20, 1935 amounted to 214 volumes, of which 57 represented the distribution of Volume 16
to subscribers. This subscription distribution of 57 compares with 26 at the time of the issuance of Volume 15. Sales of sets and additional subscribers make the probable pre-publication sale of further issues approximately 70.

Respectfully submitted,
ALEXANDER LESSER, Secretary

REPORT OF THE NOMINATING COMMITTEE

On recommendation of the nominating committee, a motion was offered and approved by the members, reelecting the present officers and directors for 1936.

President: Dr Gladys Reichard, Columbia University
Vice-Presidents: Dr Elsie Clews Parsons, Harrison, New York; Dr Bruno Oetteking, Columbia University
Secretary-Treasurer: Dr Alexander Lesser, Columbia University
Editor: Dr Franz Boas, Columbia University
Directors: Dr Clark Wissler, American Museum of Natural History; Mr Clarence L. Hay, American Museum of Natural History; Dr Ruth Benedict, Columbia University

REPORT OF THE TREASURER

January 23, 1935 to January 22, 1936

CURRENT FUND

On deposit, Corn Exchange Bank, January 23,
1935 ......................................... $2477.75
Cash on hand, January 23, 1935 ................ 4.65

$2482.40

Receipts:

Dues, 1932–34, Fellows and members ...... $  59.90
  1935, Fellows and members ............... 1063.30
  Affiliates .................................... 3.00
  Life membership ............................. 100.00
  1936, Fellows and members ............... 482.00

1708.20

Interest, N.Y. Mortgage bonds .............. 45.62
  Bonds of Mortbon Corporation ............ 31.87
  U. S. Liberty bond at expiration ........ 10.62
  Accrued interest, Bowery account ....... 24.02

112.13

Cash from sale of U. S. Liberty bond ...... 500.00
Cash in Mortbon reorganization ............ 450.00

950.00
Gross sale of publications, Stechert...... 675.40
Overpayment, credited Stechert........... 141.45

816.85

Expenses:

Amer. Anthrop. Assn., 1935 subscriptions ...... $ 969.00
J. J. Augustin, printing Publications, Vol. 16...... 1055.32
Permanent funds, deposited in Bowery Savings
Bank........................................... 1050.00
G. E. Stechert, charges as agent................. 207.34
Commissions, 1934................................ 23.76
Commissions, 1935................................ 139.78
Shipping, Vol. 16................................ 41.50
Mailing charges.................................. 2.30
Meetings of American Ethnological Society....... 39.31
Printing, stationery, and circulars............... 90.63
Postage.......................................... 117.03
Secretarial assistance............................ 96.60
Brill, for Publications, Vol. 7, Part 1........... 2.72
Safety deposit box rent........................... 6.60
Bank charges, collections........................ .98
Returned checks and coupons....................... 31.25
Supplies......................................... 3.46
Refund, miscellaneous................................ 1.65
1934, uncollected check, drawn.................... 7.50

$3679.39

Balance, Corn Exchange Bk., January 22, 1936.... 1638.16
Cash on hand, January 22, 1936...................... 2.03
Current funds, Bowery Savings Bank............. 750.00

2390.19

$6069.58

Permanent Fund

Bonds of N.Y. Mortbon Corporation.................. $2550.00
Permanent funds, Bowery Savings Bank................ 1050.00

$3600.00

Remarks on the Treasurer's Report:

The balance sheet shows a favorable balance in Society funds. This is traceable to increase in membership, improvement in composition of membership, and sales
of the publications. Expenditures involved in circularizing and mailing in connection with drives for members and sale of publications amount to about $240.00. Results from this expenditure include a net gain of 43 members to date of this report, an approximate doubling of the number of members, a gross sale of $418.90 of publications (apart from distribution of Volume 16), and a substantial increase in the subscription list for future issues of the publications.

Important changes in the composition of our permanent funds have occurred as a result of the calling of our Liberty Loan bond, and a reorganization of the New York Mortgage Company, with its assets and liabilities taken over by the Mortbon Corporation. In the latter reorganization, the Society has received 2½ percent bonds to 85 percent of the face value of the bonds formerly held of the New York Mortgage Company, and 15 percent in a cash payment. These cash funds, along with proceeds of the sale of the Liberty Bond and the dues on one new life membership, have been deposited in the Bowery Savings Bank. While our permanent funds are now in a very liquid condition, our income from interests has been substantially reduced, since savings bank interest rates are low, and our bonds now draw 2½ percent as against the 5 percent rate of the bonds we formerly held.

Respectfully submitted,
ALEXANDER LESSER, Treasurer

Audited and approved by RUTH Bunzel, Auditor
BOOK REVIEWS

NORTH AND SOUTH AMERICA

An Introduction to Nebraska Archeology. William Duncan Strong. (Smithsonian Miscellaneous Collections, Vol. 93, No. 10, Washington, 1935.)

In Nebraska an archaeologist emerges from the trenches with a new outlook. The current views of historians and ethnologists are to his mind not in harmony with his archaeological observations. He seems sure that archaeology is right, especially the new archaeology which begins with the protohistoric, puts new substance into the framework of history and above all integrates the sequences of archaeology with those of history. He views the historical and ethnological concepts of aboriginal Nebraska as "lop-sided" and misleading. He further assumes that all current regional concepts are probably at fault and calls upon archaeologists everywhere to come up out of the trenches to set the ethnological house in order. That his views are fundamental is indicated by his statement, "it seems evident that any anthropological or sociological approach which ignores or underestimates the importance of time perspective is open to the same criticism" (p. 300). Further he avows the historic accident theory of origins and rules out the environment, but in the body of the text accepts the bison and the horse as prime conditioning factors.

The foregoing remarks are in order, since something more than a fifth of the book under review is devoted to the destructive criticism of Plains ethnology. However, the bulk of the volume comprises a technical detailed report on archaeological field-work, which we pass over with a few brief comments. Thus in 1929 the author projected a plan for work in Nebraska. Then followed a great deal of surface exploration; some five ossuaries, an equal number of villages, and many minor sites were excavated. The details are set down in the book and interpreted in terms of time sequence. The field-technique and organization of the text are commendable and the work as a whole stands more than an even chance to become a classic in the archaeology of the Missouri country. We may add that though time sequence was the all important objective, but two cases of stratification were encountered, Signal Butte near the western boundary of the state and the Gilmore Walker site near the Missouri River on the east. For other sites, indirect dating methods were used; horse bones and trade objects taken as defining the historic and arranging such sites in series according to the quantity of horse bones found therein. However, the limitations imposed by the rarity of true stratigraphy are acknowledged by the author before he offers his interpretations.

We turn now to general statements. For eastern Nebraska the author proposes a time sequence which he coördinates with those for Iowa, Wisconsin, etc.; thus, (a) the present historic level, or horse culture, (b) an immediate prehistoric horizon, named Nebraska culture, and finally, (c) an earlier horizon called Algonkin. The Nebraska is assumed to correlate with the prehistoric Siouan which archaeologists think underlies the historic Siouan east of the Missouri and in turn covers up an earlier Algonkin horizon. However, it was eastern Nebraska, only, which integrated
in this way. In the central part of the state were the Pawnee, whose prehistoric
time sequence, though obviously roughly contemporary with Siouan, shows a
different development.

The western part of Nebraska lies in the dry plains where the historic period
opens with less sedentary tribes, for example, the Comanche. The most distinctly
stratified site, Signal Butte, lies within these plains and gave three separate levels,
all prehistoric; the upper with pottery, the others without. The lowest level was
thought to have a good claim to antiquity. None of these levels seemed to correlate
very well with either the Pawnee or the Nebraska.

In general, then, Nebraska from east to west falls into three zones of sequence
having little in common save the historic level, or horse culture. The author recog-
nizes this lack of prehistoric homogeneity and the tentative nature of his correla-
tions. Whence then the new outlook?

In 1914 Holmes proposed two prehistoric areas for central United States, a
Plains area and an Upper Mississippi area. The provisional line separating them
was conceived as passing through Nebraska from north to south. The author seems
to say that such a boundary is incompatible with his new data—we fail to follow.
No one expects such a map-line to be absolute, though we agree that the author
gives this boundary its first clear definition, no mean contribution. Then he finds
pottery at Signal Butte; H. I. Smith has unpublished information as to places
where a few crude sherds have been found in the dry plains, especially in Canada.
So the author may be justified in assuming a thin pottery layer over the dry plains.

However, the Pawnee data seem to have contributed the keynote to the new
outlook. It was observed that as horsebones increased in the proto-historic sites,
pottery deteriorated. This, with historic data for the decline of Pawnee power, sug-
gested that the immediate prehistoric Pawnee represented the Grand Period in
their career. Their turn to Plains culture was then a degeneration. The next step
is to assume that the historic Siouan, also, degenerated from a similar golden age.
So follows the generalization that Plains culture is merely a degeneration of a Grand
Period. Thus, what ethnology has placed so much emphasis upon, the author re-
gards as “but a thin strikingly uniform veneer” over the prehistoric Pawnee-Siouan
culture.

Though not quite clear, the author’s attack upon the ethnologists implies that
his uniform veneer wherever found in the Plains is merely a dressing for a pre-
historic culture of the Pawnee-Siouan type. There are obvious difficulties in
harmonizing such a view with geographical and historical data for the dry plains
and the northern grasslands. Unpublished data for the Assiniboine, Flathead, Cree,
and Kutenai seem not to fit into such a scheme. However, since the author’s inter-
pretations are based upon Nebraska only, we prefer to wait until the archaeologists
in a few other states present their data.

Yet, though not ready to accept the view that the Plains culture of ethnology
is a broken down prehistoric culture, we believe that the author does present a new
approach through the methods of archaeology, a procedure by which the historic,
protohistoric, and prehistoric can be distinguished. Apparently ethnologists ir-
ritate him by ignoring time relations. It is true that the documentary data for Plains tribes is dated and as a whole falls into such periods as exploration, fur-trade, and reservation days. He shows how archaeology can enrich this historical framework and integrate it with proto-historic and prehistoric sequences. There are those who believe ethnology bankrupt and seeking refuge in psychiatry, but now we see archaeologists as ready to occupy the abandoned field with a rejuvenated historical method. They demand that ethnologists become thoroughgoing historians or move out.

American Museum of Natural History


Clan and Moiety in Native America. Roland L. Olson. (University of California Publications in American Archaeology and Ethnology, Vol. 33, No. 4, 1933.)

Here are two volumes of considerable interest to the student of social organization. While Dr Olson’s paper has already been critically examined by Lowie in a recent number of the American Anthropologist,¹ it still seems profitable to compare his general assumptions and results with those of Professor Nieuwenhuis. Both authors are primarily concerned with the history of dual organizations in the New World and both make use of much the same data. Here, however the similarity ends, for so different are the presuppositions and methods of the two authors that quite different results are achieved.

Dr Olson as a matter of fact does not deal especially with the actual history of “unilateral institutions” in the New World. By defining these institutions very broadly he attempts to show that they are preponderent in all the major areas except the peripheral ones. Further they have too many arbitrary associations to be independently developed, and their wide distribution suggests that they most likely belong to the “archaic” culture of the early immigrants. Presumably, then, they would be spread by migration and diffusion southward, the peripheral groups losing them under the stress of environmental conditions in marginal regions.

Professor Nieuwenhuis, on the other hand, limits his study to moiety organizations and attempts to trace their actual history in the New World. From a series of earlier studies of the “Dual-Culture” in Oceania he has come to the conclusion that the basis for this culture complex is to be found in a principle of the sexual division of society and nature, e.g., the sex-totemism and beliefs of southeastern Australia, Extending his investigations to the New World he finds evidence of sex-totemism in Preuss’ account of the Kágrabá, an Arawak tribe of northern Colombia. Following Pater Schmidt he hypothecates for the New World an Urkultur (with high gods, simple social organization, etc., represented by the Algonkin, Inland Salish, North-
ern Plains tribes, etc.) which is overlaid by a *Dualistische Kultur* with exogamous moieties, a dual differentiation of nature, complex mythology, sex-gender in language, etc. This "Dual-Culture" which developed among the Kágaba and other Arawak tribes was introduced by way of the Antilles into the lower Mississippi region, from whence it spread fanwise to California, to the Central Siouans, Iroquois, and other tribes, the culture diminishing in intensity the further it spread.

There is much that is suggestive and valuable in these two papers and other workers will be grateful for the mass of material which is here compiled. But in neither case does the "history" seem to develop out of the evidence presented; rather both authors develop their "history" on the basis of certain assumptions which are open to serious question, and proceed to marshall the evidence to support their position. The degree to which there is a selection and interpretation of data is clearly brought out by a comparison of the two studies. For example, Olson makes much of the data for the Northwest Coast, Mexico, Central America, and the Andean region, while Nieuwenhuis ignores the Northwest Coast and considers the other regions as marginal to his "Dual-Culture" and not involved in its transmission. The Southwest is shown to be not a transmitting point on the basis of Zuñi material, whereas other Pueblo groups would give a different picture.

With these recent studies we now have several quite contradictory "histories" of social organization in the New World. It seems to the reviewer that these studies are starting at the wrong end. It would seem important to first find out what dual organizations are and how the various forms of unilateral institutions are related to each other, and secondly, to attempt to unravel the historical situation in the individual areas. Olson compares areas rather than individual tribes, and takes the historical relations of tribes within an area for granted. But if these relations give a series of social organizations as varied as those found within the Southwest, the Northwest Coast, California, or the Plains, how can more diffuse contacts between areas, coupled with diverse environments, yield greater similarities?

Fred Eggan

*University of Chicago*


Though the Montagnais-Naskapi are found to be an exceptionally crude and simple people, they possess an essentially religious nature. Obtaining subsistence solely by the chase, they have worked out a spiritualistic system as complete and as artificial for gaining control over animal spirits as their hunting devices and weapons are effective in accomplishing the physical slaughter of game.

It is this spiritualistic system of the Naskapi-Montagnais rather than their secular hunting culture that Speck describes in the present work. An orderly summary of the content of this system is not easily made. Naskapi theologians have not reduced their religious ideas and practices to a coherent unity expressible in a simple formula. The following appear as the outstanding features of the system.
Manitu, described as representing something close to our notion of unseen force, is the dominant spiritual element. In the practices based on the manitu-concept the soul of the individual is the focal center. The active soul or Mistapeo provides the means of overcoming the spirits of animals by revealing in dreams when and where to hunt for game, how to proceed and how to satisfy it when slain. The process of self-study, of dream cultivation, and submission to dream control becomes the dominant idea of the inner life. Dreams are induced by personal, as distinct from professional, conjuring practices, such as singing, drumming and rattling, the sweat bath, seclusion and meditation. The dreams must be interpreted with the aid of divination, which consists principally in the use of bones of animals.

Finally the dreams obtained and interpreted remain to be paid for; satisfaction to be rendered to the dream source and to the animals whose lives fall victims to its power. The means of compensation are to be found in the respect paid to the animal remains, in the observance of the animal post-mortem rites . . . and in the execution of symbolic and decorative art designs.

Their mythology reveals the culture hero
as the personification of those aspirations held highest in the mind of the Naskapi, as the master of that conjuring craft exerted over man and animal which the hunter strives so hard to develop within himself.

The religious concepts and practices of the Naskapi-Montagnais with whom Speck has had such intimate contact during these last three decades are in the main identical with or similar to those of the western Naskapi-Montagnais of the east and southeast coast of James Bay, but on the other hand differ therefrom in several important respects. Among the latter, manitu is not so much unseen force as supernatural personality. Mistapeo is the head spirit of the shaking tent rite, not a super-self. Most of the hunting observances appear much more closely tied up to aboriginal theism, and the Supreme Being concept itself, with its related hierarchization of subordinate spirits, is much closer to that of the York Cree as described by David Thompson than to that of Speck’s Naskapi.

All in all, the close-up view given us in “Naskapi” of the inner spiritual life of the Labrador hunters is a distinguished contribution, by a sympathetic and uniquely qualified observer, to our knowledge of the aboriginal American religious consciousness.

REGINA FLANNERY

Catholic University of America


This is an excellent book which no anthropologist or ethnologist interested in the Southwest or Plains can afford to overlook. It is not only full of suggestive points of inquiry but also offers solutions to many of the major problems of cultural integration in these two areas. Dr. Thomas is one of the very few American historians who makes real use of ethnological data and understands the significance of “cul-
ture" in its anthropological context. His works are doubly valuable because of this dual perspective.

The first third of the book contains an account of the Spanish, French, and Indian relations from the reconquest of the Southwest up to 1727. The remainder consists of the translated documents from which the information is derived. It shows conclusively that until the beginning of the eighteenth century there was no abrupt line of demarcation between Pueblo and Plains culture and that the eastern Apache-speaking people were appreciably like the Pueblo in culture. This very real cultural transition disintegrated only after the intrusions of the Comanche and Ute. Abundant data are also given on the movements and inter-tribal relations of Pueblo, Apache, Comanche, Ute, Pawnee, Kansa, and on the extent to which these tribes were influenced by the French and Spanish. These with many cultural observations offer valuable clues to the ethology of these peoples and a basis for a reexamination of and further archaeological research in New Mexico, Colorado, Kansas, and Nebraska.

At two points Dr Thomas' conclusions may be questioned. He conjecturally places the site of the Villasur massacre near the junction of the North and South Platte Rivers. There is no present archaeological evidence confirming the location of a Pawnee village in this vicinity. Furthermore, both archaeological and other historical data for this period place the nearest Pawnee settlement approximately one hundred and twenty miles east of this point.1 Should this engagement have occurred instead at the junction of the Loup and North Platte, as has been suggested by Sheldon,2 it would also alter or somewhat enlarge Dr Thomas' location of the Apache rancheria El Cuartelejo. In this connection it is possible that El Cuartelejo, like Quivira, was a term applied to a much larger area than is indicated by the present account. However, these are minor points which in no way diminish the high quality or uniform excellence of the contribution.

BERKELEY, CALIFORNIA

The Prophet Dance of the Northwest and Its Derivatives: The Source of the Ghost Dance. LESLIE SPIER. (General Series in Anthropology, No. 1. 74 pp., map. $1.00. Menasha, Wis.: George Banta Publishing Co., 1935.)

Spier's main thesis is that the ultimate origin of the two Ghost Dance movements of 1870 and 1890 was not with the Paviotso, as has been generally assumed, but in the Northwest among the tribes of the interior Plateau area. For this much earlier source cult, which appears in the ethnographic accounts under various names such as dream dance, ghost dance, religious dance, praying dance, and so forth, he coins the name Prophet Dance. His marginal thesis is that this northwestern cult


2 Nebraska History and Record of Pioneer Days, Vol. 4, No. 1, 1923.
was also the source of the Smohalla cult of eastern Washington-Oregon and its modern form, the Pompom or Feather religion, and perhaps of the pseudo-Christian sect of Shakers now flourishing in adjacent coastal territory.

The Prophet Dance complex was known to all the tribes of the northwestern interior from the Babine and Sekani in the north to the Paviotsot of western Nevada in the south. It is known to have flourished in its northern form at least as early as the opening of the nineteenth century. Its basic components were an integral part of the Northwest Coast culture as well as of that of the Plateau.

This northwestern cult agrees precisely, Spier maintains, with the core of the Ghost Dance religion among the Paviotsot, and the circumstances of its appearance among the latter parallel those of earlier date in the Northwest.

Each phase of this embracing synthesis and genetic reconstruction is generously documented. The evidence is drawn from widely scattered manuscript and published sources. Spier’s splendid command of his source material has served him well in assembling these data. Incidentally, the study is a signal illustration of the aid which we in our ethnological reconstructions can often find—but which we so often neglect to seek—from dated and strictly historical documents.

The evidence is marshaled with order and clearness and discussed with caution and balance. The study as such is a model of restrained and sustained ethnological reasoning and interpretation. To the present reviewer Spier appears to have established both his main and marginal theses on a basis that will not easily be shaken. As a result, the chaos of aboriginal Indian messianic and kindred cults has become a cosmos, and a new epoch begins in the historical interpretation of American messianism. The General Series in Anthropology is off to an auspicious start.

A final word on format. The monograph appears in double-column pages, photolithographed from typescript. Spier, to cut costs in these days when funds for publication fall so far short of waiting copy, has followed the counsels of his own good sense and taken his courage in his hands. And happily, too, he has not seemed to worry much whether the typescript right-hand margins were flush or not. Who cares anyhow?

Catholic University of America

The Significance of the Dated Prehistory of Chetro Ketl, Chaco Cañon, New Mexico. Florence M. Hawley. (x, 80 pp., 18 pls. Albuquerque: University of New Mexico Press, 1934.)

In 945 A.D., a masonry pueblo (now designated Chetro Ketl) was growing near a small surface stream in an evergreen forest. Its inhabitants used “Type I Masonry” and preferred a semi-polished black-on-white pottery.

Between 1030 and 1090 A.D. the pueblo showed its greatest development. “Type II Masonry” (1030–1070) was better; “Type III Masonry” (1062–1090), best. Extensions and rebuilding resulted in the ground plan of today. Polished black-on-white pottery became most abundant. This most flourishing period began with a six-year drought, and growth showed no dependence on rainfall.
For no apparent reason more plausible than aridity caused by deforestation, the pueblo declined and was deserted about 1116 A.D. Masonry types IV, V, and VI display increasing degeneration during this period; but polished black-on-white pottery continued prevalent.

Such is Miss Hawley's reconstruction of a bit of the prehistoric Southwest.

Primarily, it shows how the Douglass tree-ring method may be applied to an archaeological problem. Secondarily, Miss Hawley has to her particular credit the development of a chronological master chart from refuse charcoal, and the demonstration that upper strata are not thereby more recent than lower strata.

The author's acquaintance with her background is suggested by a fluent and pleasing use of current assumptions. Her style is clear, concise and vivid with almost none of the prehistorian's difficulties-to-be-overcome tremolo. There is no undue stressing of handicaps. Logic over-rules desirability in her conclusions. Fact after fact suggests long, tedious, arduous, personal work; but Miss Hawley owes herself, as well as the reader, a more definite statement as to the exact work she did in preparation for her conclusions.

After explaining the Douglass tree-ring dating technique, Miss Hawley dates the wooden construction material in Chetro Ketl. Next, the six masonry types of the pueblo are described and dated through their relation to the wooden material. Superposition of masonry types furnishes a check on the tree-ring dates. The masonry types are now available as relative age criteria (e.g., like geological strata); an understanding of the re-use of material has been developed; and a framework of dates has been constructed. Building, alteration and reconstruction within the pueblo can now be recognized. Periods of activity, growth, and decay are established.

However, Miss Hawley's originality is most obvious when she turns to one of the refuse mounds. Soil characteristics enabled her to recognize four strata. First, sherds from these strata were classified into eight types, significance tests were applied, and strata compared on the basis of sherd content. Stratum 1 was essentially the same as 3; 2 as 4. Then, from the tree rings on the charcoal fragments in the strata, she constructed a chronological chart.

Now, many an archaeologist's digging has left him with but the dust of yester-years on his hands and mind, yet sustained by the knowledge that virtue is its own reward and stratification is always stratification. How demoralizing then to learn that Miss Hawley's chart showed Strata 4 (the topmost) and 2 older than Strata 3 and 1 (the lowest). The orthodox unrolling of culture from the bottom up is impugned. In our dump, the can-opener may be older than the coup de poing. Here is a very real contribution to archaeology.

Miss Hawley's explanation is that Strata 1 and 3 consist of current waste while 2 and 4 are composed of material torn down or shifted because of building operations.

Pottery types are thus connected with masonry types. Soil composition and statistical studies are tied to tree rings. The results are three age criteria, a remarkable series of dates, and a capable demonstration of how to use certain new tools in archaeological research.
The editor of this series cannot be congratulated. From the “Table of Contents” where “protocols” appears under “Plates” (as they do not in the paper) to the last plate (where no figure numbers are given), errors are numerous. Not to list them all, but to give some examples: p. 19, “Fig. 2, Plate 7,” which is Fig. 2?; p. 34, “Fig. 2, Plate 10,” none; p. 47, “Protocol 2,” 3?; p. 54, “underlined figures,” none; p. 69, p. 74, “Plate 10,” 1? Far more deplorable are the plates. Intelligently conceived and ingeniously devised, most of them are so incompetently reproduced as to be practically worthless; e.g., I, VI, IX, XIII. What wonder that Miss Hawley finally illustrates different things with the same figure—“inferior” and “superior” masonry (p. 25, “Fig. 5, Plate 12”)?

A foreign scholar might wish to ask Miss Hawley some questions on secondary matters. For example: Can the years 900–950 A.D. (from Plate I) be referred to as the “9th century”? (p. 11). Or is 1116 part of the “11th century”? (p. 22). Is not the age of Mesa Verde (075–1252) puzzling? (Plate I). Are not the peripheries of today the possible centers of tomorrow in a refuse dump? (p. 52). Do westerly winds drift sand on the west slope of a dump? (p. 56). What is the “natural death” of a culture? (p. 74).

Twentynine Palms, California


Mrs Nomland presents a study of archaeological collections from three prehistoric sites, Coro, El Mamón, and La Maravilla, located in the coastal section of the State of Falcón between the towns of Urumaco and Coro. The work consists of the description and analysis of an unstated number of unstratified specimens, the great majority of which are fragmentary potsherds. It is not clear from the report whether the author has ever been in Venezuela but the lucid descriptions of the sites indicate a personal experience. The material from La Maravilla and the principal part of that from El Mamón are definitely stated to have been collected by others. The specimens for the most part are described collectively for each site, first according to types of ware, then other characteristics. To the verbal description seventeen pages of excellent drawings add invaluable clarity.

In analysis, Mrs Nomland couples with her descriptions useful comparative data on specimens from other parts of the Caribbean area. A special chapter on “Clays and Temper, Firing and Modeling” treats special aspects of the subject with notable thoroughness. In general, the author’s conclusions indicate interest in both the chronological and geographical relationships of her material. With regard to the former, the methodology implies concern with the development of simple wares into complex as an index of relative time in an evolutionary relationship. This is also borne out by the statement “Differences in pottery styles imply differences in chronology,” which appears invalid from other points of view. The reviewer does not mean to imply that the author is not aware of other points of view but
may be less secure in working with them (vide p. 23, "... vessels executed in the most primitive style may belong to a later period than newer appearing varieties"). At least emphasis on a consideration of the possibilities of diffusion from the point of view of time is lacking.

With reference to space relationships, a table gives differences and similarities between the three sites described in this study and that of Hato Viejo previously studied. The table lists thirty-eight pottery types or characteristics (one appears to be repeated) but loses most of its value because the characteristics are not weighted whether considered objectively (flat griddle-plate is equivalent to plain red ware) or as statistical units (only presence or absence is indicated with one exception). That "Coro stands out as being quite dissimilar to the other sites" is the principal factual conclusion. That "it appears probable that Coro was chronologically earliest, and La Maravilla ... the latest, while El Mamón and Hato Viejo ... probably lie somewhere in between," is given as conjecture.

CORNELIUS OSGOOD

PEABODY MUSEUM, YALE UNIVERSITY

OCEANIA AND ASIA


These two books are both valuable additions to our knowledge of Melanesia. They are interesting both in themselves and as examples of different techniques in the study of uncivilized peoples. Miss Blackwood's book is the most complete account of the life of a Melanesian tribe which the writer has so far encountered. It is a monumental work, running to six hundred pages of small print without counting the numerous and excellent illustrations. It is unusually well organized, with a minimum of repetitive material, and this bulk merely reflects the richness of its content. Any investigator can find here information on any phase of native life, while the section on dreams represents a new departure in ethnological research and one of great interest to psychoanalysts. There is abundant evidence throughout that the author was a keen observer and phenomenally hard worker and no problems which can be solved by this combination remain to be solved.

At the same time, one feels that the author's very breadth of interest is reflected in a certain loss of depth. Although there are a number of good and valid interpretations of particular phases of native life, the reader is left uncertain as to how the natives actually feel about most things. It is plain that the author allied herself with the men's side of the society. This threw open for observation large areas of the culture which would otherwise have been closed to her, but it also prevented her

from becoming a full participant in the life of the community. The men apparently welcomed her at their ceremonies and gave her full facilities for observing and recording everything that happened, but the reviewer doubts whether they ever forgot that she was there. The women seem to have extended similar facilities but there is plenty of evidence that they never fully accepted her as one of themselves. One feels that Miss Blackwood knew her tribe well but not intimately. It is quite possible that the attitudes of the tribe precluded such intimacy with a European in any case, but its lack is to be regretted.

In sharp contrast with the preceding work, Dr Fortune's book reveals a close intimacy with his subjects. He seems to have known every one in the village well and to be able to describe their personalities and emotional reactions as accurately as the average individual could describe those of his European neighbors. All this knowledge is applied to the description and interpretation of a single phase of the culture. This is presented to us in all its aspects with a wealth of illustrative case material, native opinions, etc. After reading the book the average student will feel that he knows a great deal about Manus religion but only a little about Manus culture as a whole. Dr Fortune is not to be criticised for this, since he has quite deliberately limited his field and has done an excellent piece of work within the bounds which he has set for himself. At the same time it is to be hoped that Dr Fortune will sometime apply the same technique to the description of some culture as a whole. For example, it would be extremely interesting to know how the Manus people feel about their constant economic exchanges and just how necessary the religious sanctions which he describes are to the continued working of the system.

At the present time there is a notable lack of ethnological studies which have both breadth and depth, insight and objectivity. A combination of Miss Blackwood's and Dr Fortune's techniques might result in such a study. While the bulk of such a work would be formidable, it certainly seems worth attempting.

RALPH LINTON

University of Wisconsin


Although intended for a popular audience this book by a professional anthropologist should not be ignored. The vignettes of field-life in the Solomon Islands, in the coastal islands and interior of southeast New Guinea, and in Bali, are skillfully translated to interpret the charm, vitality, and intimate sympathy of Bernatzik's viewpoint. Of prime value are the many photographs which magically capture every mood of native life from the quiet rapport of a father and son to the pitched intensity of a poised spear-thrust.

Certain items provoke speculative interest. On Owa Raha (Solomons) the author obtained an extensive series of pencil drawings made by children, adults, and a priest, who presumably had never used such a medium. The subjects were spontaneously chosen: in the enormous range, which clearly exposed each drawer's interest, but two were definitely erotic, these from two frustrated boys. The priest reproduced
in extenso the form and activities of innumerable spirits whose world existed only in the realistic imagery of his own mind (pp. 21–23, 36–39).

Beyond the upper Purari River (eastern New Guinea) Bernatzik encountered Papuans whose basic culture closely resembled that of the upland pigmy tribes in the high northwest interior of the island (p. 97 passim).

Bali’s tainted culture is briefly and regretfully touched upon.

NEW HAVEN, CONNECTICUT

A. H. GAYTON


This work is the author’s fifth monograph on the aboriginal tribes of the hill region of central India, following studies on the Munda, Birhor, and Oraon. The Bhuiya (variants: Bhumia, Bhuihar, Bui, etc.) are a Munda or Kolarian people numbering approximately one and one-half millions, including the kindred Musahar of Bihar. Although replete with comparative notes on the Hinduized Plains Bhuiya, the book concerns itself principally with the more primitive culture of the Hill Bhuiya, who live between 21° and 22° N. lat. and 85° and 86° E. long. in the forested hill ranges of the Orissa Feudatory States of Keonjhar, Bonai, and Pal Lahera.

Bhuiya culture is consistent with that of adjacent tribes, as is revealed by a brief tabulation of some of its outstanding elements: a food quest based on the cultivation of dry rice and vegetables with subsidiary hunting, gathering, and fishing activities; brand-tillage with decennial relocation of villages; dormitories for bachelors and commonly others for unmarried girls; a patrilineal, patrilocai, and patristotal family organization; village exogamy with subtribal endogamy; predominant monogamy with cross-cousin marriage and the levirate; traces of tree-marriage and ceremonial capture; dual chiefship (secular and sacerdotal) in the village; earth-burial with subsidiary cremation; worship centering about a supreme sun god, an earth goddess, ancestral household spirits, and tutelary divinities of villages and subtribes; and an annual cycle of ceremonies correlated in the main with the phases of agriculture. Contrasts with neighboring cultures are noted, e.g., the absence of sibs and totemism.

All aspects of culture receive consideration. One might wish, however, for a somewhat more adequate treatment of the material culture and a clearer analysis of the theory of disease, which is said to be caused by offended spirits, sorcery, and the evil eye and cured by sacrifices, magical techniques, and herbal remedies. If the work falls short, in many respects, of the highest standards of modern ethnographical reporting, it is nevertheless so incomparably superior to most of the existing accounts of aboriginal cultures in India that criticism seems like cavil. One welcomes in particular the detailed anthropometric measurements of one hundred adults given in Appendix I and the full recording of the kinship system with the behavior patterns associated therewith. The book has a map and an index, but lacks a bibliography and is marred by numerous typographical errors.

GEORGE PETER MURDOCK

YALE UNIVERSITY

Social Organisation of the Aimol Kukis. J. K. Bose. (Ibid. 24 pp.)

Dual Organisation in Assam. J. K. Bose. (Ibid. 29 pp.)


An Ethnic Analysis of the Culture-traits in the Marriage Customs as found among the Ràdhiya Brahmins of Mymensingh. Nirmal Chakravarti. (Ibid. 80 pp.)

The Khasis. Tarakchandra Raychaudhuri. (Ibid. 24 pp.)

The above list of titles will serve to remind American anthropologists that the "Journal of the Department of Letters" of the University of Calcutta carries a considerable number of articles of sufficient quality and interest to merit their attention. The first of those listed supplements the general study of Clements. The last is strictly anthropometrical in content. The second is descriptive, the fifth comparative, the third a survey. The fourth is a significant contribution to Santal social organization and includes a full list of kinship terms.

George Peter Murdock

Yale University

PHYSICAL ANTHROPOLOGY


The amount of anthropological research now going forward in India is beginning to be commensurate with the vast anthropological importance of that great country. As is fitting, Indian anthropologists are taking the lead in these investigations. Dr B. S. Guha, Anthropologist of the Zoological Survey of India, has produced a work which must be reckoned as a more important contribution to the physical anthropology of the country than any heretofore published. The material includes measurements and observations of 51 "racial groups" from all parts of India, totalling
3,774 individuals. Virtually all of these subjects were measured by Dr Guha himself, using standard instruments and techniques.

The method of analysis employed is principally the comparison of groups by the use of Pearson's Reduced Coefficient of Racial Likeness. The reviewer must confess that he himself regards the said coefficient with no little suspicion, arising, no doubt, from his inability to comprehend the merits of its beautiful elaboration. Nevertheless he agrees that some expression of the degree of likeness or difference between two series, based upon a large number of characters, is highly desirable for racial analysis, and he supposes that no one is more fully competent to devise such a measure than Professor Pearson. Was it Samuel Johnson who said, "Doubtless God could have made a better berry than the strawberry, but doubtless he never did"? We shall then disregard our personal dislike for strawberries and coefficients of racial likeness and consider Dr Guha's results.

On the basis of the Coefficient of Racial Likeness and of morphological observations, Dr Guha discerns the following racial elements among the peoples of India:

A. A short-statured, long-headed element with high cranial vault, faintly marked supra-orbital ridges, broad, short, orthognathous face, medium lips, mesorrhine index, skin color from light brown to dark brown, hair color black, hair form straight to wavy, and moderate in amount on face and body. This element forms the predominant type in the greater part of the lower stratum of the population of Northern India.

B. In the western littoral and Bengal, a brachycephalic element of medium stature, with flattened occiput, high head, and receding forehead. The face is short and orthognathous, but broader than in the first type. The nose is long and "highly pitched," but often arched and convex. Skin color varies from pale white to tawny brown. Eye color is usually dark; hair is usually black and straight. A few light eyes and rare cases of brown hair occur in this type.

C. In Northern India a tall, long-headed, lower-headed strain with long face and prominent, narrow nose. In its purest form the skin color is rosy white, and eyes gray-blue. The hair is straight and "chestnut" in color. In mixed varieties the pigmentation is darker.

In the aboriginal population:

D. A short dolichocephalic strain with marked brow ridges, broad, short face, prominent lips, small flat nose with spread alae. The hair varies from wavy to curly and the skin color approaches black. This type is closely allied to the Veddas of Ceylon, the Toalas of Celebes, the Sakais of the Malay Peninsula, and represents a less primitive form than is found among the Australians.

E. A dark pigmy strain with spirally curved hair, remnants of which are still found among the Kadars and the Pulyans of the Peramubicullan Hills, but which is mostly submerged in India at the present time.

F. A mongoloid type along the sub-Himalayan regions; brachycephalic, short, flat-nosed with alveolar prognathism. This type appears in several variants.

G. A mongoloid type with medium stature, longish head, and medium nose, but with the typical mongoloid features of face and eye. This type occurs in Assam and Northern Burma.

All of the foregoing classification seems to be established indisputably, with the exception of the pigmy strain. Dr Guha's anthropometrically distinguished types
are apparently substantiated in the gallery of portraits appended to his monograph. His summary of prehistoric skeletal evidence is less satisfactory, probably because the material is very scanty. Guha finds probable descendants of the Combe-Capelle race, Armenoids, Alpines, Mediterraneans, and Proto-Nordics, as well as primitive Australoids. The skeletal remains include nothing which can be identified as Negrito, although the author is sure that such an element must have been present in the population. The reviewer finds nothing inherently improbable in these conclusions, but is inclined to regard them with reserve. As to the anthropometry of the living races—if one brings home the bacon, it is possibly immaterial whether one has got it with a shot-gun or a blow-gun, with Pearson's Coefficient of Racial likeness or with Dixon's infamous trinity of indices. In any event, Dr Guha has produced a magnificent and invaluable study.

Dr Datta, in his "Races of India," proceeds with great gusto to macerate all of the anthropologists who have dealt with the peoples of India, beginning with Risley and ending up with the Baron von Eickstedt and some one who Dr Datta refers to, rather contemptuously, as "the Census reporter." One suspects this unfortunate to be Dr Guha. As an iconoclast, Dr Datta wields a most vigorous hammer. He seems to feel, and probably rightly, that Indian anthropologists have been altogether too willing to accept the speculations of Europeans as to racial origins of the Indian population. He comes to the conclusion that what is needed is more of accurate and systematic investigation and less of speculation. The reviewer (who has not written anything in the nature of a serious contribution to the anthropology of India) found Dr Datta's critique most stimulating and amusing. The other two papers of Dr Datta indicate that he is active in anthropological research as well as in criticism. More power to his elbow!

Baron von Eickstedt's brochure on "The Position of Mysore in India's Racial History" was written as an introduction to a number of volumes on the tribes and castes of Mysore by Mr Ananthakrishna Iyer, described as the "Nestor of Indian Anthropology." Dr von Eickstedt's contribution is an anthropo-geographical excursion upon the relation of Indian races to their environment, a classification of the races, and a reconstruction of their historical sequence in Mysore. The present review must be limited to a brief outline of the author's racial findings. He recognizes three main racial divisions:

I. Primitive jungle group— Ancient Indians or Weddid
   (1) Dark brown, curly haired with totemistic mattock-using culture (with matriarchal influences): Gondi race
   (2) Black-brown curly (narrow curls) with originally ancient culture (with foreign influences): Malid type

II. Mixed and dislodged group—Black Indians or Melanids
   (1) Black-brown progressive people in the most southern plains with strong foreign matriarchy (now strongly overstratified): South Melanids
   (2) Black-brown primitive people of the northern Deccan forests with strong foreign (totemistic and matriarchal) influences: Kolid type

III. Racially progressive people of the open regions—New Indians or Indids
   (1) Gracile brown people with enforced patriarchy: Gracile Indid race
   (2) Coarser light brown people with possible original patriarchal herdsmanship: North-Indid type.
The reviewer, who yields to none in admiration for the prodigious industry of Dr von Eickstedt and who hereby highly resolves to read his vast work on the races of man, is revolted by his innovations in racial terminology. All of the Ids from Hominid to Mediterranid and Brachid seem superfluous, cacophonous, and Germanid. His Weddidd group corresponds with Guha’s D race, his Gracile Indid with Guha’s A (Mediterranean) race, and the Coarser Indid with Guha C (Proto-Nordic). He recognizes no Armenoid and no Negrito elements. Von Eickstedt’s effort to bring the races into relation with their geographical environment and with their social structure is interesting. The reviewer does not venture to offer an opinion as to its validity. There is a passage in the chapter which merits quotation:

With the question as to the causality of racial facts the temporal moment of the development of the Hominids (mankind) enters the foreground, and from racial anthropology originates racial history. By this the method is fixed: the curve of racial historical happenings results from the inner connections between the somatic facts and their biological dynamic on the one hand, and the different spacial conditions on the other. Its course, disclosed on [sic] a deductive way, then underlies the critical comparison with the results of the neighbouring historical disciplines which have arisen in the same spacial unit (p. 71).

The foregoing statement ought to arouse the interest of methodologically inclined readers of the AMERICAN ANTHROPOLOGIST.

This woefully inadequate review must not neglect at least a mention of the numerous and valuable ethnographic notes upon the tribes of India edited by Mr J. H. Hutton. A bare listing of topics and authors would consume a page of space in this periodical. One can only congratulate Mr Hutton and refer American anthropologists to this treasure-house of material.

E. A. HOOTON

HARVARD UNIVERSITY


In this small volume Professor Hurst has condensed the contents of his earlier presentation of the same subject, published in 1932 as “The Mechanism of Creative Evolution.” It is by the author’s word for the general reader, a popular epitome on “recent research in genetics in so far as it is concerned with the origin, evolution and ascent of man.” But the scope of these 138 pages is even wider than this statement would imply; Dr Hurst aims to build up a picture of the whole biological procession from the lowly progenie of recent biological speculation to a far-distant “immaterial type of being” of his own imaginative conception, and to give what he regards as the most plausible account of the dynamics involved. Obviously there can be little space for any very detailed analyses of controversial issues.

A large part of the treatise deals with specific genetic topics: the functioning of the gene complex in the distribution and development of traits, the sources of change in germinal material through gene mutation and chromosomal transmutation, disjunction, dislocation and polyploidy, the mechanism of sex and its interpretation in terms of the relatively new theory of genic balance. The presentation, however, is not textual in character. It is highly selected for the purpose of explaining evolution
and Dr Hurst discusses only as much of the material of elementary genetic doctrine as may be necessary for initiating the non-professional reader into the idiom. In the application of experimental results to the interpretation of evolutionary problems he has synthesized the latest available data and filled in the outlines of the Mutation Theory with ease and conviction. The general reader will resent the simplification in proportion to his awareness of the questions left unraised.

The author’s attack on the problem of origins is in line with considerable recent biological speculation on the apparent similarity between the auto-catalytic viruses and the bacteriophage, and the gene. He reconstructs an early progene, half-alive, “midway between a self-reproducing chemical ferment and a living ultra-microscopic organism” which by a fortunate combination of atoms achieved the power “to grow, to reproduce and to mutate.” In the course of evolutionary time the superior genes survived and incorporated, as it were, into chromosomes, whence was derived the setup for the progressive evolution of more and more complicated forms and the functioning of hereditary material as the geneticist now understands it. The account as presented is a persuasive one, but the amount of clearly established evidence is slight. Dr Hurst does not indicate it specifically, nor does he make entirely clear to the reader the line between reasonable inference and the purely hypothetical reconstruction.

The remaining chapters deal with Man, the final product of evolution to date. Dr Hurst presents a panoramic view of the history of civilization by way of illustrating the character of Man’s most distinctive evolutionary potentiality, mind. He suggests that future progress will be chiefly in the direction of increased intellectuality, provided man can be moved to “replace natural selection by human selection,” by the increased fertility of the high grade.

CAROLYN ADLER LEWIS

COLUMBIA UNIVERSITY


The author assumes a causal relationship between environment and race, and announces as his thesis “that a natural selection for an economy of various food substances has played a very important part in guiding the evolutionary process.” Thus Man’s peculiar physical type may be due to a possible combination of lime-deficiency to encourage femininity, iodine-shortage to favor foetalization, and phosphorus-shortage to cause slow-growth (pp. 23–24). This assumed constellation of negative qualities (plus others) is invoked to define the first proto-hominid: “a hairless, hydrocephalic, achondroplasic, and therefore in most senses degenerate, variety of the big Dryopithecus . . .” Fortunately “both in the matter of mineral economy and in cerebral capacity this first proto-hominid was greatly superior to his predecessors” (pp. 127–28). The human ego is somewhat soothed!
The author brings to bear upon problems of human origin and evolution a good working knowledge of physiology (animal and plant), biochemistry, soil chemistry, genetics (cattle breeding), and climatology. One can hardly demand that all of these varied approaches be adequately treated, yet one cannot escape the feeling that too often hypothesis is accepted for fact; too frequently the reasoning is purely teleological. In the utilization of genetics, for example, there crops up the temptation to evoke long-hidden or suppressed genes as responding to a similar environment, or to explain convergence; there is too much reversal: legs are shortened, arms lengthened, and vice versa, whenever the change in limb-proportion is to be explained (cf. pp. 127, 141 ff., 149). Again, to explain the deployment of races, following the initial transition from Anthropoid to Man, Maretz hypothesizes a "Northern, or Palaeanthropic, type adapted to arid country, and a Southern, or Neanthropic, sub-species adopted to the decalcified soils of the tropical rain-belt" (pp. 39, 195 ff.) It is the unequal rate of evolution in these two forms that accounts for the appearance of mixed Man-Ape types, e.g., human cranium and simian jaw of Piltdown (p. 197).

The book is thought-provoking, with many suggestions for future work. Not the least suggestive is Maretz's definition of race: "the temporarily stable product of a mixed ancestry that, for the time being, is not being subjected to either in-breeding or intense selection" (p. 194).

The author refers to his book as "necessary bridge-work." In the same sense we must conclude that much of it is only in the nature of a temporary filling. The book impresses the reviewer as a patchwork of doubtfully related ideas woven together by strands of wish-thinking. The hypothesis is willing, but the facts are weak.

W. M. Krogman

Western Reserve University

GENERAL


This work takes up various problems of language and speech, and attempts to relate them to human behavior. Linguistic change occupies a major part of the book: the theory is advanced that linguistic change takes place in response to a law of equilibrium requiring that a state of balance be maintained between the frequency of an element and its phonetic complexity. As evidence, data on the correlation of these two factors in several features of a number of languages are brought forward, and the attempt is made to show that certain historical processes make for the preservation of the correlations.

Studying the frequency of lenis unaspirated voiceless stops as compared with the corresponding fortis aspirated stops in Danish, Peipingese, Cantonese, and Burmese, Zipf finds that the former outnumber the latter in each case. I have found corroboration of this general tendency in short samples (1100 to 2400 phonemes in
extent) of Navaho, Takelma, and Dakota: the lenis unaspirated exceeded the fortis aspirated in 11 out of 13 pairs. The actual ratio varies widely: in Zipf’s material, from (approximately) 19:1 to 3:2; in my count, from 46:1 to 1:3. Thus, there seems to be a rather general preponderance of the less complex type, but the actual ratio is far from constant and exceptions are not absent.

But Zipf seems to consider the actual ratios of relative frequency valid to the point of using them in place of and in contradiction to ordinary phonemic method (study of phonetic and distributional facts in utterances known to be homonymous or non-homonymous to native speakers) for determining what is phonemic. This seems to be what he does (p. 321) in suggesting that the dentals and cerebrals of Singhalese be combined as “binary phonemes” because he finds the ratio of t+t to d+d more normal than that of t to d and t to d taken separately.

I gather that Zipf sees in correlations between frequency and complexity and in the presumable tendency to maintain acceptable correlations, an explanation of virtually all linguistic change. This view is untenable because, in spite of some general tendencies, the correlations seem to be obviously too diverse to show more than the vaguest general tendencies, and because there is at least one other kind of factor that definitely seems to have an effect on linguistic change regardless of or in spite of frequency or complexity. This influence is the social one, the tendency for persons or peoples in contact with one another to influence each other’s behavior. The fact that geographically contiguous languages tend to have common traits is but one evidence of this important influence.

Studies of the correlation of frequency and complexity have to face at least two serious difficulties. Aside from the great care, expert discrimination, and tremendous labor involved in determining relative frequencies in adequate samples of sufficiently representative material, there is the fact that relative complexity is determinable only in the roughest terms; in many cases it is not even possible to determine which sound is the more complex, let alone to arrive at any notion of the actual ratio of complexity. Another difficulty is the great scarcity of languages on which we have adequately worked-out and accurate descriptive material.

Yale University


This volume by the rector of Exeter College, Oxford, is an omnium gatherum of three Presidential Addresses, three Donellan Lectures (1933), one paper (Ritualism as a Disease of Religion) read at the International Congress of the Anthropological and Ethnological Sciences (1934), another (The Sacrament of Food) written for “Essays Presented to C. G. Seligman” (1934), together with material culled from

1 In one case (Dakota c:c’), the lenis unaspirated is outnumbered 1 to 3. Consistency between the halves of the count seemed to indicate that the ratio would not be greatly changed if the count were extended.
Hammerton's "Manners and Customs of Mankind" (the Introduction and seven chapters), and two essays on technology from Harmsworth's "Universal History."

All of these items, then, except the Donellan Lectures and one Presidential address, have been previously published. The Lectures, under the titles "Religious Feeling," "Religious Thinking," and "Religious Acting," expound the familiar thesis of the author in regard to the earliest stage of religion, for which he now adopts the term "pre-theological." This appellation is preferred to "godless" since the latter word "has an ugly sound and might therefore prejudice fair discussion of what I venture to propound here as a problem of some scientific interest." Marett adopts a reconstructive evolutionary approach to religious origins which he implies is characteristic of anthropologists, although some uncertainty of its value lurks in his aside "for I am just as bad as the rest" (p. 82), and the subsequent remark, "however unsatisfying, it is the best that can be done on the existing evidence." Perhaps it is, if the pseudo-historical use of ethnological data is the beginning and the end of anthropology.

To American anthropologists the cavalier manner in which the author embraces the trinity of psychological categories, of which the title of the book is a reflection and from which he takes his point of departure in the Lectures, will be of interest.

Let me proceed to consider the beginnings of religion [he says], seeking for them first and foremost in the domain of feeling. For working purposes the conception furnished by Analytical Psychology of mind as a trinity of functions, constituted by feeling, thinking and acting, will serve well enough. Moreover, Genetic Psychology treats feeling as the basic element that finds expression in acting with an intermediation on the part of thinking that does not become effective until mental evolution is well advanced. The chances are, then, psychologically speaking, that if religion corresponds to any ingrained disposition of human nature it will come out of feeling, and through feeling in acting, before thinking can get to work on it. Just as the child wants and tries to talk long before he can express himself articulately, so, I believe, it is with religious experience; which needed and sought a god, if one likes to put it that way, long before it knew him by name. So let us consider the genesis of religion first of all in the light of the impulse or set of impulses that it arose to satisfy (p. 86).

Since Marett says in the Preface that despite its somewhat miscellaneous contents the book has "unity in the sense that its working principles are throughout the same," the sample given of the author's methodology and assumptions in probing religious origins is sufficiently indicative of the treatment accorded much of the other material in the volume.

A. IRVING HALLOWELL

University of Pennsylvania


This paper is an excellent critique, by a philosopher, of the theories of the men enumerated in the title. There is nothing of the uncritical, receptive attitude of the layman in Dr Maitra's treatment. He has discriminated fact and interpretation, and
has judged each theory according to its own internal merits and weaknesses. He has extracted the essence of each theory and presents it lucidly. In the light of his own conclusions, the recurrent questions as to the priority of magic or religion, the antagonism between these two, the antagonism of the two to science, appear trifling and irrelevant. They seem to have arisen as a result of inadequate definitions made on the basis of "genus and species." Dr Maitra's own definition is in terms of relations—a functional definition. Religion, according to him, is an experience of recovered unity or harmony with reality, after one of estrangement or separation. "The really important factor in religion is the religious end." Any means used to this end, whether magic, prayer, mysticism, science, Buddhistic nihilism, is religion. The means chosen depend on the type and complexity of the culture.

This is an illuminating study and a valuable addition to our literature on religion.

D. DEMETRACOPOULOU

CLAREMONT, CALIFORNIA

The Origins of Religion. RAPHAEL KARSTEN. (293 pp. 12s. 6d. London: Kegan Paul; Trench, Trubner and Co., 1935.)

In this book, Professor Karsten seeks to determine the origin of (a) Religious Beliefs, and (b) Religious Cult. On the whole, he tends to agree with Tylor as to the importance of animism, though he conceives animism in a broader vein. He believes that there is no such thing as animatism apart from a belief in spirits and souls. All animatism, he tells us, derives from and is a branch of animism. Impersonal power is an abstraction, and therefore must have arisen late in the evolution of culture, since primitive man does not have the power of abstract thinking; furthermore, the evolution of thought proceeds from the concrete (spirits) to the abstract (impersonal power), not vice versa.

To him, belief in the soul is all-important in religion. Magic is associated with the idea of the soul. The worship of plants, animals, inanimate objects, as well as fetishism and ideas of supernatural power inherent in natural objects—all these arise ultimately from a belief in the soul. Man endows objects with spiritual life through a projection of his own psychical life. Totemism is based on the idea of the transmigration of souls. Spirits, demons, ghosts are either souls which "interfere in a mysterious way with the destiny of man," or have been evolved through an analogy to the soul.

The section on Religious Cult discusses such things as the treatment of disease, shamanism, priesthood, sacrifice, prayer.

The treatment of the subject is comprehensive but lacks insight. The author believes at bottom in psychic unity and set evolutionary stages, and uses the comparative method throughout, though he warns us against these in so many words. Often, the ceremony of one tribe is explained in terms of the belief of another, distant tribe, or is given a motive which the writer considers "obvious." On the whole, I feel that though certain theories have been aired and patched, our understanding of religion has not been furthered through this study.

D. DEMETRACOPOULOU

CLAREMONT, CALIFORNIA

Professor Lévy-Bruhl's latest book is in large part a compilation of examples on the following topics: amulets, charms, and talismans; omens, good and bad luck; baneful influence of ill will and rites for propitiating persons, plants, objects, and animals; worship of ancestors and the dead; witchcraft; transgressions and incest; defilement and purification in connection with mourning, homicide, hunting, intercourse, childbirth, menstruation, etc. Examples are drawn principally, but not exclusively, from eight or ten standard ethnographies on Australia, New Guinea, Dutch East Indies, Africa, and the Eskimo. The thesis which justifies this collection of disparate data runs briefly as follows: primitives, because of their pre-logical cast of thought, do not categorize phenomena factually but emotionally; to group ethnotologic data in "scientific" categories is a distortion; fear is the one basic affective category which underlies all of the behavior and the belief connected with the topics discussed in this volume; therefore it is not surprising to find similar rites used in very different connections since the psychological motivation is constant; therefore also, primitives are traditionalists because sanctioned behavior in the past has preserved the group from the things it feared; this attitude benumbs the intellect. In other words, Professor Lévy-Bruhl is suggesting psychological causes, or at least concomitants, for his formulation of the pre-logical.

I believe that this is a fair statement of the underlying principles in a book which is not always distinguished for its clarity. There is much which is suggestive if not provenly valid in the thesis. Emotional rather than intellectual categorizing of primitive data might lead to interesting results. For example, cultural elements like the scratching stick, seclusion, and the drinking tube have wide and scattered distributions in the New World, due presumably to historic events about which we are at best only able to make conjectures. These traits appear in a whole series of cultural complexes like shamanism, menstrual observances, vision quest, and so on. Might not a working assumption be that these elements, everywhere in solution, have crystallized out into complexes with very different cultural functions under the influence of an emotion common to all the complexes? In other words, there are series of elements in the cultural realm associated with emotions in the psychological realm. The cultural elements are used as devices for dealing with emotional situations. This type of approach might furnish a partial explanation of puzzling similarities between different complexes and between the same complexes widely separated in space. It fails to explain the presence of the elements and their association with given emotions, but it might elucidate the problem of similar elements in disparate institutions.

HARVARD PSYCHOLOGICAL CLINIC

CORA DU BOIS
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General

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DISCUSSION AND CORRESPONDENCE

SOME WIDER OBLIGATIONS OF THE FIELD ETHNOLOGIST

In his discussion of "Africa: What Do We Know Of It?" Edwin W. Smith suggests¹ that the complaint of geographers who urge that ethnologists working the African field have provided the world with less geographical knowledge than one has a right to expect, is not unreasonable. I know nothing from first hand of this specific situation, but I do think the implication of the complaint is worth considering. From it I would derive the thesis that although the anthropologist should be the master of his own techniques in the study of primitive culture, nevertheless he has certain obligations to science as a whole (if he is to present a well rounded survey of his chosen native society) which inevitably make him the servant of many sciences. I think this thesis is valid, and I wish to make a plea for a greater recognition on the part of anthropologists of their obligations in this respect.

In spite of theories and schools to the contrary, I assume it is the task of the anthropologist to study all aspects of the life of the people with whom he is directly interested. This means, of course, that he does not confine himself exclusively to a rather narrow definition of his field. It is not uncommon for the fieldworker to study little else than the language, social organization, religion, and folklore of his people. I do not quarrel with personal tastes but I would suggest that this limitation of interest is not good science. It has produced capable monographs of certain aspects of primitive cultures but for all the reader or the fieldworker is concerned, the tribe under discussion may be living in a physical and environmental vacuum. There is rarely any information to the contrary. Yet it is often the case that the anthropologist, with considerable expenditure of time and money, penetrates into obscure corners of the world. The expense of an expedition may be such that it is impossible to send with him a physical anthropologist and a natural scientist. Yet when the anthropologist completes his field study and his report, how often is it that the general sciences or even physical anthropology are the wiser for his experience? Rarely, I think. But it still remains true that had the anthropologist been broader in his interests in the first instance, not only would cultural anthropology have gained, but other general sciences as well would have received information (crumbs perhaps, but valuable crumbs that could surely be fitted into a general or particular scheme of knowledge).

Illustrations may make my meaning clear. I presume that physical anthropology would welcome all the data it could get. Rarely does the ordinary schooled worker acknowledge this interest even though he penetrate into the wilds of New Guinea or wander among the mazes of South America. He assumes anthropometry is outside his province in the first place and may produce the further rationalization that anthropometric technique is too difficult to acquire. Any physical anthropologist would,

I think, vigorously oppose the second suggestion and the ethnologist has no reason to assume the first. A few demonstrations in the handling of instruments and a general familiarity with simple required measurements will give the ethnologist a sufficiently good technique to enable him rapidly to secure a series of measurements that would be extremely valuable. The series may well be turned over to the specialist for analysis. But the material is collected whatever happens to it. If not immediately, at least some day, it will be of value in closing a gap in otherwise complete knowledge. Too often the ethnologist feels that he is not expected to be interested in anthropometry. My point is that whether he is interested or not, he should willingly assume the obligation of making measurements. Only by coöperation can the sciences of man advance on a united front. Even the student interested in the study of personality differences will not find anthropometry unrewarding. There is much to be learned from close observation of subjects under the test situation. A careful eye will discover subtleties of cultural and individual reaction that may be followed up with advantage in later and more intimate study. A well known anthropologist once told me that on his way to, say, Timbuctoo, he was so bored by the prospect of having to make physical measurements that he quietly dropped his set of instruments overboard one night and explained later to inquiring authorities that they must have gone astray in transit. He felt his action was humorous. I felt that in spite of his reputation as an anthropologist, his appreciation of the spirit of science and scientific method was not even rudimentary.

Some fieldworkers are fortunate in studying in areas where complete geographical and climatological data are already available. That they sometimes make rare use of them is another matter, though one fails to see how a study, intensive or slight, of the economic aspects of a primitive culture can be complete without the use of material bearing on the relationship between a people and the land they live in. Less fortunate workers find that there are no geographical data available for their area. Rarely do they see the obligation to provide this, not only to make their own study complete, but to assist geographers in general. Again, did they recognize their responsibility, it would not be hard to implement. A half hour lesson in the reading of climatological instruments and study of the Beaufort wind scale would enable any fieldworker or an easily trained native assistant to keep adequate record of weather conditions with little expenditure of time. Such records, even if continued for but a short time, enable the ethnologist vividly to suggest seasonal changes; when correlated with seasonal changes of economic activity they have the value of presenting factually what would otherwise be less well said. Other geographical knowledge may often be obtained from a simple day to day record: material on the rhythm of vegetation and animal life, the regime of rivers and swamps, the modification of natural vegetation by man, and the influence of environmental factors on the material life of the people—these are important and easily observable phenomena.

In dealing with the economic patterns and material culture of any society a detailed and recognizable knowledge of the flora and fauna of the environment is surely desirable. In some areas the flora and fauna are well known; in others, not so definitely. Again it should be of value for the anthropologist to procure such
specimens as will complete his own report and at the same time assist the botanist or zoologist in studying plant or animal distributions. The collecting of botanical specimens is not difficult. Native assistants are often only too anxious to help. Even if specimens are limited only to the plants and trees that have an economic, medicinal, or social value to the native people, this should of itself provide sufficient incentive to make such a collection. Any competent botanist can supply scientific identifications later, and any laboratory an analysis of plants believed to have medicinal value. In the collection of zoological specimens difficulties are naturally much greater and treatment of specimens for preservation may often require more technique than the anthropologist may legitimately be expected to possess. But insects are not hard to collect, nor are the smaller fish. In both cases preservation is easy. This is true also of reef, beach, and mountain fauna like sea and land shells. They are easily secured, easily preserved, and a factual knowledge of them will often add clarity to the report on fieldwork.

I do not touch on the problem of where instruments, measuring apparatus, and collecting bottles are to come from in the first instance. There is no reason why the anthropologist should be expected to equip himself from his usually inadequate budget. But cooperation with university departments or museums will provide what is needed with willingness and alacrity.

I am still serious when I suggest that it should not be without the province of the fieldworker to know enough of elementary astronomy and star identification to enable him adequately to test his informants’ knowledge of these matters. Contrary to opinion such knowledge is not difficult to acquire. By its aid the fieldworker will be better able to appreciate the value to the people he is studying of their own knowledge, often not inconsiderable, of the movement of stars and planets and their relations to seasonal change, their value in navigation, folklore, and legend. In my own experience idle gossip with informants when sitting outside at night may well be turned towards the stars and not only adequate identifications but much related knowledge secured with relative ease.

I believe the same values hold in a slightly different sphere for an elementary knowledge of anatomy and physiology. Only with the background of this knowledge is it possible to study with most efficiency native beliefs and practises in regard to medicine, diagnosis and treatment of disease, practises relating to childbirth and initiation. I do not suggest that every anthropologist should be a properly qualified student of medicine. That would sound ridiculous but it would be far from fantastic. What would be of value to the fieldworker would be that practical knowledge which would enable him to use with ease and freedom medical dictionaries, anatomical and physiological charts and diagrams.

I do not feel that the objection to this suggested widening of the scope of fieldwork which is based on an assumed lack of time has any general validity. Few ethnologists living among a native people can honestly assert that they work for the whole day every day with formal sessions. There are always odd hours and odd days when informants are not inclined towards formal discussions. It is during these times and during other odd moments, when change of activity and thought is a
welcome relief to both worker and informants, that it is possible to acquire borderline information without undue tears.

The implication of my thesis then is that anthropologists tend to have too narrow an outlook and too limited a technique. It is convenient but ostrich-like to assume that a primitive culture consists of little other than its broad social organization, its religion, its language, and closely related subjects. This view enables one to be a "cultural anthropologist" certainly, but only at the cost of blissfully neglecting as unimportant whole aspects of primitive culture. Schools of anthropology are at fault when their training is unduly narrow. This is perhaps but a reflex of the personal idiosyncrasies of teachers. But I see no valid reason why personal biases should be projected as if they alone embraced the whole field of anthropology with the implication that it is slightly indecent to be interested in any other aspect of primitive culture. We would realize the futility of this if a teacher implied that all linguistics was useless, or all archaeology a waste of time. Why not recognize then that a knowledge of geographical and climatological data, of botany, of native nature science, of anthropometric measurements, of simple astronomical and medical principles are also of value in preserving for present or future study aspects of primitive society that are no more unimportant to the native himself, or to science at large, than information about the kinship system or the functioning of marriage regulations?

One feels in this connection that there is a much wider outlook to be gained if the student has the opportunity to work in close connection with a museum that is interested in covering not only the anthropological field but is concerned to utilize also the services of botanist, zoologist, physical anthropologist, and entomologist for a concerted attack on a specific problem or on a large ethnic province. In this way the anthropologist realizes the values of coöperation between the general team of sciences. He can understand that in order to work efficiently and with a minimum waste of effort and money, coöperation is vitally necessary that each may contribute according to his opportunity field information that will help establish, for instance, botanical or racial distributions, and otherwise help to build up a generic picture of many different cultures within the one geographical area. Polynesia comes to mind here, Melanesia as well, or Papua, New Guinea, or Indonesia. These are all areas that can well afford to develop or further a coöperative technique of scientific research that will be of mutual benefit to each science. The Bishop Museum has splendidly sketched the outline of such coöperative research in Polynesia. Its work has also suggested that though it is fashionable to ridicule "Notes and Queries" in some anthropological circles, nevertheless digested with a grain of common sense the viewpoint from which the "Notes" were compiled will go far to help the anthropologist understand more fully the culture which he wishes not only to make his own but also to present in intelligible manner to the world of his scientific co-workers.

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ERNEST BEAGLEHOLE
NOTE ON PIMA MOIETIES

According to the data of Russell and Parsons,¹ the role of moiety and gens among the Pima was so weak—whether from the outset or through a process of attenuation—that it is difficult to point to anything in which these social frames did function. There was, to be sure, the use of gens names when the child addressed the father. There was also the teasing of people belonging to different moieties; bragging; and looking upon the red or grey "cowkiller" insect as a sort of moiety pet or badge.

In pursuing on various occasions studies of music and language among the Pima of the Gila River reservation, I received further information and comments on the subject from my main informant, Thomas Vanyiko of Sacaton, Arizona. These are offered here in view of the scarcity of our information on Pima social organization. Vanyiko is now about eighty years old, a story-teller and singer, considered by the modern Pima an authority on old Pima culture. The statements were checked with him a number of times. Since so few old Pima are left who may be competent to consult on the matter, it may not prove possible to check the information with others.

In olden days, before my time, the moieties played against each other in the games. This is what I heard. It was done so there wouldn't be any cheating. It was done in the Pima guessing game,² perhaps also in the dice game. It was done also in the foot races. There too it was done to prevent cheating; sometimes they tried to buy out a runner, or to scare him.

Naturally, these functions of the Pima moiety are not necessarily originally connected with the Pima moiety organization, but may represent the diffusion of a moiety alignment on occasions of gambling and racing only.

The moieties came to be in the following way. Both Coyote and Buzzard were great warriors. Some people grouped themselves around the one, in fighting or in hunting the deer, some around the other. Some asked Coyote for bravery in war, some asked Buzzard for it.³ So there was a kind of rivalry between the two groups, and they began to reckon themselves apart.

Buzzard is red,⁴ and the Buzzard people are red. Coyote is white,⁵ and the Coyote people are white. This is because the ceremonial father⁶ of Buzzard was red whirlwind, the ceremonial father of Coyote was white whirlwind. Buzzard could spread heat; Coyote could not stand heat. Coyote could spread cold; Buzzard could not stand it.

² That is, the canute, reed game, as distinguished from the Yuma guessing game which the Pima took over, where the hiding is done with the hands held behind the back.
³ I.e., they asked them for dream revelations in which to acquire bravery or luck in war.
⁴ In songs and myths Buzzard is always referred to as "Yellow Buzzard."
⁵ "Grey" is also mentioned in songs; "Grey cross-cousin."
⁶ The old man delegated to care for a warrior who had killed an enemy, while the warrior was in the seclusion of the purification period. See J. William Lloyd, Aw-aw-tam Indian Nights (Westfield, N. J., 1911), pp. 90–94.
All the animals are either red or white. The Bear is red, for instance. That's why one must tell him, "I am red," if one meets him. Everything red is red people; everything white is white people.

The term vi'mi'gar [cross-cousin] is not used by itself for the moieties. One does say, "coyote cross-cousin," or "buzzard cross-cousin." But one does not address this way, or with the word "cross-cousin," a member of the opposite moiety. The terms are terms of respect which one uses if he speaks about the other moiety; they refer primarily to Coyote and Buzzard, and then to their people, who are the two moieties.

Vanyiko's explanation of the use of the term "cross-cousin" for the moieties went back to the Creation myths. After the Flood, the Creator, Elder Brother, and Coyote have a discussion about which of them emerged from the Flood first, and should thus be considered senior. Coyote's claims are dismissed, but the two other protagonists tell him: "All right, you'll be our cross-cousin!"

The moieties—or the gentes—did not have anything to do with ceremonies or dances: they were all mixed up. In war they were all mixed up. A moiety or gens did not own any stories that the other moiety or gentes could not tell. There was no office for which people of a certain moiety or gens had to be chosen. It just happened that the last chiefs were all from the Buzzard people, as the office happened to get into the Azul family, who are Buzzard people. There was never a rule that people of one moiety had to be buried by people of another moiety.

Olson's study of clans and moieties gives under "Concepts adhering to moieties" the following entry for the Pima and Papago: "Red side linked with earth, white side with underworld." To my knowledge, the only pertinent item in the literature is Russell's reference to the migration legend: "The Red People are said to have been in possession of the country when Elder Brother brought the White People from the nether world and conquered them. . . ." The legend, however, speaks of the five Pima gentes all emerging together. At any rate, the association seems very vague and tenuous, and is so in Pima thinking; so that it is hardly justifiable to speak of a "concept" associated with the moieties on the basis of this evidence.

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AN ADDED NOTE ON THE SCOTTSBLUFF QUARRY

The recent progress in the study of the Folsom complex (see, for example, Roberts, Howard, Antevs, and others) has contributed much new information on the subject. It is in the light of this additional knowledge, and in the endeavor to keep

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7 See Russell, p. 262.
8 The statements in this paragraph were all in answer to my questions.
10 See Russell, p. 197.
11 Besides Dr. Roberts' more recent unpublished material, and the as yet unpublished geological studies of the Lindenmeier site made by Dr Kirk Bryan.
the record as free from inaccurate details as possible, that we wish to offer a few short comments on our previous paper in the American Anthropologist.²

It has become evident from excavation at Fort Collins, Colorado, that true Yuma blades are almost entirely lacking at this site.³ In addition, definite occupation levels revealing Yuma artifacts and “Folsom-like” points⁴ completely lacking grooves have since been discovered in old deposits in western Nebraska. Flint from the latter sites, deeply buried under what appears to be a Pleistocene lake bed, is similar in composition to that from the Scottsbluff bison quarry (whose flint was not of a very common texture for that neighborhood) and would seem to add supporting evidence that the latter quarry is not a fluke of redeposition.

One of the points from the new site is to be featured by E. H. Barbour and C. Bertrand Schultz in a paper now going to press.⁵ It is similar to what Renaud has classified as Yuma type 2b. This finely worked, but ungrooved “Folsom”⁶ blade suggests that what the writers assumed, in their report on the Scottsbluff quarry, to be merely an atypical and unfinished Folsom blade (lacking the groove) is actually an ungrooved stage of Folsom type development, accompanied by typical Yuma points. Since Dr Roberts has found, so far, but one true Yuma blade at the Fort Collins site, it would seem that, though undoubtedly related, the grooved Folsom blades do bear some time sequential relation to the Yuma type. In other words, if they are related, as they seem to be, our assumption of more or less exact contemporaneity for Yuma-Folsom artifacts⁷ should be changed to a statement of only partial contemporaneity. As to which may precede the other the tendency among archaeologists the last few months has been to assume that the Yuma type is a later—even late—innovation within the Folsom complex. However, the new Nebraska finds, which suggest extreme antiquity, may reverse the sequence again in favor of a more ancient dating for the Yuma types, as Renaud has long suggested. All that the writers wish to suggest at the present moment is that there seems not to be an exact contemporaneity, and that these “Folsom-like” artifacts without grooves⁸ seem

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³ “The tip of a true Yuma” (Dr F. H. H. Roberts, Jr., in litteris).
⁴ See footnote 6 below. Under Dr Renaud’s system of classification these would all be Yuma types.
⁵ Paleontological and Geological Evidence Relating To Yuma-Folsom Artifacts in Nebraska (Bulletin, Nebraska State Museum, No. 45, in press).
⁶ We use quotation marks with this term because this type is regarded by Renaud as a classifiable Yuma blade. Certain of these more Folsom-like points are by others not yet admitted as true Yuma artifacts, and hence the utmost care is necessary to avoid confusion in terminology.
⁷ Schultz and Eiseley, op. cit.
⁸ They fall approximately into Renaud’s Yuma types 2a and 2b, but would be what Howard has designated Folsom-like except that they lack even a trace of a groove. It is, however, possible, judging from points in collections in the vicinity, that when the new sites are fully excavated, true Folsom-like points as defined by Howard in “Evidence of Early Man in
definitely associated with true Yuma blades. But the new and extensive sites to be excavated in the coming season promise to throw much light on the problem. Further remarks at this time would be pure conjecture.

The bison problem is another upon which we should like to offer comment. It has been suggested by Mr J. D. Figgins\(^9\) that there is a mixture of *Bison oliverhayi* with *Bison bison* at the Scottsbluff site, as well as an accompanying "modern" artifact.\(^10\) Leaving aside all mention of the highly improbable nature of such a coincidence and the tendency to minimize certain points which we made in regard to skeletal articulation, we should like to point out:

First, that the so-called "modern" point has never been definitely shown to be a product of any associated recent site, and that, in addition, it bears certain characteristics which we have previously commented on, which take it out of the class of true notched points.\(^11\)

Second, that the bison material (of which Figgins had access to only two specimens)\(^12\) does not justify his claim that both *B. oliverhayi* and *B. bison* are mingled in the quarry. It is to be remembered that we provisionally referred these bison to *occidentalis* Lucas. Time has justified us in this caution. Later studies carried on through the generosity of Dr Barnum Brown and the American Museum of Natural History, who allowed the Nebraska State Museum access to the original Folsom bison, have shown that the male skull featured in our previous article resembles the Folsom skulls.\(^13\)

Most of the skulls from the Scottsbluff Bison Quarry were those of females, along with several from immature individuals. These female skulls also resemble those of the same sex from Folsom. All of this will be sufficiently discussed in the forthcoming paper by Dr Barbour and Mr Schultz. Our purpose here is merely to indicate that the more recent and intensive studies of the Scottsbluff bison quarry justify the reclassification of these animals as *Bison antiquus taylori*. It is a well established point of paleontology that the promiscuous naming of different species out of a single quarry, particularly from one possessing an obvious age-range in individuals, is a highly dubious procedure.\(^14\) In such forms as the American bison, where identifica-

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\(^10\) Found, incidentally, six feet back from the face of the bank.

\(^11\) Tanged points made their first known appearance in the late Mousterian of Europe. This is not said with any idea of comparison, but it is sufficient to indicate that, in itself, a tang is no guarantee of modernity!

\(^12\) One of which he designated as *B. oliverhayi*.

\(^13\) Dr E. H. Barbour and Mr C. Bertrand Schultz have gone into this in great detail, with tables of comparative measurements, in their forthcoming paper. The appellation *antiquus* is a change in nomenclature, not a reassignment of species, to adjust to the established priority of *antiquus* over *occidentalis* as a specific name for the southern bison of this type.

\(^14\) W. D. Matthew, Critical Observations on the Phylogeny of the Rhinoceroses (University of California Publication, Bulletin, Department of Geological Sciences, Vol. 20, No. 1,
tion is strongly dependent upon the securing of adult male specimens, the writers do not feel that identifications can be multiplied on such a basis as that suggested by Figgins.

In summary, the writers would merely say that the extensive new site from beneath a possible Pleistocene lake bed, already examined by competent authorities, and a preliminary report of which has been given at the meeting of the American Paleontological Society in New York recently, should go far to justify their assertions of the antiquity of the Scottsbluff quarry. They believe also that some sort of sequential relationship between Yuma and Folsom types now seems definite, but which is the earlier, and the part played by the Folsom-like forms, is still a subject for investigation. In addition, we take exception to calling one point "modern" simply because it is not typical of what has come to be considered as Yuma. This seems highly questionable in view of the unknown nature of a vast complex recognized from only one large excavated campsite to date. As for the geology of the Scottsbluff site, its undisturbed nature is attested to by Dr Paul MacClintock of Princeton, who examined the site during the past summer.

Mr Figgins, it is true, makes some point of our reference to chunks of Tertiary sandstones and clays as "supplying adequate proof of redeposition." If this were true these fragments should be indiscriminately deposited from top to bottom; a point which is sufficiently disposed of in our previous paper, as is easily seen by referring to the photograph (fig. 1) therein. Since Mr Figgins was not of the party that excavated the site and has had access only to the specimens to which we have referred, we find it difficult to discover on what basis he so confidently implies that all the articulated material in the quarry is to be attributed to the inclusion of modern bison "inextricably associated with the extinct oliverhayi." It is our opinion that this sort of hair-splitting over individual variation could be invoked over any bison quarry which happened to contain numbers of individuals of both sexes and all ages.

The writers are quite willing to rest their case on the forthcoming developments to which they have referred, and merely urge a careful and fair approach on the part of the reader through what must often seem a mass of conflicting detail. A whole new situation in American archaeology can hardly be clarified, nor its ramifications seen, in a few months' time.

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LOREN C. EISELEY

NEBRASKA STATE MUSEUM
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p. 6, 1931); Range and Limitations of Species as Seen in Fossil Mammal Faunas (Bulletin, Geological Society of America, Vol. 41, pp. 271–74, 1930).

Tooth structure has so far proved of questionable value in the identification of the bison of the late Pleistocene.


DISCUSSION AND CORRESPONDENCE

THE WITOTO KINSHIP SYSTEM

In surveying recently the ethnographic sources on the Witoto of northwestern Amazonia, I encountered partial lists of kinship terms in the works of Farabee,¹ Hardenburg,² Koch-Grünberg,³ Preuss,⁴ Schmidt,⁵ and Whiffen.⁶ The lists are clearly independent, but they supplement and confirm one another very well, although that of Schmidt reveals a number of errors. It seems worth while, therefore, to put the various lists together, especially since that of Preuss, the most nearly complete, is mainly scattered through a general vocabulary. Diacritical marks will be largely omitted, and the authors cited by initial.

A. Grandfather (cf. also C2 and D1 below). 1. marama, “grandfather” (W); mogama, “grandfather” (K); mokama, “stepfather” (P). These may or may not be different renderings of the same term. 2. mota uaiikka, “grandfather, grandmother” (S). Perhaps a descriptive term based on motai, “father” (C1), and the term for “old man,” uaiikka (P), oekesa (K).

B. Grandmother (cf. also A2 and G1). 1. einyoko, “grandmother” (W). Apparently a derivative or variant of einyo, “mother” (F2). 2. haino, “grandmother” (K). Perhaps a variant of einyo, “mother” (F2).

C. Father (cf. also F1). 1. motai, motaika, “father” (P); motae, “father” (K); mota, “father” (F). 2. moma, momaka, “father, ancestor” (P); moma, “father” (W); mon, “father” (H). According to Preuss (Vol. 2, pp. 732, 719), the term mo is used vocatively for “father” and (man speaking) for “son, son-in-law, stepson, foster son,” while ma is a masculine suffix.

D. Paternal uncle. 1. usuma, “father’s brother, old man” (P); iusuma, “uncle, grandfather” (F); hisaima, “uncle” (K). 2. iso, “father’s brother” (W); iso, “uncle, old man” (P).

E. Maternal uncle. hinobidyama, “mother’s brother” (P); hinobiyama, “uncle” (S); vichama, “mother’s brother” (W). The prefix hino- confers the meaning of “standing outside” (P).

F. Mother (cf. also G1 and Q1). 1. aitai, aitai, “mother” (P); aitai, “father” (S). A derivative of ai, “wife” (P). The rendering “father” by Schmidt is apparently an error. 2. einyo, enyo, nyo, “woman, mother” (P); einyo, “mother” (W); eño, “mother” (H); nyonyo, “mother” (P); fño, “mother” (F). According to Preuss (Vol. 2, p. 739), the term nyo is used vocatively for “mother, daughter, daughter-in-law, niece.”

G. *Paternal aunt*. 1. usunyo, usunyoka, "father's sister, older woman living with another sib" (P); usunnu, "aunt, grandmother" (F); usunyo, "mother" (S); usu, "aunt" (K). The rendering "mother" by Schmidt is probably an error. 2. ita, "aunt" (P); ita, "aunt" (S). Conceivably a term for "maternal aunt," although there is no evidence.

H. *Brother* (man speaking). ama, "brother (m. sp.), male cousin within the sib (m. sp.)" (P); ama, "brother (m. sp.)" (W); ama, "brother" (K); ama, "brother" (F); ama, "brother" (H).

I. *Brother* (woman speaking). 1. iyo, iiyo, iyoka, "brother (w. sp.), clansman (w. sp.)" (P); iyo, "brother" (H); tiio, "brother (w. sp.)" (W); ugo, "brother" (S); uyo, "male or female cousin" (S). 2. iima, "man, brother (w. sp.)" (P). Apparently the usual term for "man" or "male," variously recorded as eimie, eima, imie, iima (P); eima (K);igma (F); rema (W).

J. *Sister* (man speaking). mirinyo, "sister (m. sp.), clanswoman (m. sp.)" (P); mirinyo, "sister (m. sp.)" (W); mirenko, "sister" (K); minenyo, "sister (m. sp.)" (P).

K. *Sister* (woman speaking). evunyo, "sister (w. sp.), clanswoman (w. sp.), woman who has married into speaker's sib (w. sp.)" (P); epunyo, "sister (w. sp.)" (W); awunyo, "sister" (S); bunuo, "sister" (F); abunyo, "woman" (S).

L. *Son* (cf. also C2). 1. hito, hitoo, hitoka, hitooka, "son" (P); hito, "child" (W); hito, "son" (F); hitoo, "son" (K); hitoo, "grandson" (S). Schmidt seems again in error. 2. konirua, "son" (S); konirue, "young man" (P); konirue, "young man" (K).

M. *Daughter* (cf. also F2). 1. hisa, hisai, hisaha, "small child, boy (rare), girl, daughter" (P); hisa, "daughter, girl" (F); hisa, "daughter" (K); hisaka, "daughter" (P). 2. rinyohisa, "daughter, girl" (P); rinyosa, "girl" (W). A compound of rinyo (Q2) and hisa (M1). 3. riñoña, "daughter, woman" (H); rinyona, "women" (W); rinyona, "women" (S). Apparently a derivative of rinyo (Q2).

N. *Nephew*. 1. eneise, "brother's son, sister's son, stepson" (P); enasai, "brother's son" (W). 2. komona, "sister's son" (W). 3. itsotai, "nephew" (S).

O. *Niece* (cf. also F2 and Y). 1. eneisenyo, beneisenyo, "brother's daughter, sister's daughter" (P); enasanyo, "brother's daughter" (W). 2. momonio, "sister's daughter" (W).

P. *Husband*. ini, inika, "husband" (P); ini, "husband" (S); ini, "husband" (F); une, "husband" (W).

Q. *Wife*. 1. ai, aii, aikaa, aiikaa, "wife" (P); ai, "wife" (F); ei, "wife" (W); ei, "mother" (H); e'i "mother" (F); eikei, ekei, "mother" (K). The term may mean "mother" as well as "wife," although the better authorities favor the latter, or there may be two terms. 2. rinyo, "wife" (S); rinyo, rinyoka, "woman, girl, clanswoman" (P); rinyo, "woman" (W); rino, "woman" (F); rino, "woman" (K). Apparently merely the term for "woman." (Cf. M3).

R. *Father-in-law*. hifai, "father-in-law" (P); hifai, "father-in-law" (S).

S. *Mother-in-law*. hifainyo, "mother-in-law" (P); hifayunyo, "mother-in-law" (S).
DISCUSSION AND CORRESPONDENCE

T. Brother-in-law. oima, "brother-in-law, wife's clansman" (P); oima, "brother-in-law" (W); oma, "brother-in-law" (S).

U. Sister-in-law. 1. oinyo, "sister-in-law, wife's clanswoman, woman who has married into wife's clan" (P); oinyo, "sister-in-law" (S). Reciprocal with T (P). 2. ofainyo, "sister-in-law (m. sp.)" (P); ofanyo, "sister-in-law" (W). 3. rifainyo, "sister-in-law (m. sp.), co-wife (w. sp.)" (P).


W. Daughter-in-law (cf. also F2). 1. mio, "daughter-in-law" (P). 2. ota, "daughter-in-law" (S).

X. Stepfather. mokama, "stepfather" (P). (Cf. A1.)

Y. Stepmother. aikanyo, "stepmother, foster mother" (P); akanyo, "niece" (S). Schmidt is perhaps again in error.

Z. Stepson (cf. also C2 and N1). erekama, ereikama, "stepson" (P).

Z'. Relative by affinity. idyareima, "member of a sib allied to that of the speaker by marriage" (P).

The system, as presented above, is defective in specific terms for mother's sister, cross-cousins, and grandchildren. It is consistent with the social organization of the Witoto, who are divided into patrilineal, patrilocal, exogamous sibs, each occupying a communal dwelling in its own separate settlement. Kirchhoff,7 who derives his information from Farabee, Preuss, and Whiffen, though he cites only a few terms, correctly points out the important influence of the sib or settlement on the kinship system: "The kinship terms—apart from those for parents and children—fall into two groups: they designate either persons born in one's own settlement or those born in other settlements, i.e., those with which one's own is related through marriage."

GEORGE PETER MURDOCK

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NEW HAVEN, CONN.

A REPLY TO "A SURVEY OF SUMATRA"

To the Editor:

The January-March issue of the American Anthropologist of the present year contains an article by Raymond Kennedy entitled "A Survey of Sumatra" (pp. 145-48) which is a review of a recent book by Prof Heine-Geldern and myself on the peoples of Sumatra.1 Since a number of errors were indicated by the reviewer in my section of the book, the review is of service both to the general reader and the authors.

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On the other hand, I would like to take this occasion to protest against what I consider undue severity on the part of the reviewer. At times sentences were abstracted from their proper context and criticised on their own merits. Thus I am accused of saying on page 82 that the Batak house type is attributable to Hindu influence. As a matter of fact I make no discussion of Hindu influence on Batak house types on page 82. The subject is first broached by Prof Heine-Geldern on pages 320–21. Next I am accused of saying on the same page 82 that the Batak had neither shamans or priests before the days of Hindu influence. I did make this statement. But throughout the book I am using the Mentawei islanders and other low cultured peoples as checks. The Mentawei islanders had no real priests or shamans, and I therefore assumed that real priesthood and shamanism were derived from Hinduism, or at least from Hindu influence. The same rule holds true for iron working. Dr Kennedy believes that statements such as these are tenuous, and in his opinion, unsubstantiated hypotheses.

Naturally every reviewer is entitled to his own opinions, and the more opinions there are on a given subject, the better it is for the advancement of scientific research. Dr Vroklage, for example, takes quite a different point of view from Dr Kennedy relative to Hindu influence in Sumatra. Dr Vroklage in his review of my book praises it for having been written from a cultural historical standpoint, and not merely with an evolutionary point of view.²

Perhaps the most original contribution which I tried to make to Dutch ethnography, and seemingly the one least appreciated by Dr Kennedy, is my assumption that sib organization did not develop in situ in Sumatra, but was diffused from the mainland. The linguistic substantiation for this theory was first presented in the American Anthropologist and Anthropos.³

According to my line of reasoning, since the Indonesian term for mother’s brother is mama, and since this is a Tamil and Sinhalese term for mother’s brother, sib organization came with the term from southern India to Indonesia. In other words, where you have an identity in trait and an identity in nomenclature the chances for diffusion of the trait are strong. Dr Kennedy, however, does not agree with my theory, but cites Adriani in support of the contention that the word mama developed everywhere separately in Indonesia from the word for father, ama, by a process of reduplication. This of course is typical evolutionary reasoning. But how can the word mama evolve among the Sinhalese and Tamil from the word ama, when the Sinhalese word for father is not ama but tātā, and the Tamil term for father is appu or aiya?⁴ My general hypothesis is not contradicted by the fact that the word

² "Het is een zeer verdienstelijk werk van Dr. Loeb geweest, om met zin voor cultuurhistorische ontwikkeling en niet alleen voor de zoogenaamde evolutie" (Anthropos, Vol. 30, 1935, p. 596).


mama may be used in Indonesia for mother’s brother by sibless peoples. I found this true myself for Mentawei. Again, while there is no question that mama is the general term for mother’s brother in Indonesia, exceptions may occur. Dr Kennedy, for example, states that the Gayo of Sumatra use the term pon for this relationship.

In a similar manner the Dravidian terms (or similar terms) for older and younger siblings, akka and nangi, may be found both among Indonesian peoples with sibs and among those without sibs. The mere fact that the terms originated among Dravidians with sibs does not prevent the terms from being diffused to Indonesians who perhaps never had sibs. The general hypothesis still remains that the three terms mama, akka, and nangi first came to Indonesia with a sib system, and that the terms later were diffused to sibless peoples.

Dr. Kennedy does not refer in his review to Prof Lowie’s article in the American Anthropologist for 1934. Here Prof Lowie deals in part with the question of kinship diffusion into Indonesia. Lowie grants, providing of course that philologists approve my linguistic tables, that there is a connection between the social traits of India, Indonesia, and even of Fiji and New Caledonia. But Prof Lowie does not admit that all social traits, such as avoidance rules, totemism, exogamy, and cross-cousin marriage, need have sprung up in a single spot and then have travelled together. Thus the presence of clans in both India and Indonesia may independently have given rise to totemism in the two regions.

In conclusion I might suggest that there remains a lot of work to be done in tracing the distribution of kinship terms and customs in India, Farther India, and Indonesia. This historical method, while less exciting than that of thinking up “origins” for social customs, in the end usually proves the more satisfactory.

EDWIN M. LOEB

BERKELEY, CALIFORNIA

REVIEW OF PEARCE AND JACKSON: A CORRECTION

To the Editor:

In the recent review of Pearce and Jackson’s “A Prehistoric Rock Shelter in Val Verde County, Texas” (this journal, Vol. 37, pp. 676–78, 1935) the writer made certain statements which have been interpreted as insinuating that the senior author had exploited one of his students and had used, without credit, much of that student’s work. Such an implication was not intended and the misunderstanding probably has arisen from the fact that it was necessary to reduce considerably the review as originally written and in the process an explanatory item was omitted. Reference is made to the mention of the use of Thomas’ thesis. It was intended to include in the review this statement: “The method followed in preparing the report was: where Thomas’ interpretations were accepted unmodified they were incorpo-

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rated as quotations; where Pearce's and Jackson's views conflicted Pearce's were adopted, except in a few cases where Jackson's were retained with annotations indicating Pearce's views." Had this not been deleted in the final draft there would have been no cause for the thought that proper credit was not given in the report.

F R A N K  H.  H.  R O B E R T S,  J R.

B U R E A U  O F  A M E R I C A N  E T H N O L O G Y
W A S H I N G T O N,  D.  C.
NOTES AND NEWS

ANNUAL MEETING OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION

At the December, 1934, meeting of the American Anthropological Association it was voted to meet with the American Association for the Advancement of Science in Washington, 1936. By letter of January 22, 1936, the latter association informed the American Anthropological Association that its December, 1936, meeting had been transferred from Washington to Atlantic City. An informal canvass of groups of American Anthropological Association members seemed to show that there is a very general feeling unfavorable to Atlantic City as the meeting place in 1936. After various alternative plans had been discussed, the matter was submitted to the Executive Committee which decided that the American Anthropological Association should meet in Washington separately and apart from the American Association for the Advancement of Science in December, 1936. The dates set for the meeting are December 27th–29th, with headquarters at the Carlton Hotel, 16th and K Sts., N. W., Washington, D. C.

The following committees have been appointed:


It was voted that the Program Committee be instructed to issue a call early in October for papers designed for the annual meeting, the titles and full papers to be in the Committee's hands by November 1st, and that the Committee be authorized to select a limited number of papers and addresses to be presented at the joint sessions of the annual meetings of the societies involved . . . [from the Minutes of the Andover meeting, December, 1935].

Nominating Committee: C. Wissler, chairman, C. E. Guthe, J. A. Mason.

Within three months of his election the President shall appoint a Nominating Committee . . . and transmit the names . . . to the Editor who shall publish the names . . ., with an invitation for suggestions; after considering such suggestions the Nominating Committee shall report its slate to the Council which shall pass on the recommendations, with such changes as are deemed advisable, to the annual meeting [from the Minutes of the Pittsburgh meeting, December, 1934].

JOHN M. COOPER, Secretary

ETHNOS

"Ethnos," a bimonthly journal of general ethnography and archaeology, has been issued by the Ethnographical Museum of Sweden, Stockholm (Statens Etnografiska Museum) in collaboration with Thule, Ltd., Publishers (Bokförlags Aktiebolaget Thule).

"Ethnos" will devote itself primarily to extra-European ethnology and archaeology. As regards subjects relating to Europe, as a rule the Editors will accept for publication only such as deal with Eurasian phenomena and problems. Attention will also be paid to general questions of principle within the sphere of ethnology.
The Journal is intended to be an organ of the Ethnological Museum of Sweden and to be published for those Swedish anthropologists (in the widest sense of the word) who are working in fields of research outside Europe. At the same time, however, the intention is . . . that it shall be international in scope, and the publishers will therefore gladly welcome as collaborators any foreign research students irrespective of the anthropological "school" to which they belong [from Ethnos, Vol. 1, No. 1].

The editors are Gerhard Lindblom and Sigvald Linné. Six 16 page numbers will be issued annually. Subscription: 6 Swedish Kr. per annum.

The Library of the late Dr Walter Hough is offered for sale, as a whole or in part. Inquiries should be addressed to Mrs Myrtle Z. Hough, 1332 Farragut Street, N.W., Washington, D. C.

We Deeply Regret to Announce the Death of Dr Thomas T. Waterman, formerly of the University of California and at the time of his death Territorial Archivist of Hawaii, at Honolulu, January 6, 1936, aged 50.
THE SOCIAL ORGANIZATION AND
CUSTOMARY LAW OF THE
NEPALESE TRIBES

INTRODUCTORY REMARKS

The anthropology of Nepal can claim a special interest by reason of the
syncretism of Tibetan, Indian, and partly also Chinese elements,
composing Nepalese culture. In particular the close intercourse with Tibet
must be borne in mind. The religious art of Lamaism in both Tibet and
Nepal is almost identical, so that in many cases it is simply impossible to
decide whether a given piece is Nepalese or Tibetan. Spiritual influences
from both Tibet and, on the other side, from India have affected Nepal,
while, in addition, we can trace Chinese influence on the decorative art and
architecture. Unfortunately the early history of this forbidden land is
veiled in legend and mystery, and it is not before the 4th century A.D.
that the country appears in the light of authentic history. Thus the latter
requires all the more assistance from anthropology if we are finally to un-
derstand the growth of Nepalese culture.

However, the anthropological study of Nepal is only beginning. Arch-


aeology is somewhat more advanced owing to the well known investigations
made by the late Professor Sylvain Lévi. The only books which provide us
with trustworthy anthropological material are Brian H. Hodgson's works,
Colonel Eden Vansittart's handbook and, more recently, the books pub-
lished by Major Northey and Captain (now Major) C. John Morris.

The ethnological jurisprudence of Nepal is also only in its beginnings.
Hodgson's "Miscellaneous Essays Relating to Indian Subjects" offer us

1 The present article is a lecture delivered at the India Institute, Oxford University, on
the 26th November, 1935.
edition, under the title "Gurkhas," prepared by Colonel B. U. Nicolay, appeared in 1915 and
was reprinted in 1918. Major Morris' handbook, "Gurkhas" (Delhi, 1933), is an entirely re-
written edition containing practically nothing of the original book.
4 London, 1880.
some interesting records of legal institutions collected by the author during his long stay in the country. His informants were educated men in the Nepalese sense of the term, and their reports deal with religious prescriptions, rules of procedure, etc., considered from the standpoint of the learned Brahman. The underlying legal rules are Brahmanic and thus Indian, and not Nepalese in the proper sense of the term, though there are a few exceptions to this. But Hodgson’s valuable books contain nothing on the customary law of the tribes in remote districts of the country where we may expect to find at least some survivals of a more primitive law. Now it is worth noting that ethnographers without a juristic training are wont, when dealing with law, to restrict themselves to a study of family law and tribal organization, or, as a jurist would call it, to constitutional law on one side and criminal law on the other. Accordingly, before 1934 we find almost nothing in the literature on civil law and its details.

In regard to my own qualifications to touch on these matters, may I explain that I was fortunate to be appointed by the Prussian Minister of Justice early in 1918 for the special purpose of collecting records on customs and tribal laws from various Oriental prisoners of war, in particular from Gurkhas. The German authorities had drafted these men to Rumania where the climate and conditions were much more suitable to their needs and health. A brief description of the Indian camp at Morile-Marculeschi in southern Rumania is contained in my article on a marriage ceremony of the Pun clan, Magar tribe, at Rigah, Nepal. The men lived there almost in freedom in houses built by themselves and surrounded by beautiful gardens on the banks of the lovely Jalomitza River. Under these favorable psychological conditions some of the more intelligent men were willingly prepared to give me much information. Thus I was able to collect seven rather detailed records on the customary law of some of the Nepalese tribes. These records of mine received confirmation fifteen years later on the appearance of Major Morris’ handbook; for we then found that the bulk of the material was to be considered as trustworthy. Thus our publications are to some extent supplementary to each other.

GENERAL OBSERVATIONS ON THE LAW OF NEPAL

The law of Nepal, i.e. the official law, is to be distinguished from the tribal law of the various tribes. If we remember the two principal elements

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5 A Marriage Ceremony of the Pun-clan (Magar) at Rigah (Nepal) (Man, Vol. 34, No. 23, 1934).
composing Nepalese culture, that is to say the Tibetan element, and, on
the other side, the Hindu element, we should expect Nepalese law to show
the characteristic features of both. The trouble is that, while we are rather
well informed on Brahmanic law, this is not the case with the tribal law as
it was before the Hindu influence overshadowed the original customary
law of those tribes which immigrated from Tibet. So we must study the
various legal institutions and then deduce what is undoubtedly of Indian
origin.

The first code of Nepal was promulgated by King Rama Sah (1606–
1633 A.D.). During the period of Maharajah Jung Bahadur (1846–1877
A.D.) another code, known as the Nepalese Ain, came into force, and is
now in use as the standard code of the country. This code is based mainly
on the Shastras. The codification has abolished some characteristic legal
institutions of the tribes, for instance blood revenge. This institution has
now entirely disappeared, as proven by the fact that not one of my inform-
ants dared to suggest that, even in cases of manslaughter or adultery, blood
revenge could be carried out. Still some of the men remembered the custom
of blood revenge very well. That does not mean, however, that all institu-
tions of the original customary law have been entirely lost.

The Newars require a few particular observations. They are supposed
to be the original inhabitants of the Nepal Valley, while some scholars
suggest that they are related to the Nairs of southern India, on the ground
that men of this tribe belonged to Nanya Deva’s army which invaded
Nepal in the 11th century A.D. This assumption is not very likely. Anyhow,
it is stated that the present Newars represent a mixed tribe derived from
Indian and Tibetan sources. They differ considerably from the purely
Tibeto-Burmese tribes; in particular their language, Newari, is somewhat
different. The geographical situation of the Nepal Valley, whose people
have always been in closer contact with India than the rest of the country,
can explain the similarity of various cultural traits of the Newars to those
of the adjacent Indian peoples. Unlike the social organization of the other
Nepalese tribes, the social structure of the Newars has not yet been satis-
factorily studied, though their religious division into Buddha-mārgas and
Shiva-mārgas (Buddhist and Hindu) is clearly established. While the
Shiva-mārgas show a social organization similar to that of the Hindus, the
Buddha-mārgas are divided into three grades. A very slight possibility

7 What is known as Nepalese art (metal work, painting, sculpture, architecture) and liter-
ature from the middle ages (approximately 1200 A.D.) up to the 19th century is entirely the
work of the Newars. Compare my forthcoming article Die kombinierten geh-Kannen und

8 Compare Morris, Gurkhas, p. 123.
exists that the Newar tribe still conserves totemistic ideas. I am thinking here of M. Waddell’s report on “Frog Worship Amongst the Nevars.”^9 In considering the customs of some of the Tibeto-Burmese tribes we have recently been led to the assumption that their organization originally also had to do with totemism. But before dealing with this point, a word must be said concerning tribal organization.

**TRIBAL ORGANIZATION**

I. The population of Nepal is divided into numerous tribes. The principal are the following: Thakur, Chetri, Gurung, Magar, Rai, Limbu, Sunwar, Damang (or Murmi or Murmi-Lama). In addition to these there exist numerous divisions not easily defined either as tribes or castes in the stricter meanings of those terms. Such are among others Damai (tailors and musicians), Kami (blacksmiths), Sunnar (goldsmiths), Kumhal (potters), Pore (sweepers), and Sarkhi (shoemakers, leather workers). These belong to a group of sixteen “menial tribes” who can do only their specific professional business, but are not warriors. Consequently men of the fighting classes are not allowed to eat in company with them.\(^{10}\) It is highly interesting to ask a Gurkha to enumerate the various “castes” (jat) representing the population of his village. He will never call them tribes but only castes; and this is a proof that, as Morris says, Hindu ideas are steadily gaining ground in Nepal. The reason is that the ruling family of the country is strictly Hindu. Consequently the caste system has already been spread all over the country including even the smallest villages in remote valleys. If you ask a native of Nepal to tell you who are the inhabitants of his village, he will give you a peculiar list wherein castes and tribes are almost inextricably mixed. Let me give an example here. A man of the Sunwar tribe gave me the following list wherein we meet with both Hindu caste names and names of Nepalese tribes, both being denoted as “castes” by the informant: Brahman, Chetri, Gurung, Magar, Limbu, Rai, Sunwar, Newar, Kami, Sunnar, Damai, Sarkhi, and Pore. The first two, of course, are well known Indian castes whereas the rest are the names of Nepalese tribes. The word “Chetri” is a corrupt form of the Sanskrit “Ksatriya” which means literally “a fighting man, one of the warrior caste.” It must be pointed out that, at least in modern times, the tribal organization is not political. The political or administrative unit is the village community, and the headman (mukhiya) of a village may belong to any group except the lowest tribes or castes.

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\(^{9}\) Indian Antiquary, Vol. 22, 1893.

\(^{10}\) See Morris, Gurkhas, p. 125 et seq.
One phenomenon is very remarkable: Morris suggests that although the caste system exists only in a very loose and ill-defined form at present and would appear to be foreign to the social organization of the tribes, it is not impossible that if the present political seclusion of the country is maintained, the caste system will tend to grow stronger in Nepal as it weakens in India. Morris calls this "a startling reversal of the conditions of a hundred years ago, when the ceremonial excesses of the 'barbarians' of Nepal were looked upon with horror by the orthodox Hindus of the plains."

The amalgamation of tribes with the Hindu caste system is to some extent facilitated by the fact that most of the tribes are actually spread all over the country, though every tribe has its home district where the bulk of its people live.

II. Every Gurka tribe is divided into a number of clans or "thars." Some of these clans are exogamous, others not. Every clan is divided into kindreds or "gotras" which are strictly exogamous. The numbers of both clans and kindreds vary considerably. Thus the Magar tribe has six clans, and the numbers of kindreds of these clans are 153, 35, 58, 70, 175, 335. The Limbus, however, have no less than sixty-two clans. Their kindreds are not as numerous as those of the Rai clans; some clans are identical with one kindred. It is to be observed that it is not always absolutely clear whether a social unit is a clan or merely a kindred. It is also most likely that a large number of kindreds have come into existence since Colonel Eden Vansittart published his handbook on Gurkhas in 1890, wherein he was the first to give detailed lists of the divisions of the Nepalese tribes. If this is correct, and if the other possibility is excluded that these numerous kindreds had merely not been noted before—and I personally am aware that they were not omitted, but did not exist at that time—this would be proof that new kindreds come into existence by the splitting off of a family from an existing kindred. It is remarkable, in this connection, to observe that in some exceptional cases we come across names of kindreds which we meet again in other tribes. Morris notes over a hundred names of social units which claim to belong to the Sunwar clan Bara Thare but are not yet satisfactorily classified. It is, of course, impossible to enter into details of clan and kindred names here, and it would require many pages to discuss all the clan-lists and the various doubtful questions arising therefrom. Much more attractive and interesting from the anthropological point of view is the problem of the underlying principle of these organizations.

III. There is a slight possibility that the tribal organization is or, better, was at remote times combined with totemism. It is well known that totem-
ism shows so many different forms all over the world that it is almost impossible to give a definition which would embrace every single form of this institution. One thing, however, is certain, and this is that totemism requires the belief in an altogether intimate relation between a class of objects, chiefly an animal-species, and man; and when speaking of group-totemism we extend and confine these relations to a group representing a unit within a human social organization. Furthermore, we know that the respect with which primitive man considers the totem animal is often manifested by food prohibitions and that, on the other hand, the taboo on the totem animal will sometimes slowly and gradually decrease. In Nepal the Hindu caste rules governing the social life, which are of growing importance, have associated with them other food prohibitions. This makes it rather difficult to trace real totemistic prohibitions. Still Mr Morris and myself independently met with food prohibitions among the Rai which have obviously nothing to do with Brahmanic prescriptions.

The following information was obtained from Nar Bahadur, a Rai of the Yampang thar:

Everybody is free to eat goat's flesh as long as his father or mother is alive. After the death of either father or mother, this is strictly forbidden.

The man explained this custom as a religious prescription, saying it was not to be found in a book but was a custom from time immemorial. Another Rai of the Lohorung clan, who lived near Darjeeling, said that after a man's death the eldest son must not touch goat's flesh during six months from the thirteenth day after the burial, on which day the house and the members of the family of the deceased are purified. At my suggestion Mr Morris was kind enough to collect some more records on the subject, partly from men of his regiment, and partly by courtesy of some friends at Darjeeling. I may point out that the results of these enquiries are based on information obtained by him from very few individuals, so that further, more detailed researches are necessary. But already the material obtained is extraordinarily interesting. First of all there exists no general prescription among the Rai that after the parents' or father's death the children must not eat goat's flesh. There is a somewhat elaborate system of restriction on the eating of goat's flesh amongst the Rai tribe. It is said that for fear of injuring the forefathers and family gods many, but not all, Rais do not touch goat's flesh at all. Especially is this the case amongst those who must from time to time do pūjā to the family gods, that is to say the senior male in each household, and thus, for the most

\[11\] The pūjā consists of sacrificing and praying.
part, the eldest son. In these cases, however, the other children are allowed, and usually do eat goat’s flesh.

The following Rai thars never touch goat’s flesh:12

1. Camlig
2. Bângdali
3. Dimmâli
4. Parâli
5. Sotâge
6. Râkhâli
7. Sôgdali
8. Bâkasili
9. Sôgpâgi (Sâgpâg)
10. Bantâwâ
11. Kulûg
12. Thulûg

The following Rai thars do eat goat’s flesh:

1. Khâllig
2. Caurâse
3. Lohorûg
4. Yâkkâ

Specific enquiries made by Major Morris’ friend among educated Rais, students of Darjeeling High School, had the following considerably varying results:

(1) Bahadur Rai—Camlig—said that no member of his family eats goat’s flesh, not even little children.

(2) Nandadhoj Rai—Thulûg—said that his parents as well as his married brothers do not eat goat’s flesh; but the rest of the family do so.

(3) Dhanraj Rai and Birdhan Rai—both Camlig—said that their parents do not eat goat’s flesh, but the remainder of the families do so. On the death of the father the eldest living son must give up eating this meat.

(4) Manidas Rai—Bantâwâ—said that in his family there is no restriction of any sort on the eating of goat’s flesh.

(5) Likewise Yuddhatur Rai—Bantâwâ—said there are no restrictions.

(6) Randhoj Rai—Camlig—said that his parents do not eat goat’s flesh but the rest of the family do so.

Curiously enough, the information obtained by Morris is that there is no restriction among the Lohorûg, while my own record is contradictory.

It must be added that if a man becomes a sâdhu (ascetic), he may then eat goat’s flesh, even though he belongs to one of the thars noted as never eating this meat. The reason given for this is, according to Morris, that once a man becomes a sâdhu he gives up all connection with his home and has no longer any need to propitiate his household gods and ancestors.

Similar food prohibitions or taboos exist among other tribes. Thus some Limbus do not touch the flesh of a certain bird. Some Tamangs do not eat

12 Transliteration according to Professor Turner in his “Nepali Dictionary” (London, 1932). The consonant “c” is to be pronounced like “ch” in “which.” A macron over a vowel denotes the “open” sound and a tilde a nasalization like that in the French word “en.”
pork. And, above all, Gurungs will not eat, nor even touch domestic pig when alive, while they have no objection to wild pig. This distinction proves that the custom has absolutely nothing to do with Islamic prescriptions.

It is impossible to draw definite conclusions from this scanty and insufficient material. Still, Major Morris tentatively suggests that it may possibly be connected with some former relic of totemism.

The Rais are more in touch with Tibet and more closely allied to the Tibetans than any of the other Nepalese tribes. Morris considers the possibility that, since the Tibetans, as Buddhists, are not supposed to touch any sort of flesh (although many do), the Rais only took to eating flesh of any sort after their immigration to Nepal; or it may be due to the fact, he says, that goats are not commonly found in Tibet, and thus to the Rais, when they first came from Tibet, the goat appeared a strange and unknown animal, and so it gradually acquired a magical or religious significance. Whether or not this theory is correct and whether these food prohibitions or taboos have really anything to do with totemism can only be decided when complete lists of the existence or non-existence of similar taboos among all Nepalese tribes are to hand. But there is an alternative possibility, which should be noted by those who seek to interpret sociological phenomena in terms of psychoanalytic theory. It was pointed out to me by J. R. de la Haule Marett that this choice of the goat as a taboo object, whose mana is sometimes intensified on the death of the parent, strongly suggests some conscious or unconscious identification between this frequently revered animal and the parent. Only the goat taboo seems to be dependent upon the death of the parents. What other parallels to this can be found in ethnographic literature?

SOME CHARACTERISTIC INSTITUTIONS OF THE NEPALESE CIVIL LAW

I. I will now turn to some typical institutions of the civil law of Nepal, starting with family law, in particular with the artificial brotherhood. This term was introduced by Professor Josef Kohler. It is often preferable to the term "blood brotherhood," since it can embrace states in which a similar psychological bond is made by a ceremony in which no blood is actually shed.

Sylvain Lévi, in his standard work on Nepal, mentions artificial brother-

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13 The advance of the Mongolian tribes down the southern slopes of the Himalaya is assumed to have taken place at a comparatively recent period. This is suggested by Professor R. L. Turner on the basis of linguistic facts. See Morris, Gurkhas, p. 37. No absolute chronology, however, can be given.

Buddhism is supposed to have been introduced in Tibet in 632 A.D.
hood very briefly, without any particulars.14 I was fortunate in obtaining rather detailed information on this institution which seems to be prevalent among the tribes of Nepal and adjacent territories.

There exists artificial brotherhood and likewise artificial sisterhood. Artificial brothers call themselves "mit," and sisters "mitni," which means "friend." One Magar translated the term "brother of virtue." When in 1933 Major Morris' book "Gurkhas" appeared, I found, on page 40, a few remarks on "mit" and "mitni" which are in full agreement with my more detailed records, so that I trust that all that I noted fifteen years ago will be found correct.

This relationship has evidently a religious basis, for the presence of a Brahman, or "bahun," is required for the initial ceremony. A man of the Pun clan told me that the day of the ceremony must be fixed by the Brahman according to the horoscopes of both partners. Inspection of the horoscope, to find out a lucky day, is quite common for all important events. Furthermore, one of my men told me that the ceremony is performed in front of what is called a "jagge." The jagge has been described by Morris and Northey in their book "The Gurkhas," while my own description of a simpler type will be found in my paper cited above.15 I wish to confine myself here to stating that, in the villages of Nepal, a jagge is a square platform, three feet in length and one foot in height, made of earth and cow-dung which will eventually be sprinkled with Ganga water and flour forming certain lines and figures, among others sun and moon. The ceremony itself consists mainly of an exchange of the contents of two men's pockets or simply of mutual gifts. Exchanging the contents of pockets is likewise in use in the Punjab and in Afghanistan, where artificial brothers are called "pogbot bhai," or "turban-brothers."

The legal effect of the mit-relationship is twofold:

(1) The artificial relationship, like adoption, naturam imitatur; thus the position of artificial brothers is like that of natural born brothers. An incest barrier between a man or woman and the family of his or her partner's family is erected. One is no longer allowed to marry one of the relatives of one's mit or mitni. Even the mit's widow cannot be married. In this connection it is remarkable that, according to one of my men, a Thapa Maga, a mit is not allowed to speak to the wife of his mit, and she has to cover her face in his presence.

15 See footnote 5.
(2) Another consequence is that the fathers of the partners consider them equally as sons. Accordingly, there exists a fictitious relationship between a man and a son of his mit. The latter will call him, that is to say his father’s mit, “mit ba,” which means “mit-father.” Correspondingly, the children of a woman who has a mitni call the latter “mit ama” (“mit-mother”).

Except for incest barrier and artificial relationship with the mit’s family, no legal obligations or rights seem to arise. Above all there is no inheritance from a man to the family of his mit.

All my men stressed the fact that the mit have to help each other in any way; and a Sunwar said that, if a man were to die and leave his family in distress, his mit would help the family in every way he could. But even this was denied by other men, and, no doubt, this is only a voluntary act of charity and not a legal obligation.

I saw two mit during the Dasahara festival who gave me a moving example of mutual assistance. One of the men, who was clad only in trousers, was dancing, standing in one place and trembling convulsively in rhythmic movements. Such a dance generally leads to a complete exhaustion of the dancer who eventually falls down and becomes unconscious. His mit stood by his side and carefully watched the movements of his brother, drying his body from time to time. At last he held the man, leaving him sufficient space to continue his dance, and thus prevented him from falling down.

Whether a mit is married or not is immaterial. Some of the men said that one can have one mit only, while others told me that one can have as many mit as one liked. A mit is not allowed to call his artificial brother by his name but he has to call him simply “mit,” which corresponds to the custom existing among natural born brothers of calling themselves “daju” (elder brother) or “bhai” (younger brother), or, as a rule, only by their number, as “jetha,” “mainla,” “sainla,” “kanchha,” etc., which means “first,” “second,” “third,” and so on.

Dhanbir, a member of the Magar tribe, Thapa clan, mentioned that a man can become the mit of a married or an unmarried woman and that he will then be considered her brother. This information is contradictory to most of the other records, but in consideration of what I learned from a Murmi Lama, there seems to be some truth in it. In the Murmi Lama tribe artificial relationship can be established between two married couples and between a single man and a married couple. The Murmi term for mit is “leng” and for mitni “leng shiya.” The effect is just the same as in the other tribes. The leng is strictly forbidden to touch his leng’s wife, and,
consequently, the exchanging of rings between the two couples, which is part of the ceremony, is performed with the assistance of a Brahman or, in Murmi, a “ghiyabring,” who will take the ring from the male partner on one side and hand it over to the woman on the other. The ceremony must take place at a “bar-tree” or a pipal-tree, and is evidently a matter of both families and not of the individuals alone. The ceremony is rather interesting, but I cannot enter into details here. An interesting parallel to this institution is the artificial relationship between married couples among the modern Quiché Indians of Santo Tomás Chichicastenango, Guatemala.

Artificial brotherhood and sisterhood are obviously widely spread among all tribes of Nepal. It is true that we find similar institutions among various Indian peoples, but it seems to me that the Nepalese artificial relationship grew up independently. The Nepalese relationship is particularly interesting for its occurrence in a country where there are not only castes but also various tribes with different languages.

I gathered my records from nine individuals of whom seven were born in Nepal. I noted the tribes and castes living in the respective villages. It was stated that there are villages where only one tribe, as, for instance, Sunwar or Magar, is represented, and other villages where various tribes are living together. For example, in Siseri, near Bhojpur, which, according to Major Morris’ map, is situated in the territory of the Rais, we find not only Rais but also Brahmans, Chetris, Magars, Newars, and low castes as Damais, Kamis, and Sarkhis. In Kalebang, Murmi Lamas live together with Brahmins, Chetris, Rais, Limbus, Gurungs, Magars, Damais, Kamis, Sarkhis, and Lepchas from Sikkim.

Now, it is remarkable that the partners of an artificial brotherhood may belong to different tribes, except only the lowest castes (sanu jat). It was stated that artificial brotherhood between Gurungs, Magars, Limbus, Rais, and Sunwars is quite common, but I learned that artificial brotherhood is also possible between Chetris, and even Brahmans, on one side and castes of middle rank (matwala) on the other. In this case the partner who belongs to the higher class (thulo jat) is, however, forbidden to have his meal with his mit; that is to say, he cannot have any food which was made in the house or by one of the relatives of his partner.

In Nepal, where clans or kindreds play an important rôle, the life of the individual is generally limited to the circle of his kindred, and likewise,

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15 See the detailed description in my “Sitte und Recht in Nepal,” p. 203 et seq.
the social life within the tribe is regularly limited to its members. In Nepal we find, as a rule, a strict tribal endogamy. Artificial brotherhood helps to extend the social instinct of the individuals beyond the range of their clan and even beyond the tribe. Kohler used to recommend the revival of artificial brotherhood as an institution of high moral value. In Nepal, the functional side of the institution consists, no doubt, more in its contributing to strengthen the social intercourse between the tribes and in this way, ultimately, the unity of the population of the country.

II. Another institution of the family law on which I wish to remark is the remarriage of widows. According to the Brahmanic law, remarriage of widows is strictly forbidden. This prescription is derived from the Dharmasutras and other sources, and is simply a consequence of both the rule that the bride has to be a virgin and the custom of marrying children. The English Act of 1856 permitted the remarriage of widows in the proper sense of the term, as well as of such girls as were married in their childhood and became, formally, widows without having ever lived in conjugal life with their husbands. On the other hand we find, in India, the well-known levirate; but it is stated that a woman who, if there were no brother-in-law, was given to one of the distant relatives, was considered as a "punarbhu" whose social standing is only a little higher than that of an immoral woman, "svairini." The position of widows in India, then, remained very low, and this is one of the well-known subjects of modernizing tendencies in India. The position of widows in Nepal was formerly quite under Brahmanic influence. In 1898, when Sylvain Lévi was in the country, Nepalese widows were free to have themselves burned with their deceased husbands. But Lévi mentions that Jang Bahadur forbade those widows who had children from having themselves burned, and that actually widows were no longer forced to commit suicide. As to remarriage of widows, however, Sylvain Lévi wrote (Vol. 1, p. 269):

Un second mariage est naturellement interdit aux veuves; la loi brahmanique est intransigeante sur ce point, mais au lieu de la condition miserable et désespérée qui les attend dans l'Inde, elles peuvent, chez les Gourkhas, contracter sans déshonneur une union irrégulière.

It is remarkable that only one of my informants, a line-boy,\(^{18}\) gave me the same information: that no remarriage is possible, but that a widow may live with another man as his concubine. All the other informants, that is

\(^{18}\) "Line-boys" are the progeny of Gurkha soldiers who are born and brought up in the regiment. But the term is now loosely used to denote any Gurkha who has been born and brought up in British India. Compare Morris' handbook, Gurkhas, p. 124.
to say, real Nepalese from the inner districts of the country, spoke of a real marriage of the widow, though they confirmed the statement that this marriage is performed without any ceremony. But this is also the case of a second, third, or further wife of a man. The ceremonies are performed only with the first wife. Thus Major Morris' statement that in Nepal a widow can remarry, but without the byāhā-ceremony, is confirmed by my informants. Major Morris observes that the practical side of this in the Indian army is, that a remarried widow, after her second husband's death, will have a pension claim. In a word, the present situation of widows in Nepal is actually that they are free to remarry and that in public opinion this is no longer considered as concubinage but real marriage. It must be added, however, that in any case the children of the deceased husband remain with the family of their father.

There exists, then, in Nepal a much greater freedom and a better legal position of widows than in any other part of the peninsula.

III. Passing to the law of property, I will confine myself to only one subject. Some of my informants gave me rather detailed reports on loans and, in connection herewith, on securities. This is remarkable, since, as a rule, we can hardly expect to find these institutions in a pure peasant population. But as a matter of fact it was stated that, even among the natives in the remote villages of the interior, direct exchange of goods hardly exists or may even be unknown, and any business transaction is performed with Nepalese money. As an exception to this rule, my Sunwar informant told me that in his district barter is quite common, and that the inhabitants used to exchange agricultural products, such as, for instance, butter for clothes, etc.

I must say a few words on sanctions of debts in ancient Indian law. In Nārada 1, 117,19 it is stated that there exist two securities: warranters and pawns. Securities must be held in pledge, whether it be an estate or a movable thing; there were no mortgages, and the actual possession of the security was required. A difference existed between a "bhogyā" where the creditor was allowed to use the security, and the "gopyā" which he was only entitled to keep in pledge without using it (Nārada I, 125). Thus there was no security without possession. Furthermore, there were warranters, or bondsmen, called "pratibhū" which means "representative," or "lag-

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19 The Nāradasmṛti, one of the principal sources of the Brahmanic law, to be dated approximately at about 500 A.D. Cf. Sacred Books of the East 31; The Institutes Nārada, by Julius Jolly (London, 1876, and Calcutta, 1885); Hindu Law and Custom, by Julius Jolly, authorized translation by Batakrishna Ghosh (Calcutta: Greater India Society, 1928), p. 44 et. seq.
naka," which means "responsible." Let us compare these classical legal institutions with my notes on Nepalese customary law. The only informant who said that the creditor could take a material object in pawn from the debtor, was Motilal, a clever man of thirty-four years, but whose information was proved as being partly based on what he had read in some books. He was certainly not naïve and thus no orthodox informant. All the other men agreed that taking anything in pawn is quite unusual in Nepal, and some emphasized that it would never be done. The Thapa Magar Dhanbir, from Dhurstung, near Tansing, gave the following detailed report.

There are, he said, four different ways of lending money.

First, if the creditor considers the debtor a reliable person, he can lend him the money without a written instrument and without a witness. He will do so for one or two months, and without taking any interest. Dhanbir added: If on the day when the debt is payable, the debtor has no money to repay, the creditor can do nothing, but, of course, he will not lend him any money again.

Second, if a poor man is in distress, he can get money from a wealthy man if he brings him a witness. The creditor will write an instrument wherein it is agreed that the money must be repaid on a certain day and that interest must be paid at a rate of from five to ten percent per annum. Furthermore, it will be added that the debtor promised to give a security to the creditor. The term for this security is "dik." As such security a field, a buffalo, other domestic animals, or any movable object can be promised, but not the house. The text runs as follows: "If I cannot repay, you shall have my field, or buffalo, and so on." The debtor, but not the witness, has to sign the instrument. If afterwards the debtor cannot repay, the creditor will not grant him a respite for payment, but he will address himself to the mukhiya (i.e., the headman of the village), who will then order the debtor to deliver the dik to the creditor. The dik, then, is no longer a security, but the creditor will now consider it his property. In a word, the dik will then become a compensation for the creditor's loss.

Third, a man who wants much money, say 400 to 500 Rupees, must bring another "witness," who is called "gawāhi." As a matter of fact, this is a warranter who will become responsible for the debt. The instrument, the term for which is "damsuk," must be signed by the debtor as well as by the gawāhi. But also in this case a dik must be promised to the creditor. Thus there is a double security in this case.

Fourth, another institution is less a security than a means for the repayment of debts. A poor fellow who cannot repay his debts can be invited by the creditor to come to his house and to work in his fields until the value
of his work reaches the amount of his debt. Such a debtor is called "bandā," and this institution is often confirmed in the literature.

The criticism of these institutions and of others with which I cannot deal here leads to the following statement. There is a typically Nepalese legal institution, consisting of the mere promise to give a security in the case of insolvency. This is quite different from ancient Indian law. On the other hand, Indian influence was evidently at work. In Nārada I, 7–9, we find a passage according to which unpaid debts persecuted the debtor after his death in his future existence. He is believed to be reborn as a slave in his creditor's house for the purpose of working off his debts, and until this happens even the religious merits of his sacrifices and prayers will be placed to the credit of the creditor.

IV. Generally speaking, the information noted by Hodgson,29 according to which "many of the decisions of the court are founded on customary laws only," is apparently still valid. A detailed analysis of the elements composing Nepalese tribal law is not yet possible. The syncretism of various elements composing Nepalese culture can also be recognized in the sphere of law. It would be difficult to decide whether the Brahmanic influence is gradually decreasing or, on the contrary, increasing in the out of the way villages of Nepal. But it seems to me that specifically Nepalese legal conceptions and institutions which show a high standard of morality (like the artificial brotherhood), or a greater freedom for the individual than under Brahmanic law (like the rules governing the legal position of widows) are becoming predominant. The most interesting point is that there are nowadays evidently very few tribal differences, so that we can speak of Nepalese law instead of Chetri-, Magar-, Sunwar-, or other group laws.

B E R L I N, G E R M A N Y

QUICHUA-SPEAKING INDIANS OF NORTHERN ECUADOR

By JOHN GILLIN

In the autumn of 1934, while in Ecuador on behalf of the Peabody Museum of Harvard University, the writer, accompanied by his wife, made a small study of the Quichua-speaking inhabitants of the Province of Imbabura, which lies just south of the northernmost territorial limits reached by the empire of the Incas. Complete anthropometric measurements and observations were obtained of 138 adult, Quichua-speaking Indian men, whose genealogies showed beyond reasonable doubt the impossibility of their being the products of crossing with either whites or negroes, and who were chosen with due regard to the principles of sampling. The data were then treated according to modern statistical methods, and the results compared statistically with other extant Quichua series, as well as with all apposite non-Quichua peoples from whom measurements have been published.1 Space does not permit the presentation of the full data and statistical treatment here, but a short summary of the observations on these people, descendants of one of the northernmost populations of the Inca empire, may be suggestive to those interested in the general problems of the region.

An examination of the history and traditions recorded by the priests accompanying the Spanish invaders, as well as archaeological and linguistic evidence, indicates that the Inca control of the Province of Imbabura had been established less than seventy years before the arrival of the Spanish conquistadors. The emperor Tupac Yupanqui began his successful campaigns against the Caras about 1452, and the Cara resistance to the Peruvian invaders was crushed by the emperor Huayna-Cápac a few years later at the battle of Yaguarcocha (Lake of Blood), some miles northeast of the present city of Ibarra. The Cara tribe, or nation, which was thus subjugated by the Inca power, had for several hundred years dominated the highlands of Ecuador from the Rio Chota southward, probably as far as the present city of Riobamba. They possessed a culture less rich than that of Peru, but far more developed than that of the jungle tribes on the eastern and western slopes, respectively, of the mountains.

There is good reason to believe that the ancestors of the Caras, or at least the ancestors of the ruling line (the Scyris), were an immigrant group which invaded the highlands from the Pacific coast in earlier times, taking up their residence, first, in Imbabura, and thence spreading their dominion southward. In the early part of the fifteenth century their capital was Quito. According to the traditional account given by Velasco,² they reached the highlands about 1000 A.D., ascending from the coast by way of the valley of the Rio Esmeraldas. Although many details of Velasco’s narrative are open to doubt, and although its veracity has been attacked by Jijón³ and others, archaeological and linguistic evidence seems to point at least to an early cultural connection between the interandine plateau of Imbabura and the coast. The principal outlines of the invasion story have been accepted by Means,⁴ González Suárez,⁵ Verneau and Rivet,⁶ and others.

Following the history of the Caras still further backward, tradition tells us that they were not indigenous to the soil of Ecuador, but that they had landed on the Pacific coast between the Bay of Charapotó and the Cape of San Francisco, where they established a village named Cara (supposedly the modern Caráques), at an early, but undetermined, date. According to the legend, these early invaders came on rafts from overseas, originally hailing from the north. Although the precise outlines of these migrations from the north are vague, the occurrence of Central American artifacts on the Ecuadorean coast and highlands and Ecuadorean objects in Central America, indicates an early connection of some sort between the two areas.⁷

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² Juan de Velasco, Historia del Reino de Quito de la América Meridional (3 vols., Quito, 1844).
⁴ P. A. Means, Ancient Civilizations of the Andes (New York, 1931).
⁵ Federico González Suárez, Historia General de la República del Ecuador (6 vols., atlas, Quito, 1890–1906).
On the basis of tradition, archaeology, and linguistics, it is possible to say that the following elements may be involved in the composition of the present population of Imbabura: (1) a very ancient aboriginal element present before the Cara invasion; (2) a coastal element, represented by the Caras, originally stemming from Central America or its general direction; (3) a Bolivian element, evidenced by late Tiahuanaco influence in the archaeological finds, although it is probable that this influence was more the result of cultural than of physical diffusion; (4) possibly some Amazonian elements, although there is little evidence to show that they were strongly represented; (5) a Peruvian or Inca element, introduced along with the Inca conquest.

The Indians live in many respects as did their ancestors under Peruvian rule. Men wear loin cloths, white homespun cotton trousers of half length, white cotton shirts, red or blue ponchos, and large, saucer-like hats of felt. Women wear hats of the same shape, wrap-around woolen skirts of blue or red, white blouses embroidered in colors, shawls, and many short strands of heavy gold beads. Most people go barefooted. Practically all of the cloth is spun and woven at home. They are grouped into communities which may include as many as a thousand individuals. The houses are spread about at intervals over the cultivated land, each house surrounded by an acre or two of irrigated fields which are cultivated with long wooden spades and wooden hoes. The houses themselves are constructed of wattle walls which are plastered with adobe and the roofs are thatched with páramo grass. Pottery and wooden vessels are still made by hand according to the old styles. At present the people are not organized in ayllus (the Inca political unit of 100), and it is doubtful that in this region they ever were. Each community handles its internal affairs, such as matters of irrigation and the communal grazing grounds, through an assembly, while the laws of the white government are administered by a teniente político or an alcalde, who is usually a literate Indian chosen by the white government with the consent of the group.

The whites have introduced the use of iron, some factory-made cloth, domestic sheep, asses and horses, the superficial aspects of the Catholic

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8 See Verneau and Rivet (op. cit.) for a discussion of the Cara language, which they believe to have been of Chibchan stock.

9 For more detailed description of present culture, see Moises Saenz, Sobre el Indio Ecuatoriano y su Incorporacion al Medio Nacional (Publicaciones de la Secretaría de Educación Publica (Mexico, 1933 pp. 30 ff.); Victor Gabriel Garces, Condiciones psíquico-sociales del Indio de la Provincia de Imbabura (Annales de la Universidad Central de Quito, Vol. 48, 1932, pp. 125–84; Vol. 49, 1932, pp. 159–74.)
religion, and their own political control. Of these, the last seems to have had the most important cultural effect, because the political and economic life of the people has on the whole been more seriously disrupted than the material culture and fundamental religious beliefs. Some of the communities are located on haciendas where the Indians live as tenant farmers, others are situated on lands, the ownership of which is divided among the individual family heads.

Eighteen measurements, 66 observations, and 13 indices were obtained for each of the 138 Indian men studied. Space does not permit the presentation of all the facts, but a few characteristic physical features may be summarized herewith. The mean stature of 156.48 centimeters places them with the range of the "short" portion of the human race. They have a mean cephalic index of 80.04. Other absolute measurements fall within the small and medium ranges of the human species. The average head is 72.44 percent as high as it is long, and 90.26 percent as high as it is broad. The length of the face is 84.00 percent of its width, and the average chin is fairly long. The nose is of medium width compared with its breadth, with a nasal index of 72.24. The shoulder breadth is 23.12 percent of the stature, and the sitting height is 52.60 percent of the total stature. The average chest is 78.58 percent as deep as it is wide. The forehead is relatively narrow, being 70.12 percent as wide as the head. The total width of the face, however, is 96.71 percent of the width of the head. Skin color is predominately red-brown.

The hair form shows considerable variation from straight: 19 percent showed straight hair, while 56 percent had light waves, and 25 percent deep waves. Other recent studies have shown a considerably greater incidence of wavy hair among the aboriginal population of the Western Hemisphere than was formerly believed to occur. The texture of the hair is predominately fine and medium. Less than seven percent of baldness of any degree was present. Scanty beards are characteristic, only 11 percent of the men having beards of even medium heaviness. One-tenth of the subjects showed some gray head hair, and 16 per cent showed some degree of grayness in the beard. Ninety-four percent of all head hair is black, and 98 percent of all beards are black. Three-fifths of the eyes are black with all the remainder dark brown. All forms of eye-fold are present, although the

internal epicanthic fold (Mongolian fold) is absent in 21 percent of the cases. Sixty-five percent, however, show some degree of eye obliquity.

The foreheads are for the most part smooth, with a hair line which tends to be low. The nasion depression is always present and in 16 percent it is marked. The nasal root and bridge tend to be of medium height, rather than low and flat. Integumental lips are usually large, although the membranous lips are small and thin. The men show little prognathism, on the average.

The majority of men bite edge-to-edge, although, in roughly 40 percent, the upper incisors project anteriorly over the lower incisors. Only about two-fifths of the men still have all their teeth, while nearly 10 percent have lost more than 17 teeth each. Almost all of the men (98.5 percent) have some caries, and 30 percent showed more than 17 cavities each. Medium or lateral body build predominates.

In an effort to throw some light on the physical affinities of the Imbabura population, a rather elaborate statistical procedure was followed. The group was broken down into two geographical sub-groups, and comparisons were made with all other published measurements of Indian groups who, in the light of the ethnographic evidence, might have contributed to the physical composition of the present Imbabura population. This necessitated the seriation and calculation of constants for all the other groups, except the Mayas reported by Steggerda. Among these apposite groups were the Machiganga and Machiyenga (the same tribe), Sipibo, Bolivian Quichua, Bolivian Aymara, Peruvian Quichua, Cayapa of Esmeraldas, and Maya of Yucatan.

Without entering into more detail, it is interesting to remark in passing that, even after all justifiable statistical refinements, the group which showed the most affinity with the Imbabura physical features was the Machiganga, a jungle tribe on the eastern slopes of the Andes in Peru. Of

11 Añgachagua, 26 subjects; Otovalo, Agato, and San Roque, 112 subjects. When compared statistically, these two groups are not parts of the same universe. Hence our Imbabura group as a whole is not homogeneous.
12 Ferris, op. cit., 1921.
14 Ibid.
15 Chervin, op. cit.
16 Chervin, op. cit.; Rouma, op. cit., 1928; op. cit., 1933.
17 Ferris, op. cit., 1921; op. cit., 1916.
the Quichua-speaking groups, the only one which showed much similarity to the Imbabura Quichuas was that reported by Chervin from Bolivia. In no case did the Imbabura group, either as a whole, or any of its subgroups, show enough traits in common with other groups to be considered parts of the same population.

Bearing in mind the possible inadequacy of samples and of methods, the examination of the physical characteristics of this one outlying population of the Inca area forces us to the following conclusions. (1) Much mixture has taken place either in Imbabura, or in each of the compared populations, or in all, to account for the lack of affinity between the several groups compared. (2) The most important foreign elements in Imbabura are associated with peoples at present living in the Amazon drainage—the Machiganga. Since these peoples live close to the mountains and there is some reason to believe that they have had access to the highlands, and vice-versa, it may be that Imbabura and Machiganga represent marginal groups of an earlier physical strain of the Inca area, later modified or pushed back, during the rise of Inca power, by other elements from the south. Our facts no more than hint at this, but it is at least interesting, in the light of the relatively small and inconsistent similarity between Imbabura and other Quichua groups. (3) The Bolivian and Peruvian Quichua groups, plus the Imbabura group, show so many mutual differences of statistical significance when compared with each other that we have no basis for believing in a “Quichua” or an “Inca” physical type, among living inhabitants of the Inca area, which is in any way correlated with the Quichua language. (4) Very few similarities exist between the Imbabura people and either the Cayapa of the Esmeraldas coastal region or the Maya of Yucatan, which indicates that whatever blood the alleged Cara invasion may have carried into the highlands has become modified and unrecognizable somewhere along the line.

20 This statement is made on the assumption that earlier physical contacts, as suggested by historical, archaeological, linguistic, and ethnographic evidence, had actually existed.

21 The writer wishes to acknowledge his debt to Professor E. A. Hooton of Harvard University for many sympathetic suggestions and much help in the field and in the laboratory.
EARLY RELATIONS BETWEEN HOPI
AND KERES  

By ELISIE CLEWS PARSONS

There have been various traditions on First Mesa about early relations with the western Keres. The Acoma were said to be Hopi who had learned to speak Keresan; the early language of the people who came to be referred to on First Mesa as Snake-Sand clan was said to be Keresan. It was believed that all these peoples and other Keresan-speaking people called Kawaika, "Laguna people," lived together at Toko'nabi, near the junction of the San Juan and Colorado Rivers; later, the "Acoma" settled near the spring A'müba, below First Mesa on the east side, and a pueblo overlooking Antelope Valley (Jeddito) was the home of the "Laguna people." The Snake-Sand people lived below First Mesa, two miles to the southwest.

On the Mesa itself the Mustard clan and their Wikwà’lobi kiva were accounted Keresan, but latter day Keresan, immigrant during the troublous times of the Spanish invasion. Probably a trickle of Keresan immigration preceded the Tewa migration at the close of the 17th century. The Tewa of Hano have kept their language, the Keres lost theirs, on a prolonged visit, it was said, to the Navaho, but the reason of the loss, whether among Navaho or Hopi, was probably because the immigrant groups were quite small. These wanderers, Tewa or Keres, introduced the Butterfly dance which is the Saint's day dance of the Rio Grande. They also introduced the Yayatú and Poboshwymkia societies, curing societies of the Keresan type which have now lapsed among the Hopi. One guess is that this migratory stream or trickle started from Santo Domingo after it was sacked and its kivas burned by Otermin in 1681. It is known that after the Great Rebellion Santo Domingo irreconcilables helped settle Laguna.

So much for historical or pseudo-historical tradition about Hopi-Keresan

1 Kayenta pottery has been found here (A. V. Kidder, An Introduction to the Study of Southwestern Archaeology, Andover, 1924, p. 72).
2 Kawaika, which is northeast of Awatobi, was sacked by the Spaniards in 1540. It was still occupied in 1582, but some time after that date and probably before 1598 it was abandoned. The latest date obtained here by tree ring method is 1495 (L. L. Hargrave, The Jeddlto Valley and the First Pueblo Towns in Arizona to be Visited by Europeans, Museum Notes, Oct. 1935, Museum of Northern Arizona).
4 Stephen, p. 1085.
5 The image of Sand altar woman in Wikwà’lobi kiva is wrapped in a charred elkskin pouch (Stephen, p. 262). The head of the image of Dawn woman is charred (Stephen, p. 966).

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relations. What kept the tradition alive, possibly even in some particulars started it? The ceremonies. The songs of the Snake-Antelope, Flute, Wüwüchim, Mamzrau, and Singers societies are said to be in Keresan, which only means, of course, that now and again a Keresan word is used. Of these words perhaps the most significant are Ka’toya, the horned or the two-headed mythical snake which is patron of the Antelope society, and chamahia, the term for the implement-weapon, the hoe-ax placed on the Hopi Antelope society altar and on the Acoma Kapina society altar. Only in Hopi opinion are these stones accounted weapons, the archaeologist classifies them as hoes. All specimens found under archaeological conditions come from the San Juan drainage. Chamahia are fetish stones, in Pueblo opinion representing warrior spirits, anthropomorphic beings of an earlier age, turned to stone. In the Hopi Antelope society songs these


5 Most of them are made of yellow hornstone, which was quarried in the neighborhood of Four Corners. A few are of black slate. In the San Juan country they are popularly referred to as “skinning knives,” but there is no justification for this identification. Although none has ever been recovered hafted, the fact that tools of mountain sheep horn of very similar shape were mounted longitudinally makes it seem likely that the stone ones were treated in the same way. The wear on the butt ends of chamahias also evidences longitudinal hafting, and the blade ends bear the unmistakable polish of use in the earth (A. V. Kidder, personal communication).

In this connection we may note the hoe dance sticks of First Mesa. They are somewhat more paddle-shaped than the chamahia, and are painted with the cloud-rain sign (Stephen, fig. 320).

6 Dr. J. O. Brew reports finding fragments of a chamahia at Awatobi.

Were the mesa towns of Antelope Valley peopled by immigrants from the San Juan drainage during the great drought of 1276–1298? Are we on the verge of establishing a northern origin for the Keres? Those ever orientating people declare that “they went to the middle south from the northwest ahead southward” (E. S. Goldfrank in American Anthropologist, Vol. 25, 1923, p. 194).

8 At Laguna ts’o’mahia, cino’hayu, T’a’-tc’aictci were beautiful stones which stood on each side of the altars. They were human beings first, but when the earth settled they became stone (F. Boas, Keresan Texts, Publications, American Ethnological Society, Vol. 8, 1928, Pt. 1, p. 39). Shinohaiye is one of the patrons of the Kashale, an initiating impersonation (E. C. Parsons, Laguna Genealogies, Anthropological Papers, American Museum of Natural History, Vol. 19, Pt. 5, 1923, p. 258 n. 2). The samahiye stone I collected at Laguna was dressed for the altar with feathers and beads. It is a rounded, conical topped stone, not a hoe (E. C. Parsons, Notes on Ceremonialism at Laguna, Anthropological Papers, American Museum of Natural History, Vol. 19, Pt. 4, 1920, p. 118, fig. 19a).
beings are invoked; in Sia prayer the warrior spirits of the six directions are addressed by the same name or names. In Santo Domingo a man who has four times served as war chief is given the courtesy term of Tsamahia.

This term or others are far from being the only links between the Snake-Antelope of the Hopi and the Snake-Kapina (Spider) societies of the western Keres. The Snake ceremony of Sia is almost a replica of the Hopi performance with the Kapina (Spider) society associated in altar rituals. The snake swallowing performance that Espejo saw at Acoma in 1582 has lapsed; today the Snake shamans make cures, but they have no altar sand painting or public ceremony. The altar of the Kapina society (extinct in 1926) was laid for the installation of the War chiefs and for the use of the Town chief who is prescriptively an Antelope clansman. The Kapina altar as set for the Town chief had a row of large crook prayer-sticks with animal figurines; as set for the War chiefs there were on the altar two feather-topped corn ear fetishes called Tsamai‘ye and Tsamahia’, Tsamai‘ye on the east and Tsamahia’ on the west, and in the middle a large stone lion. The sand painting drawn for Dr White as that of the Fire society I am strongly inclined to think is actually that of the Kapina society. It is almost a replica of the Hopi Antelope society altar painting, particularly that of the Second Mesa Shuñopovi society. The medicine drink of the Kapina chief was ground-up snake droppings in water. This medicine gave strength and the ability to dream the future. The society gave power to men going to war. The Kapina society of Laguna used red stained prayer-

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10 Stevenson, p. 130.
11 The Pueblo of Santo Domingo, New Mexico, p. 39.
12 Stevenson, p. 76 ff. The Snake society performance includes Lion sand painting, exposure of novice to the snakes, Snake bower, dance, and snake release.
15 The Acoma Indians, p. 42, pl. 1a.
16 L. A. White, Supplementary Data on Acoma, New Mexico (ms.). These are the familiar Mothers, at Acoma called, curiously enough, by a Hopi term, honani, badger. But stone chamahia are reported from Acoma (Stephen, p. 745).
17 The Acoma Indians, pp. 48, 49. Prediction, through omens, is a Zuñi-Hopi war trait.
18 Supplementary Data on Acoma, New Mexico. The extinct Kah’bena ceremony of the Tewa at Hano was derived, I believe, from the Keresan Kapina society. It was a war (also conceptually snake) ceremony, jealously guarded from Hopi neighbors. The presence of the kachina and clowns at the conclusion of the altar ceremonial is a Keresan feature (E. C. Parsons, The Ceremonial Calendar at Tewa, American Anthropologist, Vol. 28, 1926, pp. 216–18). Similarly, after the winter ceremonies of the Snake-Antelope and Flute societies a kachina dance is in order.
sticks, as do the Hopi Snake-Antelope societies and war societies in general.

Several other traits identify the Hopi Snake-Antelope societies as Keresan societies. The Antelope society chief of Shipau'lovi said to Stephen: "Spider woman is my mother; she is the mother of all." This is the position given Spider in Keresan mythology. The Bear and Lion impersonations at the Snake initiation and the impersonations of the chiefs of the Directions (Cloud chiefs) by the Antelope chiefs are characteristically Keresan and so is the dedication of infants to the societies. Again the rites of emesis and of depositing offerings in a circuit over four days are characteristically Keresan.

The Keresan society closest in function to the Hopi Flute society is the Flint society which cures for knife or war wounds, but there is nothing specific by which to identify the Flint and Flute societies. There are however a few general Keresan traits in the Flute society: twofold organization (into Blue and Drab societies); observance of ceremony in a dwelling, not in kiva; representing the sky by pendent crossed sticks which are swung around during the ceremonial; finally flute playing itself which is very characteristic of the Keresan curing society. Navaho traits are suggested, too, in the Flute ceremony: notably through Locust who is proof against arrow or lightning, through the four birds on the altar and the use of birdskins, through the four mountains of the directions.

The Flute ceremony alternates biennially with the Snake-Antelope ceremony. The Flute ceremony has been more or less camouflaged as a dramati-

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19 Notes on Ceremonialism at Laguna, p. 118. The society controlled pigments for moccasins, presumably the red and black, and Awl man was a patron spirit (Laguna Genealogies, p. 258 n. 2).
20 At Oraibi the Antelope chieftancy is in the Spider clan.
21 In the Hopi Snake myth Spider woman accompanies the youth and is very helpful, as she ever is in Pueblo folktales. In Hopi myth and ritual Spider is specifically the grandmother of the War Brothers. Antelope chief, being a war chief, may have this only in mind.
22 Compare, too, the four day snake hunt, in circuit; and compare in this connection initiation by trespass. If anyone encounters the Hopi snake hunters he must be initiated, just as at Acoma if anyone encounters a medicine man on the four night circuits made by societies during an initiation (Supplementary Data on Acoma).
23 The Emergence myth told Stephen by a Navaho at Kead's Cañon and various variants of the Hopi Emergence myth have much in common: the reed used to ascend on; ascent by Badger and by Locust; Locust shot at by men of the Directions (i.e., Clouds); presence of Spider woman; creation of Sun and Moon through blankets and two youths; quarrel and separation of men and women; flood caused by Water serpent; ground ploughed up by Mountain Sheep to form arroyos.

The grandmother and great grandmother of the Walpi Town chief, hereditarily in charge of the Flute ceremony, were married into the Navaho (Stephen, pp. 949–50, 952, 1046).
zation of the reception of the Horn clan into Walpi; it has come clear quite recently that it is really or also a dramatization of the Hopi Emergence myth. The altar and standard paraphernalia point to this interpretation and the esoteric night ritual at the sipapu of the Emergence together with the impersonation of Masauwu, the god of Death and Fire, the First Denizen of the upper world. In myth Locust goes up the sipapu, finds Masauwu and is shot by the lightning bolts of the Clouds. Locust, the flute player, is brave and revives. Locust is depicted playing a reed flute on the tiles on the Flute society altar and Locust was said at Oraibi to be medicine for mortal wounds and, I infer, for lightning shock. The Flute society of Walpi cures for lightning shock; arrow points which are shot by the Clouds may be gathered up only by the chief of the Flute society.

One of the two Walpi Flute societies was in the trusteeship of the Squash or Sorrow-making clan, which became extinct in the 'seventies or before. The Wüwüchim and Mamzrau chieftaincies were also in this clan; but they were handed to the Mustard and Snake clans, both having Keresan affiliations. The Sun youth patron of the Wüwüchim society and the clowning associated with this society indicate Keresan influence. Mamzrau is affiliated with the Wüwüchim society conceptually and ritually, but the Mamzrau basket wrapped tiponis point to western influence.

On the whole the Keresan complex on First Mesa centres around Wikwā'lobi or Watch kiva, around the Mustard, Squash, Snake-Sand-Lizard clans, and around the Snake-Antelope, Flute, Wüwüchim, and Singers societies. Beetle, Locust, Snake, Bear, and Mountain Lion are spirit helpers or medicine. Taíowa, the seducer, Chakwaina, the kachina warrior maid, Child-water woman or Sand-altar woman who is Earth woman and Mother

24 This part of the ceremony is very much like the ceremonial reception of a foreign spouse into Acoma (Supplementary Data on Acoma).
25 These Blue and Drab Flute societies may have been named from the blue and drab bows of war (E. and P. Beaglehole, Hopi of the Second Mesa, Memoirs, American Anthropological Association, No. 44, 1935, p. 19). The Drab Flute of Oraibi were said to be named from the grey or immature locust (Fred Eggan, personal communication).
26 The name is said to be derived from inverted speech, ever associated with clowning; members of the Wüwüchim society (also of the Singers society) are per se clown impersonators. Singers chief acts as clown chief.
27 They are called brother and sister; they both cure for "twisting sickness;" they sing taunting songs and drench and befoul one the other.
of Game, and Lightning, great god of war and of fertility, are the outstanding anthropomorphic spirits. Clowning is a special feature; reproduction and war, the special interests.

In conclusion I would mention the Hopi-like traits observable at present day Acoma as indicative of early relationships. The Acoma Kachina cult has been greatly influenced by Zuñi, but the whipping of the children, boys and girls, and the performance of a single annual summer rain ceremony are Hopi-like (Zuñi and the other Keres have a series of society retreats and kachina dances). The legend of how the Acoma Corn clan got Shuracha, the little fire god, into their clan is typically Hopi. The clan head meets Shuracha making his bonfires to heat up the Earth Mother into fecundity. "I am glad to receive you and welcome you," says the clan head. "I want you to be our head." So the kachina stayed with the Corn clan. That Acoma got its clan system from the Hopi is proved by the fact that the paternal aunt gives her godchild one of her clan’s stock of personal names, a naming practice peculiar to the Hopi.

Two hypotheses about Keres-Hopi relations may be advanced, from the ethnological evidence. (1) A long period—the archeologists should tell us how many centuries—before the Spanish invasion, groups from the Hopi country migrated to the territory of the western Keres, probably to Acoma. These people carried with them Snake rituals and myth, also the Hopi clanship structure. Their relationship with kindred who had remained behind was not completely severed, and through this line of communication the Snake rituals, moulded into Keresan society patterns, got back to the Hopi. During the Spanish invasion, another line of Keresan communication started up through the Rio Grande towns. Curing societies of the out and out Keresan type were introduced at this time. Through these Keresan immigrants relations with the Navaho were carried on, and some Navaho-Keresan traits introduced into ceremonial, particularly into the Flute ceremony.

(2) The second hypothesis, the Fewkes-Hargrave hypothesis, is not contradictory of the first hypothesis, it merely begins further back, with the suggestion that all the towns in the Antelope or Jeddito valley were Keresan, including Awatobi which was destroyed in 1700, traditionally by First Mesa people, possibly because the Bow people had been "washed" by a visiting friar. (In this connection note that in the Oraibi story ac-

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90 Supplementary Data on Acoma.
91 Ibid. It occurs sporadically, only in some families, in Laguna (Laguna Genealogies, p. 181).
92 My guess is that Awatobi and, before it, Sikyatki, were destroyed not by Hopi but by nomad raiders, possibly the Utes. That is why Walpi, fearing it would be its turn next, invited
counting for the attack on Awatobi the Town chief of Awatobi gives away his "children," the townspeople, by giving away two clay figurines, male and female. Such collective human representatives are kept today by the Town chiefs of San Felipe and Santo Domingo.) 33 Several Hopi ceremonies are said to have been introduced from Awatobi: 24 the women's Owakül society, 25 Singers society, the War society, and the women's Mamzrau society. 36 (In this connection note that Owakül has wooden bird effigies and cone pedestals like the Flute society; 37 that Owakül and the War society use the Keresan sky sticks; that Singers, like Wówóchítü, clown in the Keresan manner, and that Snake and Flute societies use painted stone tiles quite similar to those excavated at "a pueblo near Awatobi.") 38 To be sure Awatobi has never been referred to by Hopi as a Keresan-speaking town, which probably means that at the time it was destroyed it was speaking Hopi, whatever language was spoken by its founders.

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colonization from the East. The reasons alleged in the folk tales for the destruction of these towns by Hopi are too frivolous to be credible. To admit destruction by the Utes would have hurt tribal pride.


24 Awatobi survivors settled at Mishongnovi. Mishongnovi claimed the Koshare-like horned figurines excavated from an Awatobi shrine in 1881 (Stephen, p. 146, fig. 90) and Mishongnovi families live today near Awatobi, claiming land ownership (J. O. Brew, personal communication).


SHOSHONI POLYANDRY

By JULIAN H. STEWARD

A NOVEL perspective on polyandry is provided by its occurrence among the Shoshoni Indians of Nevada. Among the Eskimo, Thibetans, and Toda, it appears to have been correlated with an excess of males in the total population produced by female infanticide. If, however, an excess of males alone were a sufficient cause of polyandry, it should occur in societies which have a surplus of marriageable males brought about by frequent polygyny. Because there is instead in such societies rarely more than a temporary extension of the husband’s sex privileges to other men, as for example among the Wahuma of East Africa or some Australians, it must be assumed that a further condition of polyandry is the absence of factors which almost universally prevent it. That is, social, economic, and legal institutions ordinarily give the male a monopoly of matrimonial, if not strictly sex, privileges in each family. In polyandrous societies, either these factors must be weak or the pressure from unmarried men must have been sufficient to override them. The levirate seems frequently to have been the entering wedge.

Shoshoni polyandry is intelligible only if regarded as the function of a social structure which did not contain factors to exalt man’s position in the family. There was no excess of marriageable males produced either by female infanticide or by frequent polygyny, and, so far as can be determined, the ratio of men to women was not abnormal. There was merely an extraordinary simplicity of social structure which made the relationship of both sexes to plural marriage almost identical.

To the extent that marriage represented an economic union, men and women contributed about equally to the household. Woman’s gathering of piñon nuts and other wild seeds was every bit as important as man’s hunting. In fact, vegetable foods were staple and lasting, whereas hunted game was only occasional and limited in quantity, so that woman’s food contributions matched man’s contributions of buckskins and other coveted

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1 These data were obtained during field work among the Shoshoni in 1935. The work was financed by the University of California and a grant-in-aid from the Social Science Research Council.

2 Polyandry among the Nāyar of Malabar, which seems sometimes though not always to have been fraternal, existed in a society in which the status of woman was strengthened by matrilineal institutions (A. Aiyappa, Nāyar Polyandry, Man, Vol. 32, No. 99, pp. 78–79, 1932). However, fraternal polyandry was common among the Dravidian tribes of southern India, where inheritance is patrilineal (L. K. Ananthakrishna Iyer, Nāyar Polyandry, Man, Vol. 32, No. 320, pp. 269–70, 1932).
articles. Economic life, moreover, provided no property rights which gave either sex an advantage.

Social structure was so simple that neither sex could achieve superior status through affiliation with groups outside the family. Shoshoni population was, because of the low population density and the great distance necessary to forage for food, thinly spread over a vast territory and clustered in small communities of two to fifteen families each. During large portions of the year, families travelled alone in their quest for food. Social intercourse with persons outside the community was rare, large gatherings were seldom possible, and any real band organization was unknown. Uncomplicated by clans, societies, age classes, or other groupings, membership in which could give leverage to elevate one's status, an individual's social behavior was regulated almost exclusively by the family, in which the sexes were equal. The very slight authority of the village headman gave advantage to one man only.

Polyandry was, however, usually fraternal. This must be understood as a function of the family contract nature of marriage, a fact underlined by the prevalence of the sororate, levirate, and preferred mating. Marriage with one's cross-cousin (father's sister's daughter) or, in some localities, pseudo-cross-cousin (father's sister's step-daughter) was preferred. When two families were already connected through marriage, a sister or brother of one spouse preferably married a brother or sister of the other, or two brothers married two sisters. In such instances, the two couples ordinarily dwelled, worked, and travelled together.

The sororate required not only that a widower marry his deceased wife's sister, but that a second wife taken while the first wife lived be a sister of the first. Likewise, the levirate required not only that a widow marry her deceased husband's brother (younger or older), but that a second husband taken while the first husband lived be a brother of the first. It is extraordinary that fraternal polyandry has not frequently accompanied the levirate. One suspects that it may actually have been more common than is supposed.

A further logical corollary of these customs would be a kind of restricted group marriage, two brothers sharing two wives as among the Pawnee and Toda. This, however, seems to have been unknown to the Shoshoni.

Although the geographical limits of polyandry are not known, it seems  

3 Lesser has recorded a very similar fraternal polyandry among the Pawnee, Wichita, Kitsai, Arikara, and Comanche. The last is of particular interest, because the Comanche language is almost identical with Shoshoni. See Alexander Lesser, Levirate and Fraternal Polyandry among the Pawnees (Man, Vol. 30, No. 77, pp. 98–101, 1930).
to have centered in central and eastern Nevada and to have reached the Shoshoni of southern Idaho. It is reasonably certain that it was not practiced by the Shoshoni of southern Nevada nor by the adjoining Northern Paiute. An instance, however, was reported from the Southern Paiute of Panaca, Nevada.

Case histories were difficult to procure because of the present disintegration of aboriginal Shoshoni culture, but the following sketches throw some light on it.

In Pine Creek Valley and Diamond Valley, south of Elko, Nevada, plural husbands were always brothers, never exceeded two, and the younger did not anticipate marriage with another woman at a future time. Both men were called úpú⁴⁷, "father," by the children. To have called either man hai', "father's brother," would have implied that he had no sex privileges with the mother. The terms for brother-in-law and sister-in-law show the influence of polyandry as well as polygyny. The Diamond Valley informant remembered two polyandrous women, one being his own mother, the other a woman near Hamilton. He thought of one of his mother's husbands, a local headman of some importance, as being his real father, but could offer no explanation of his belief. He was convinced that in all polyandrous marriages, one husband was always away hunting. Similar polyandry was reported in Steptoe Valley. A Eureka informant thought that in a comparatively recent instance of polyandry, the husbands were not related. The woman has left both polyandrous husbands and is now married to another man.

In Spring Valley, east of Steptoe Valley, where Shoshoni territory adjoined that of the Gosiute, it was claimed that although polyandry might be fraternal, cousins sometimes shared a wife. This was the only locality in which it was thought that a woman might have three husbands. Here, too, men were all called "father" by their children.

Little Smoky Valley to the west had an attenuated polyandry. The informant had observed one instance, and thought there had been others, in which a man, after marrying, brought his younger brother to live with himself and wife. The woman cooked for both men, but the younger slept with her only when the other was away. Such an arrangement was admittedly temporary, the younger brother anticipating true marriage with some other woman at a future date. The children, moreover, called the younger brother by the term for father's brother, hai'.

In all localities except the last, polyandrous marriage was contracted with intentions of permanency. Both men seem to have enjoyed the same
privileges, both were called "father," and biological paternity was a matter of indifference.

Two instances were reported from neighboring Shoshonean groups. A Snake River Shoshoni recounted an instance of a Bannock woman in Idaho who married two brothers, the three habitually sleeping together. The marriage eventually broke up, each man marrying a different woman. A Shoshoni woman at Fort Hall, Idaho, similarly married two unrelated men and subsequently separated from them.

An Ely Shoshoni described an instance of polyandry among the Southern Paiute of Panaca, Nevada. A woman married two brothers, one or the other of whom was away most of the time. When both men were home at the same time, one of them slept in the wagon.

It is impossible to state the relative frequency of polyandry or to obtain a real picture of its place in native life. It seems not to have been uncommon, a fact attested by its influence upon brother- and sister-in-law terms, and it certainly carried no social stigma. In discussing it, the modern Shoshoni who, of course, is now duly impressed with the importance of monogamy, invariably laughed heartily and speculated about how it could have succeeded. It was the common conviction that one husband had somehow always managed to be away hunting while the other was at home. To the extent to which this was true, polyandry was a rapid alternation of spouses or a substitution by one man or brother for the other who was away rather than a simultaneous enjoyment of matrimonial privileges. The Pawnee usage seems to have been somewhat similar.

These data seem to indicate that Shoshoni society was among the very few in the world in which the relative lack of female jealousy or importance, which makes polygyny so commonly possible, was matched by a comparable weakness of jealousy and importance in the male. Obviously, there must have been a tenuous development of that monopolistic feeling, that will to dominate, in the man, which, whatever be its true nature or explanation, in most cultures does not tolerate a rival in the home.

BUREAU OF AMERICAN ETHNOLOGY
WASHINGTON, D. C.
J. B. TRUDEAU'S REMARKS ON THE INDIANS OF THE UPPER MISSOURI, 1794–95

By G. Hubert Smith

Early documents of ethnographic significance for the Indians of the upper Missouri River are particularly to be desired, inasmuch as our knowledge of these tribes scarcely extends further than to the time of the Lewis and Clark expedition. It is, however, well known that there were persons living among these tribes much earlier, and descriptive material of a somewhat earlier date than that usually referred to is available in at least one source. This consists of certain notes which were made by Jean Baptiste Trudeau of the Spanish Commercial Company of St. Louis. Parts of Trudeau's journals of his trading expeditions for the company during 1794 and 1795 are available and have been carefully edited.1 He had, however, prepared another manuscript, portions of which were translated and published in 1808 in the "Medical Repository," edited by Samuel L. Mitchill.2 In that place is to be found the following, which is here reprinted because of the apparent neglect of this source, and because the original is not known to be extant.

From a statement made elsewhere in the "Repository,"3 it is evident that these notes were part of a longer manuscript put into Dr Mitchill's hands in Washington in the winter of 1805–06 by Nicholas Boilvin, subsequently Indian agent at Prairie du Chien. Although a translation of the whole work was not published, note was fortunately made of its contents.

1. A description of the Upper Missouri; 2. A sequel to that description; 3. The opinions of the Indians as to their origin, faith, and ceremonies in religious matters; 4. Mode of making peace, smoking the great pipe, and dancing in different ways, such as the calumet-dance, the sundance, and the bull-dance; 5. Conduct of the sexes toward each other, and

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3 See Trudeau's Description of the Upper Missouri (Medical Repository, 2nd Hexade, Vol. 3, pp. 313–15, November, 1805–January, 1806). This article contains extracts, with brief editorial comment, from the same source as that provided by Abel (op. cit.).
marriages; 6. Their wars." The translation was probably by Mitchill, who is given credit in the table of contents of the bound volume of the "Repository." Finally, it should be stated that the present material, from evidence in Trudeau's journals, probably relates to the Arikara, except where specifically stated otherwise.

Only the concluding portion of the account as published in the "Repository—the more significant material—is here reprinted.

Remarks on the MANNERS of the INDIANS living high up the Missouri: Translated from a manuscript of JEAN BAPTISTE TRUDEAU, put into the possession of Dr Mitchill, by Mr NICHOLAS BOILVIN.

... They have a singular kind of polygamy among them. If a man takes a woman to wife who has several younger sisters, it is common for him to marry them all in succession, as fast as they become old enough. I have seen several who had as many as six wives, and all these sisters.

A young Indian seldom however lives long with his first wife. This is so much the case, that by the time he is thirty years old, he has perhaps cohabited with ten different women, and abandoned them. After that age, they usually grow more permanent in their attachments. The men generally are allowed the liberty of divorcing their wives when they please, and of marrying again. The women have not the freedom of doing this until after they have been deserted by their first husband. Then they have full range and power to act as they please. Accordingly these women take new husbands as often as their curiosity or convenience prompt them. After a woman becomes more advanced in life, she attaches herself to some one man, and he is commonly that one by whom she has had the greatest number of children. If a man quits a woman by whom he has had a number of children, he only takes away his arms with him; but the horses and other things remain with the wife.

When a young woman has lost her husband by war or otherwise, and there are surviving brothers of the husband, one of them marries the widow, or rather has the right to do so. It must be observed, however, that this takes place only among the savages who value themselves most highly in keeping up an observance of their ancient customs. The Indians to whom we relate the circumstances of our marriages, are wholly at a loss to comprehend how white men, possessing so much understanding and knowledge, should be so blind as not to see that marriage is a source of pain and torment to them; they look upon it as a monstrous thing for a man and woman to be so indissolubly bound together as never to get loose. In short, talk to them as we please, they remain unalterably persuaded that white
men are the slaves of women. There are few Indian females who are constant and faithful to their husbands, but are much given to intrigue and incontinence. This is however not equally the case with all the nations; for among some of them, the women are more reserved and chaste.

The Panis, Mandanes, Ricaras and Bigbellies, are somewhat more than ordinarily indifferent as to their women.—No such sentiment as jealousy ever enters their breasts. They give this reason for it, that when a man dies he cannot carry women with him to the regions of the dead; and that they who quarrel, fight, and kill each other about the possession of a woman, are fools or mad-men. They are so firmly convinced of this, that many of them take a pride in treating some of the considerable men among them with their youngest and handsomest women. So true is this, that husbands, fathers and brothers, are importunate with the white men who visit them, to make free with their wives, daughters and sisters, particularly those who are most youthful and pretty; and in consideration thereof accept a few baubles or toys. Indeed both the girls and married women are so loose in their conduct, that they seem to be a sort of common stock; and are so easy and accessible that there are few among them whose favours cannot be bought with a little vermilion or blue ribbon. This kind of commerce is carried on to a great length by our young Canadian traders. The consequence of these libertine manners is the venereal disease. This is very frequent among them; but the Indians cure it by decoctions of certain roots. I have seen some that were rotten with it, cured in six months.

When menstruation happens, the woman goes out of the hut, makes a fire by herself, and cooks her food alone. No person takes any of her fire on any account, not even to light a pipe with, for fear of bringing some misfortune upon himself. Their War-mats and Physick-bags are at these times carried out of the house and suspended from the end of a pole in the open air, until the operation is over. While the women are in this situation they are very careful not to enter any cabin where there is a sick or wounded person; lest the patient’s recovery should be retarded. When a woman finds herself with child, she receives the embraces of her husband no longer, but abstains from them entirely until thirty days after her delivery. There are, as may be supposed, exceptions to this: but such women are considered as behaving foolishly, and endangering both the lives of mother and child. When a pregnant woman finds her labour approaching, she withdraws to the hut built for the lying-in business, in all places where they have a stationary settlement: some old women follow her and give her all the help they can. But parturition is effected without the aid of a midwife; for they bring forth their young ones with a facility of which our civilized ladies
have little idea. The term of their confinement seldom lasts more than two
days. And if the party should find it necessary to march, the woman’s
lying-in never detains them half a day. For the mother, as soon as the child
is born and swaddled, travels with the assistance of some of her friends,
the whole day’s journey to the place of encampment. And the next day
after the infant is brought into the world, she plunges and washes it in
water, both winter and summer. She then wraps it in a piece of Bison-
skin and ties its back to a plank about three feet long. The women nurse
their children themselves, and as they never wean them, they suck as long
as they please.

MINNEAPOLIS, MINNESOTA
INDIAN MASKS FROM THE LOWER YUKON

By FREDERICA DE LAGUNA

The archaeological and geological expedition to the middle and lower Yukon River, Alaska, sponsored by the University of Pennsylvania Museum, was fortunate in securing a group of wooden dance masks, made by the so-called Ingalik group of the Tena (middle and lower Yukon River Athabaskans). These masks were obtained at the village of Hologochaket ("Holikachuk") on the Innoko River, a mile and a half below the junction of the Shageluk Slough. It is roughly twenty miles northeast of Anvik. These masks were in use until about two years ago, when the cache in which they were kept collapsed. We do not know whether they were discarded because they were damaged by the fall, or because this accident was supposed to have robbed them of their potency, but in any case they were thrown into a rubbish pit, and new masks were made to replace them. An Indian at the village, Simon Holikachuk, helped us to gather up these masks, and gave us permission to study and photograph the new set in the new cache. All of the old masks were supposed to have been made by one man, Sunday, and the new ones resemble them so closely that they may also have been carved by the same person.

Three villages in this region are accustomed to entertain each other at potlatches and masked dances. These are Hologochaket, Anvik on the Yukon, and Shageluk on the Innoko below Hologochaket and at about the same latitude as Anvik. These celebrations are generally held in winter. According to Dr John W. Chapman, who has published some notes on Tena ceremonials,1 they are not encountered on the Innoko or Yukon above these villages, and from this fact, as well as from internal evidence, he concludes that the ceremonies are copied from the Eskimo. He recognizes four types of festivals (named in order of their solemnity): the Death Potlatch, given by any one who had lost a relative during the year whom he wished to honor; the Feast of the Dolls (similar to that described by Nelson among the Eskimo); the Feast of the Animals' Souls; and the Feast of Masks. In 1905 the Dolls and Masks ceremonies were no longer held at Anvik, though the latter was still given at Shageluk. To this day Shageluk seems to cling more closely to the old festivals than do the other two villages. The ceremony of the Animals' Souls seems to be still kept up in these

1 Notes on the Tinneh Tribe of Anvik, Alaska (Congrès International des Améri- canistes, 15th Session, 1906, Quebec, 1907), Vol. 2, esp. pp. 14–38. Quotations and references to a masked dance witnessed at Anvik by Chapman in 1905 are taken from this article.
villages, or at least if abandoned, has only become so at a very recent date.

Of this ceremony Chapman writes:

The purpose of the feast was a thanksgiving for abundance of fish and game, with the intention of securing a further supply, by showing gratitude to the thinking spirits of the animals. The purpose of the festival is achieved primarily by making masks representing the various spirits, and figures of the animals which correspond to them, attached to the masks, and by composing songs in their honor, which are sung by dancers wearing the masks; also by the exhibition of certain insignia which go with the masks which are essential to the feast. Other masks not essential are worn by dancers who entertain the company in separate acts. The secondary purpose of the festival—that of entertainment—is quite as important in the minds of the people as the first reason.

It is customary for one village to entertain another at this dance. The guests are summoned by two messengers wearing small forehead masks, peculiar to their office. On the particular occasion described by Chapman, it was impossible to invite outsiders and the villagers divided themselves into two bodies, one to be entertained by the other. Of the seven dances given, the first, an animal dance, in this case an Otter Dance, and the third, the Silver Salmon and Gull Dance, were essential.

Our informants, however, did not distinguish between the several types of dances, but spoke as if all celebrations in the kashim were held in honor of the recently deceased. A lamp is lit for each person so honored. These lamps were formerly small clay saucers, and were set on wooden stands about a yard in front of the bench that runs around the wall of the kashim. There are holes for these stands in the wooden floor of the kashim at Holo-gochakhet. A native of this village told us that he has seen as many as seven lamps burning at the same time. Chapman reports that at the Animals' Souls feast there were two lamps on each side of the room, with a row of candles between, serving as footlights. At the ceremonies the men sat on the high bench, the women sat under it, and part of the space under it was reserved for the dancers between performances. At the Death Potlatch the relatives of the deceased give presents, chiefly clothing, to every one, but especially to those who assisted at the interment of the dead. These include those who washed and dressed the corpse in new clothing, and those who

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3 Chapman's remarks on the observance of this ceremony are self-contradictory. On page 16 he says that neither it nor the Feast of Masks was given at Anvik at the time of his writing; yet the ceremony which he witnessed there in 1905 and describes under the title of Feast of Masks, is certainly the ceremony of the Animals' Souls, as is evidenced by the description here quoted.
dug the grave and erected the grave house. Every guest is expected to make a suitable return when it comes his turn to potlatch, and pride does not force the givers to pauperize themselves, as on the Northwest Coast. It is understood that the souls of the dead enjoy in some way the food and clothing given the guests. Some days before the potlatch is held a messenger travels to the neighboring villages to invite the guests, wearing on his forehead a miniature mask. Such a mask was seen at Hologochaket. It represents a woman’s face with a sorrowful expression. The hair is black and is parted in the middle. The face is white except for two bright red spots on the cheeks, and a red line around the edge of the mask. The eyebrows are black; the slanting eyes, almost closed (as if in grief), are outlined in black ink, and these outlines are connected over the bridge of the nose, giving the effect of a pair of spectacles. The downcast mouth is outlined in ink, and three ink lines on the chin represent the traditional tattooing, which can still be seen on the older Indian women in the Nulato-Holy Cross region.

Both men and women take part in the dancing. We had no opportunity to witness a potlatch, and the only information we received about the style of dancing was that the women hold their arms slacker and not so stiff as the men. The singing and dancing are accompanied by one or more large drums of the Eskimo tambourine type. Three drum frames were hanging from the cache at Hologochaket. These are hoops of wood, grooved on the outside for the lashing which secures the gutskin head. The handle is a short projecting piece of wood.

It is not clear if the “Potlatch Dance and Feast” described by Chapman is the same as the Death Potlatch. For the ceremony recorded by Chapman, two messengers were sent to another village, carrying invitation sticks, to invite the villagers to the potlatch. One messenger returned home with their acceptance, while the other accompanied the guests until they had almost reached the hosts’ village, when he ran on ahead to announce their arrival. This feast seems to have been essentially an occasion for trade. Certain traditional gifts were asked of the guests, and they brought other objects to trade with their hosts. The latter, however, would attempt to give away more than they had received. Each man traded with his inherited “partner.” The trading was accompanied by songs and dances, but the performers were not masked.

Both the old and the new masks from Hologochaket are clearly in the same style. Those collected by Chapman at Anvik in 1905 resemble them, and yet seem to be closer to the Eskimo masks figured by Nelson. We reproduce some of Chapman’s illustrations for comparison. A further com-
parison of interest will be made possible when the masks which were recently collected at Anvik by Dr Krieger of the U. S. National Museum and Dr Osgood of Yale are published. In general, these Indian masks are mechanically simpler than those of the Eskimo, which so often have hands, feet, or various oddments attached to the edge. They also appear less well carved than the Northwest Coast masks, which often achieve such realism as to suggest individual caricatures.

The new masks at Hologochaket do not duplicate the old; in each set are a number which are not present in the other. In both, however, we can distinguish two main groups; human beings, and animals or animal "owners." Our comparison of the two sets cannot be as complete as I should wish since we did not have time to take full notes on the new collection, and failed to note in detail the methods of suspension, except that all had a string or cord to fit around the back of the head. It is likely that the larger masks, like those in the old set, also had a mouth piece to be gripped by the teeth. Unless otherwise stated, the eyes of all the masks are cut through, though they are so small that the wearer had only a limited field of vision. The paint on all the Hologochaket masks is common commercial house paint. On some of the old masks green is used instead of black, probably because the supply of black paint was limited. The human masks will be described first.

In the new set there are at least six masks representing "Up-River Indians," that is, the Koyukuk Indians above Nulato, who formerly were the enemies of the Ingalik who lived from Nulato to Holy Cross. Two masks (pl. 17: A, 3; C, 2) represent "Up-River" men, nitsutq; others (pl. 17: B, 1) an "Up-River" young man, and (pl. 18: A, 4; A, 6; B, 7) "Up-River" women. These characters all danced together. The faces of the masks are painted white; the hair, eyebrows, and beards are black. All of them have, or had, a single feather rising from the middle of the forehead. This feather is shaved to the midrib for most of its length, and the tip is furnished with a tuft of down. The eyes are outlined in black, and as on almost every Hologochaket mask, animal and human, the outlines are connected, spectacle-fashion across the bridge of the nose.2 The mouth is also outlined in black. One of the men (pl. 17: A, 3) is bald. The young man (pl. 17: B, 1) has a very long head; the hair, on each side of the wide central part which continues the line of the nose, is red. The women have three black lines (tattooing) below the mouth. Two women have red spots on the cheeks (pl. 17: A, 6; B, 7); the third has a beard.

2 Three of the masks published by Chapman have these spectacles. On the Otter mask they are supposed to represent natural markings.
New masks at Hologochaket. A (Upper left): 1, Feather circlet; 2, Lush; 3, Up-River Man; 4, Up-River Woman; 5, Bird preceding Salmon Woman; 6, Up-River Woman; 7, Arctic Owl; 8, Feather wand. B (Upper right): 1, Up-River Young Man; 2, Indian; 3, "Outside Siwash;" 4, Caribou figure; 5, Old Man; 6, Crane; 7, Up-River Woman. C (Lower left): 1, Up-River (?) Man; 2, Up-River Man; 3, Raven; 4, Dog Salmon Woman; 5, Berry Woman; 6, 7, Bird preceding Salmon Woman. D (Lower right): 1, "Fish cap" with stuffed loon; 2, Stuffed loon; 3, Stuffed rabbit "in a snare;" 4, Bear (or Moose Man). (In each photograph read from left to right and from above down.)
Old masks from Hologochaket. Upper row (from left to right): 1, "Boss" of Up-River Indians; 2, 3, Up-River Man; 4, Man; 5, Russian Captain. Lower row: 6, Buffoon's mask—Beluga or Russian Man; 7, Buffoon's mask—Man; 8, Berry Woman; 9, Dog Salmon Woman (willow and feather borders of Nos. 8 and 9 are missing). (Centimeter scale.)
The old collection contains three “Up-River” Indians, the “boss” (pl. 18: 1) and two men (pl. 18: 2, 3). All are made slightly askew, and like the new set are painted white, and were originally furnished with the feather tufts. They were attached to the head with a cord. The “boss” has green hair, parted on the side, green eyebrows, and green lines about the eyes and mouth. One of the men has black hair parted in the middle, a black beard, black brows, and black outlines around the eyes and mouth (pl. 18: 2). The other mask is finished in green paint. Instead of hair, there is a pointed green cap, and the chin is beardless. A single line runs down the middle of the chin. Though identified as a man, this mask may represent a woman (pl. 18: 3).

In the new collection there are several other masks which are said to represent natives but about which no further information was obtained. One of these is a man (pl. 17: B, 2), with red ears, carved out of separate pieces of wood. The head is bald, except for two tufts of red down above the eyes; the beard, eyebrows, and outlines are green. The face is white except for three faint red streaks on the forehead. A grotesque mask (pl. 17: B, 5), with mouth twisted to the left, is an old man. The hair, eyebrows, and mustache are black. The mouth is outlined in red, and instead of being cut through, as it is in all the other masks, has three small holes bored through between the lips. These may possibly have held small objects. The usual feather tuft is fastened to the middle of the forehead. There is a third man (pl. 17: C, 1), who may belong to the “Up-River” group. He is bald; the eyebrows, beard, and lines about the mouth and eyes are black. The mouth is smiling. The feather tuft is missing from the forehead.

A very amusing mask in the new set represents an “Outside Siwash” (pl. 17: B, 3), that is, a Plains Indian, as the Yukon Indians have pictured him from the white men’s descriptions. The face is pink, with red spots on the cheeks, and red lips. The hair is real, and is separated in two braids, the ends of which are wound with red cloth. The traditional feather headdress is made of wrapping paper; the “feathers” are striped with alternate lines of red and black. The most curious feature, however, are the three black lines below the chin, showing that the “Outside Indians” were tattooed like the lower Yukon women!

In the older set there are also a number of other human masks. One of these (pl. 18: 4) was not identified. The nose is blue, and there are red patches on the chin and about the small crescent-shaped eyes. One of these is much larger than its mate. The face was probably once white. There was formerly a string for suspension, but the feather on the forehead seems to have been lacking. A rather amusing mask (pl. 18: 5) was identified by one informant
Fig. 1. Masks and staff from Anvik (after Chapman, pp. 22, 28, 30, 31). 1, First Cannibal Woman’s Son (length 13½”; drab and white; squirrel skin in mouth. By Chapman wrongly titled “Messenger Mask,” but cf. text); 2, Second Cannibal Woman’s Son (length 30”; forehead drab, face white, red around mouth); 3, The Old Man in the bad dream; 4, Staff carried by Cannibal Woman’s Son; 5, The Dreamer; 6, The Old Woman.
as a "Russian Captain." Another said that it was simply an old man, ṅnafix. The first identification seems to be correct, to judge from the character of the mask itself. The bulging head has red hair that runs down to the eyebrows. There is a large black beard, formerly covered with bear fur. The eyebrows and mustache were probably of bear fur also, as a number of small nails suggest. There was a feather on top of the head, a string for suspension, and in addition, a wooden cross piece inside the mask, just below the level of the carved mouth, to be gripped between the teeth of the wearer.

In the last two numbers of the mask dance described by Chapman the performers wore human masks. In the first of these, a male dancer caricatures an old woman (fig. 1: 6); in the second, a man (fig. 1: 5) falls asleep and is bothered by a bad dream in the shape of a troublesome old man (fig. 1: 3).

In the old set there are two masks which were worn by buffoons, before or between the dances. Such a mask is called ṭ=k̓im̓í, "have fun in it." One of these (pl. 18:7) has a lopsided face, with protruding lips and high projections on the cheeks, criss-crossed with cuts. The other (pl. 18: 6) was said to represent a "Roosian man," gacalt'án ("white man"). Another informant, however, said that it was a beluga, carved by Sunday to celebrate his killing of such an animal. Belugas occasionally ascend the Yukon as far as Nulato or Koyukuk. The mask shows a grotesque human face with a beard. It is larger than any of the other human masks. The nose is broad, the mouth lopsided. Both of these masks are so badly weathered that no trace of paint remains. They seem to have lacked feathers and had no means of suspension besides the string.

A pair of masks in the old collection (pl. 20: 3, 4), not found in the new, represent the mythical Half-Man, ṃnáq'ωθ, literally "Half-Face." He was a being with half a head and half a body, and only one arm and one leg. In spite of this handicap he was supposed to be the greatest hunter and the richest man. He held his bow with his foot when shooting. The two masks are looking-glass copies of each other. The two dancers performed together, one on each side of the kashim. The step is said to have been like a one-step. The masks are both white, with a single small eye, and no mouth. The left-hand mask has a blue line around the eye and a trace of black paint on the chin (to represent a tattooed line ?). This may be a Half-Woman, though our informants did not mention such a character. The other mask has no painted lines at all. Each had a shingle fastened to the top (it is still present on one), to which were attached rattles. The masks were suspended by a string fastened to one edge and to the bridge of the nose, but
they are so narrow that they could not have covered the faces of the wearers.

In the dance described by Chapman, the chief characters of the second and fourth acts represented two cousins, sons of cannibal women (fig. 1: 1, 2). The sons danced in order to lure victims for their mothers. They were each accompanied by two female dancers (unmasked ?), carrying finger masks or circlets (similar to plate 17: A, 1).

We now come to the masks representing animals, birds, and spirit owners. A very large flat mask in the new collection (pl. 17: C, 5), was said to be an old Eskimo woman. It is, however, so similar to the old mask from the same village (pl. 18: 8), which represents the Blueberry Woman or Berry Woman (nuqátlu, "woman"), that it is more likely to be this character. The Berry Woman, acted by a woman, dances with a basket, pretending to pick berries. The new mask has green hair, a white face, black brows and lines around the eyes and mouth. The cheeks are red, and there are three black lines under the chin. The old mask is similar, except that the hair is black. Around the top and sides of the new mask (and formerly on the old one, also) is a frame of willow withes, fastened to short cross-sticks which are pegged into holes in the edge of the mask. Feather tufts are attached to this border, and a single feather rises from the center of the forehead.

Both the old and the new set contain identical masks, supposed to represent the Dog Salmon Woman (oepól, "fish woman") (pl. 17: C, 4; pl. 2: 9). They are slightly smaller than the Berry Woman masks, but are otherwise very similar. In spite of their size, none of these masks has any method of suspension in addition to the string. The chief difference between the Fish and Berry Women masks is that on the latter the hair is black and does not extend down the sides of the face. In the dance, the Salmon Woman (the part is taken by a woman) is preceded by three men, representing three birds of a certain species (unidentified) (pl. 17: A, 5; C, 6; C, 7), and who swing a "fish trap." After they have set this down, the Dog Salmon Woman enters, dancing backward, carrying the "fish-cap" (pl. 17: D, 1), to which is attached a stuffed loon which is dragged along the floor. She is flanked by two men, representing Seagulls or Terns (pl. 19: 1). According to this description, which was given by our informants in Hologochaket, we may infer that the dance represents the arrival of the salmon in the early summer. The three birds who open the dance are probably migratory birds whose coming warns the people that it is time to prepare their fish traps. The terns (or seagulls) come with the salmon, and the fish are followed by the loons. It is interesting to note that the Eskimo of Prince William Sound
Fig. 2. Masks and regalia from Anvik (after Chapman, pp. 24, 26, 27). 1, Dipnet frame carried by Seagulls in Salmon Dance (length 39"; white, dark wing tips, red face); 2, Seagull in Salmon Dance (width 22\"; white; face dark blue above [representing salmon's back] and white [salmon's belly], with red lips and throat [salmon's flesh]; salmon figures in holes are blue and white; red edge of mask [shore line], willow border [water] with feathers [white capped waves]); 4, Salmon Messenger (blue and white with red midrib); 5, Silver Salmon insignium (fish is blue above and white below).
say that the tern comes to announce the approach of the king salmon, the first of the runs in that district. The Berry Dance and the Salmon Dance are apparently performed for the purpose of insuring a plentiful supply of food for the next season. It is natural that a woman should perform the Berry Dance, since it is women who gather the berries. But it is curious that a female character should play the central rôle in a Salmon Dance.
which involves a fish trap, since the fish trap (and its modern improvement, the fish wheel) belong to the men, while the women fish with a net. This makes us wonder if there is not here a reflection of the Eskimo belief in the two female guardians of the food supply, the "Owner (or Soul) of the Land" and the "Owner of the Sea." However, this description of the dance differs in several respects from that witnessed by Chapman at Anvik. This may mean that our information is faulty and that this interpretation of it is likewise erroneous, or that there was a difference between the ceremony performed at Anvik in 1905 and those held in recent years at Hologochaket.

The Anvik dance was called the Dance of the Silver Salmon and the Gulls. The central dancer was the man representing the Father of the Silver Salmon (fig. 2: 3) who leads his tribe on their yearly migration up the river. He was flanked by two women (unmasked?) who exhibited the salmon insignia. The latter are wooden figures of a dog salmon and a silver salmon. The silver salmon was hollow and had a burning candle inside (fig. 2: 5). These were suspended from an inverted cage or basket (the "fish cap" of our informant), which symbolizes water. On the farther sides of the women were two masked men, representing Seagulls (fig. 2: 2). They carried as insignia the frames of dip nets, ornamented with feathers and a small carving of a fish (fig. 2: 1). On each side, in front of this line, danced the masked messengers of the Salmon (fig. 2: 4). (The significance of these messengers is not explained: they seem to take part in the important dances.) All the performers entered simultaneously (?). In the first part of the dance the insignia were exhibited; in the second part they were replaced by finger masks or feather circlets.

The chief differences between this dance and that described by our informants at Hologochaket is that in the latter there were three messengers (birds), and the Salmon Father and his two supporting female partners were replaced by one central Salmon Woman.

The three birds who preceded the Salmon Woman wear long slender masks, representing human faces. These masks are found only in the new set (pl. 17: A, 5; C, 6; C, 7). The masks are white, with a red stripe running up the bridge of the nose and bisecting the green spotted forehead. Guessing by analogy with other bird masks, this would mean that the birds had a spotted back. The small round mouth is outlined in black and has a red pit in the center, instead of the usual hole. The eyes are outlined in green and the usual feather rises from the forehead. The mask worn by the dancer in the center is fringed with (wolf ?) fur. The other two have a wicker frame around the edge, like that on the Salmon Woman and Berry Woman masks. This frame is striped with red. (It has been broken from
one of the masks). One mask has a single wooden bead hanging from the nose, and there were probably similar beads on the other masks. The "fish trap" which the birds carry is described as a very narrow ladder with tufts of feathers attached to one of the longitudinal pieces. It would seem to represent, not the trap itself, but a section of the fence which leads to the trap. It must have resembled rather closely the insignium used in the Otter Dance at Anvik, which is supposed to represent a pond (fig. 3: 1).

The "fish cap," also found only in the new collection (pl. 17: D, 1) is a basket-shaped frame of thin wooden strips. One of these is bent into a hoop to which are attached two curved cross-pieces. From the circular rim hang twigs to which are tied small wooden buttons, representing berries. The stuffed loon skin is tied to this frame by a string. We did not learn what the "fish cap" symbolized. From the shape we might infer that it was supposed to be the fish trap, though this hypothesis does not explain the berries hanging from it. By analogy with the salmon insignium of the Anvik dance this object ought to represent the water and contain a wooden fish.

According to one informant, the Salmon Woman was flanked by two Seagulls (nlabai) or Terns; according to another, only one Tern danced alone in front of her. This bird is represented by a single mask in the old collection (pl. 19: 1). It was not copied for the new set. The semi-human face has a tail at the top, wings (one is missing) at the side, and a bill instead of a mouth. The piece of wood forming the bill projects on the inner side of the mask to make a grip for the teeth. There was also a suspension string. The face, or body, is white; the tips of the upward curving wings and the end of the tail are black. The small round eyes are red, and the edges of the bill are also red. There was a feather tuft on the forehead.

The Seagull mask used at Anvik is in the shape of a white bird, with black wing tips (fig. 2: 2). On the middle of the back is the round human face, painted red, which represents the "thinking spirit" (or "owner") of the bird. A wooden fish hangs from the bill. Instead of the feather tuft, the tail of the bird is decorated with a border of feathers.

In both the new and the old collection is a single mask representing the Crane (mutlit) (pl. 17: B, 6; pl. 3: 3). According to one informant, three cranes danced together; another said that there was only one or possibly two. The shape of the mask represents a semi-human face, with wings, and with a long blue bill instead of a mouth. The bill on the old mask (on the new also ?) forms a mouth piece. The edges of the bill are red. The forehead, which also represents the back or tail of the bird, is red. The small, slanting eyes have black spectacles. The feather on the forehead has been broken from both masks. On the older specimen there seems to have been something fastened to the bottom of the mask, now broken out, to judge by a round
hole. The only significant difference between the two masks is that the wings of the old one are green, while those of the new are blue.

Both collections have a Raven (yωxkatsfi) but they exhibit the same type of difference as that between the Hologochaket and the Anvik Seagulls. The old Raven mask (pl. 19: 4) is in the shape of the bird, with black tail, head, and wings (now missing). In the center of the back is a small round human face. This represents the viyiG or “man behind the animal,” a concept equivalent to the Eskimo cuA or “owner” (Chapman’s “thinking spirit”) which is always in human form. This face is white, with red eyes and mouth, the former outlined in black. At the left corner of the mouth is a small pit, suggesting that some object (miniature labret ?) may have been inserted here. The bird’s head, at the bottom of the mask, and now broken off, was made of a separate piece of wood, pegging into a hole in the projection forming the neck. This peg did not serve as the mouth piece, for there is, instead, a wooden bar nailed across the back of the mask. The feather tuft at the root of the tail is broken off. The new Raven mask (pl. 17: C, 3) combined the bird with the human face. The black tailband back (the forehead of the human face) are at the top; the black wings are where the ears should be. The face is white, with human eyes and nose. The long black bill, instead of replacing the human nose and mouth as on other masks of this type, looks more like a long black tongue sticking out of the human mouth. We might note that in many of the myths which we collected on the Yukon, the Raven’s beak was often referred to as his “teeth.” On each side of the white face, just below the nose, two oblique lines with spurs on the upper edge vaguely suggest a black mustache or tattooing.

In the new collection there are two bird masks which are not found in the old. One of these is a bird called a “lush” (pl. 17: A, 2). The mask is in the shape of the bird, with red tail and black beak. On each side of the tail are attached separate pieces of wood, carved to represent the red, three-toed feet. Below the tail is the usual feather tuft. The body is spotted black. The wings are represented by two large feathers. In the center of the bird’s back is the small round white face of the spirit “owner,” with black outlines around eyes and mouth.

The other mask (pl. 17: A, 7) is the Arctic Owl. Here, the mask is both the body of the bird and the face of the “owner.” The black tail is at the top; the black beak with red edges continues the line of the human nose and takes the place of the human mouth. The face part is white with black eyebrows and eyes. There is a feather tuft at the base of the tail, and on each side are attached two small pointed pieces of wood, representing the “ears” of the owl. There are no wings. In the dance, the Owl is supposed to steal a rabbit caught in a snare. The rabbit (pl. 17: D, 3) is the stuffed
white, winter skin of the Arctic hare, which is mounted on a board with a stake at one end. A few willow branches, also attached to the board, represent the brush fence in which the snare is set.

In the old collection is a bird mask which was not replaced. This is a small specimen, said by one informant to be a Hawk. A more trustworthy native, however, said that it was a Woodpecker (Χωτλκωλ) and that at the end of his dance some one would knock on wood to imitate the woodpecker’s tapping. This mask combined the human face with the bird’s body (pl. 19: 2). The tail is at the top, the wings at the side, but instead of the beak alone, an entire bird’s head projects from where the human nose should be. This head is made of a separate piece of wood, and is nailed to the mask. Below it is a wide grinning mouth, with a small hole bored through the middle, possibly to hold some small object. (Compare with the hole at the bottom of the old Crane mask.) This specimen is broken, and part of the left side is missing. The wings were very short: the broken wing on the right side seems to have its full length, but has only about half its original width. Above the wings, at the root of the stumpy tail, were attached two other projections, probably the feet. These are missing, as is the feather at the base of the tail. The tip of the tail was drilled for the attachment of something. The mask is brown, spotted with black and white. Both the eyes of the human face and the bird’s head are small round holes. The latter are outlined in black, and there are traces of orange or red paint on and above the beak. On the inside of the mask is a long narrow peg for a mouth piece.

The fifth act of the Anvik ceremony was the Dance of the Ruffed Grouse (fig. 3: 3). The mask is a human face, with the bird’s tail, wings, and feet attached to the edge, and the bird’s head protruding from the mouth like a tongue. The man wearing this mask was flanked by two women, and his movements were caricatured by a grotesquely masked clown who danced behind him.

Besides the bird masks, the old collection contains two specimens representing the Red Fox (τωλάq’οι) (pl. 20: 1, 2), but there are none in the new set. The bushy tail is at the top, the animal’s head at the bottom, and the round human face of the spirit “owner” in the middle of the back. The masks are entirely red except for the white face and the tip of the tail. The mouths of the spirit faces are smiling; the eyes are very small and round. There was formerly a feather tuft at the root of the tail. On the larger mask (pl. 20: 1) the fox’s eyes are represented. The foxes also had small ears, pegged into the head, but now missing. There are said to have been legs at the four corners of the mask, but these are also gone. In addition, there are holes into which other things were pegged: on each side of the human face and below the face.

3. Crane (the end of the bill is missing). Lower row: 4. Raven (bill missing); 5, 6. Caribou (side and interior views).
Old masks from Hologochaket and Shageluk. Upper row (from left to right): 1, 2, Fox (legs missing from both masks); 3, Half-Man (Half-Woman?); 4, Half-Man. Lower row: 5, Old Woman (from Shageluk); 6, Finger mask (from Shageluk); 7, Bear (or Moose Man).
One of the animal dances seems to have been performed by three persons, but our informants gave conflicting identifications of the masks used. According to one, the central figure was a Caribou (γανοι) who was flanked by two Black Bear (cœc). The other said that the central dancer was a Moose and that his partners on the side were Moose-Men. The old set of masks contains one Caribou or Moose and one Bear or Moose-Man; the new set has only two masks of the second type.

The Caribou (pl. 19: 5), to take the interpretation of the informant who seemed the most reliable, is the largest mask in the collection (45 cm. long, 28 cm. wide), and represents the animal’s head, life-size. It is made out of a log, cut in two pieces, hollowed out, and nailed together again. It was formerly covered with skin, traces of which still remain. The eyes are round, with red rims, and formerly held small panes of glass. The edges of the open mouth are red. The nostrils are cut through. Formerly there were horns of willow. Two heavy pieces of fish-net cord which cross over the open top of the mask and a loop of baling wire served for suspension. There is also a mouth piece of wood nailed across the opening at the top of the mask. Because of its weight, it is possible that the mask was not entirely supported by the wearer. The loop of wire suggests that it may have been suspended from the wire across the back of the kashim.

There are two Bear or Moose-Man masks in the new collection (pl. 17: D, 4; the mate was not photographed) and one in the old (pl. 20: 7). These are all identical, with an exception noted below. They are quite large and heavy. The face is white, with light blue nose and forehead on the new masks, and light blue-green nose and forehead on the old. The large grinning mouth is red on the edges and is outlined in black. The small slanting eyes are black: the black outlines around them do not cross the nose. A black patch on the chin suggests a beard. Four slender black legs (now missing on the old mask), which seem to suggest moose rather than bear legs, are attached to the four corners. White feathers (also missing from the old mask) are stuck into holes around the edge. The general expression of the mask suggests the bear. A few features are found on the old mask about which we have no information with respect to the new. The former was supported by a string and by a wooden bar nailed across the inside for a mouth piece. Though the eyes are cut through, they are too high and far apart to have served the wearer, who must have looked out through the nostrils. There is a narrow slit under the nose where a nose pendant (?) may have been attached. At the top are three pits on each side where something may have been pegged in (the Moose-Man’s horns, according to one informant).

At Anvik the first dance of the ceremony was performed by three men
masked as Otters (fig. 3: 4), accompanied by two messengers, also masked to represent Otters (fig. 3: 2). The insignium, which is supposed to represent a pond, is a ladder-shaped object, decorated with sticks ("weeds") and feathers ("tufts of grass") (fig. 3: 1). It must have been similar to the "fish trap" carried at Hologochaket in the Salmon Dance. After the "pond" was displayed, the dancers used feather circlets. The mask of the Otter combined the human face with the tail and legs of the animal's body. The mask of the messengers suggests a headless body of an otter, with the small human face of the "thinking spirit," or "owner," in the middle of the back.

Besides the masks already described, the cache at Hologochaket contains several properties. Among these is a wooden figure of a caribou (pl. 17: B, 4) with white belly and legs and a black back. It is made of two pieces, hollowed out inside. On each side a round hole opens into the interior cavity. In the dance, this figure, with a lighted candle inside, is suspended by the swiveling snap of a dog's leash to the wire stretched across the back of the kashim. Two strings are fastened to the caribou figure so that it can be drawn back and forth across the hall. A Raven enters from the smoke hole and attacks the Caribou, but the latter finally gets away. We are not sure if the Raven in this case is represented by a man wearing the Raven mask, or a wooden figure carried on wires.

The device of the wooden animal figure used as a lantern has already been mentioned for the Salmon Dance at Anvik. In the same festival a stuffed seal with candle attached was hung from the wire across the kashim, and was jerked back and forth by strings, in time to the music.

The Hologochaket cache also contains a second stuffed loon (pl. 17: D, 2) suspended by a string from the middle of the back. We do not know its use. There are also a number of feather circlets (pl. 17: A, 1). These are rings of willow, decorated by large radiating feathers. Those used at Anvik were made of grass and were ornamented with feathers or tufts of caribou hair. There is also a slender wooden wand (pl. 17: A, 8), striped with red paint, to which are fastened two pairs of feathers. These objects were carried by the dancers, but we lack specific information about their use.

In contrast to the masks at Hologochaket which illustrate a single style, and which may be, as already suggested, the work of one man, is a mask from Shageluk, the gift of the Reverend and Mrs Henry Chapman. It represents an old woman (pl. 20: 5), and is rather similar in style to the masks used at Anvik in the dance of the old man tormented by the bad dream. As on these masks, the ears of the Shageluk specimen are made of separate pieces of wood, lashed on. Two black wooden beads hang from the nose. The mask is white, originally with black eyebrows and lashes. The eyes are small crescentic holes and lack the outlines so typical of the
Hologochaket masks. (In these respects it resembles the Anvik specimens.) Three or four (?) faint black lines on the chin may be tattooing. The edges of the eyes, the lips, and the inside of the ears still retain traces of the native red stain (not manufactured paint), and there seems to have been some red stain on the chin. The nostrils are cut through. The remains of a fur border encircling the mask can still be seen. Our informant at Anvik said that sometimes a young girl danced with this old woman.

From Shageluk (also the gift of the Chapmans) is a finger "mask" (pl. 20: 6), such as is carried in each hand by young girls. It is a wheel-shaped object of wood, with a ring at the bottom to fit over the finger. Painted red and green, it is ornamented on one side with small white beads set into the wood. Feathers and tufts of white caribou hair are inserted in the edges of the wheel.

Petroff describes a masked dance, "connected in some way with hunting reindeer," which he saw on the "Chageluk river." He states that the Indians lacked the kashim or dance hall of the Eskimo, and that the dances had to be held in the larger dwellings. This is not true at the present time. We quote Petroff's description:4

Two men, who had been donning their costumes behind a screen of deer-skins suddenly appeared in the center of the house, the sides of which were lined with spectators. One man was attired in a fantastic hunting costume, richly ornamented with beads, fringes, and tassels, and wearing a band around the head studded with eagles' feathers, and with bow and arrows in his hands. The stuffed skins of several animals and birds were drawn forth from some corner in rapid succession by means of strings, and as each animal appeared the hunter made an attempt to kill it. Every attempt, however, was foiled by the other man, who was dressed in imitation of a raven, with the appropriate mask and with wings fastened to this arms. With these wings he would spoil the hunter's aim, and then hop about, imitating admirably the awkward jumping of the crow, while he kept chattering away in derision of the awkward hunter. This was kept up for some time, until a shaman or sorcerer appeared upon the scene, dressed in a long hunting-shirt nearly covered with strings of bears' claws, eagles' beaks, beads, etc., and with rattles in both hands. The shaman pressed upon the hunter the acceptance of a charm or amulet, for which he received in payment nearly everything the hunter had about him. Then the animals began to appear again, the hunter slaying them one after another without any further interference from the raven. It was evidently unnecessary to look for any deep meaning in this performance, as it was only the shaman's advertisement of his charms and services pure and simple. In such festivals as are celebrated in memory of the dead the performances are more varied and of greater interest.

University of Pennsylvania Museum

NOTES ON PIMA LAND LAW
AND TENURE

By W. W. HILL

MOST Pima legalistic aspects lacked definition and were loosely formulated. The only exceptions to this were those laws governing land ownership, tenure, and inheritance. Published information on these subjects is extremely meager, hence it is with the view of filling out the general picture of Pima economic life that the following notes are set down.

The Pima had, and still have, a surplus of agricultural land above subsistence necessity. This was particularly true in the past when wild vegetable and animal foods played a much more important rôle in the economy than they do today. The village was the agricultural unit and was always located with reference to the availability of the water supply for farm and domestic use. Aside from this primary factor of water, the necessity of communal endeavor in irrigation and for protection against alien tribes tended to solidify these village units. They were controlled by a village headman whose position was inherited. This man was responsible only to the chief of the tribe and to public opinion. Whether or not this position existed before Spanish contacts is uncertain, but if it did there is no doubt that the Spanish were at least responsible for giving substance to the office and continuing it.

Land assignments under native custom were accomplished in two ways. When a large tract was to be taken in, it involved the coöperative efforts of individuals from one to three villages. Qualified men were first sent out to choose the land and to "survey" the canal location. Then the community or communities, with the permission and under the direction of the headmen, constructed the canal from the river to the selected area. When this was completed, the men who had taken part in the work chose or were assigned plots of land under the supervision of the headman assisted by an advisory body. This advisory "land board" usually consisted of six men. The headman customarily chose the most advantageous location for himself. In case of a dispute the decision of the headman was final.

The second type of assignment concerned a single individual. A man, or one of the men's relatives, applied to the village headman and a plot of land was designated for him. In both cases once the assignment was made, the land became the inalienable property of the assignee and his heirs. Woman were never assigned land and did not inherit it, though they owned certain usage rights in it which will be discussed in a later section.

All lands but those assigned were free for purposes other than agriculture. There were no privately owned grazing, hunting, or gathering dis-
tricts. Likewise, the wild products growing on assigned land might be harvested by anyone. "They matured without work on the land owners' part." However, this applied only to the products, not to the tree or bushes. No one but the owner had a right to cut down a tree, cactus, or bush that grew on his land.

Agricultural plots were of no particular size. The ends of the farms bordering on the ditch were measured off in "so many ropes," and each family received a section of land of the same width. (This width might and did vary in different villages.) The length of the farm was determined by the needs of the owner and by the feasibility of conveying water to the far end of the field. If the family outgrew the assignment, more land was given to them. The number of acres in each plot varied. According to Russell, each family farmed from one to five acres. However, according to informants, with the introduction of wheat in Spanish times, the cultivated area per household increased to as high as fifteen acres.

The responsibility for the development of the assigned plot was purely individual, though communal labor was often involved in clearing and harvesting. The family, occasionally with the help of neighbors, cleared the land, fenced it when necessary, and built and maintained the lateral or feeder ditches. Any outside assistance was always paid for in kind or in produce.

Water rights were included in the land title. Formerly the Gila River and springs furnished ample water for agricultural and domestic purposes. In rare cases when a shortage of water did occur farms were watered in turn. Otherwise, each was irrigated whenever the owner saw fit. The only regulation placed on water users was that they must contribute labor to the construction and maintenance of the main dams and canals. This was enforced at the will and under the direction of the village headman and a "ditch foreman."

The use of the land was under the direction of the patriarchal head of the family. His influence was partly inherited and partly due to experience and personal magnetism. His only coercive weapon was public opinion. He chose the type of crop, decided upon the time of planting and harvesting, and directed the irrigation and cultivation. The entire able-bodied portion of the family participated in the work. The crop itself was shared equally by both workers and dependents of the family unit. While women had no actual ownership in the land, their work in the field gave them rights in the crop and they were at liberty to dispose of portions of it with-

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out consulting other members of the family. Normally any exchanges were for other food products. On the other hand, children must consult their parents before using farm products for other than subsistence purposes.

If the land owner moved to another district, he turned his farm plot over to a close relative. This relative farmed the land and generally gave the owner some share of the crop, though this was not obligatory. When the owner returned, he again took charge of the land. If the move was known to be permanent, a gift, such as a cow or a horse, was given to the owner, and the land title was considered transferred. Such transfers usually occurred only between close relatives as there was a strong tendency to keep the land within the patrilineal family.

Two types of land rental were practiced. If a Pima, a non-relative of the owner, wished to farm he might obtain land for a share of the crop (this share varied) and by contributing his services to the maintenance of the irrigation system. Indians of other tribes, who applied for land, might be allowed to use it rent free, subject only to contributing their services towards the maintenance of the main dams and canals. Except in rare cases, these foreigners never gained actual title to the lands.

The most important legal aspect of land, aside from ownership, was inheritance. Normally, a man’s male children inherited all his land, and the oldest resident male member of the household assumed the patriarchal responsibilities of directing the agricultural procedure. Unless a verbal will had been made dividing the land, the household head was considered the nominal owner.

Women never inherited land. The widow retained her rights in a share of the farm produce as long as she stayed with the family unit. If she chose to return to her own family these rights were forfeited. Such rights were also forfeited in the case of divorce or separation, though they could be reestablished if the wife returned. The children of the union retained their rights in both land and croppage whether they stayed with their father or not, or whether or not their mother remarried.

Unmarried daughters had only produce rights, and these only as long as they lived with the family unit. On marriage they relinquished their share in their families’ croppage and became totally dependent on their husbands’ people. (However, it must be noted that a balance was affected by the inclusion of the daughters-in-law in every family as dependents.)

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2 One informant stated that women might trade produce only for food products. The others said that there were no restrictions on a woman’s disposal of farm produce.
In no case did a girl take property of any kind, except a few personal effects, to her husband’s home.  

Ordinarily land inherited by orphan children was cultivated for them by some relative. When they reached maturity they were given their full rights and became economically independent. It was extremely rare for the land of such individuals to lie idle.

In a case of the death of orphan children or old dependents, with no near relatives, the land was usually given to the individuals who had supported them, whether or not they had been relatives.

Should the deceased die without heirs or if the heirs concerned be distant relatives with sufficient land, the inheritance was usually reassigned to another individual. If only distant relatives survived the deceased, the distribution of the land was decided on in a family conference. This family conference also acted in the distribution of any personal property that remained.

Aside from ownership and inheritance, the legal aspects of land and crops were few. Those which were recognized were mainly concerned with crop damage.

Damage for the destruction of crops by cattle was collectable if the owner wished to push the case, though under ordinary conditions this was seldom done. The same applied to damage by ditch water. The responsibility lay with the individual who allowed his ditch to break, and damages could be collected if pressed. Restitution was most often collected for damage by fire. If an individual burning brush, stubble, or weeds allowed his fire to get away and burn up another’s crop, he was held liable. In all three cases, if disagreements over the amount of the payments occurred they were settled by the headman assisted by an advisory board.

Crop theft was unrecognized in the old days. A system of crop exchange existed between neighbors, and it was considered right for each to help himself if he saw fit. Likewise, a stranger might help himself from another’s field if he was in need of food. The reason for this appears to lie in the fact that there was an overabundance of agricultural and wild products.

Berkeley, California

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2 Matrilocal residence did occur, but was comparatively rare. “A man would be looked down on for allowing his wife’s people to support him.”

4 It was customary to destroy all personal property, livestock, the house, and storehouse at the death of the owner.
RITUAL FESTIVALS AND SOCIAL COHESION IN THE HINTERLAND OF THE GOLD COAST

By M. FORTES

SOCIAL cohesion, or some equivalent concept, has gained an honorable place in anthropological literature as a labor saving-device. To an ethnographer working with an Australian horde, a nomadic Beduin tribe, or even a people with a strong centralized government, it may seem a self-evident concept; a first law of social life like Newton’s first law of motion, by which everything else can be explained. In the Northern Territories of the Gold Coast, however, there is nothing self-evident about it. In the limited area north of the White Volta and east of 1° W. long. we find a congeries of peoples speaking different dialects of the Mossi-Dagomba language family with an ostensibly uniform culture, but lacking a centralized political organization. There are no villages in this country, but for miles and miles, continuously, one mud compound follows on another. There is often nothing to mark the boundary between one settlement and another, nor can exact frontiers between dialect areas be established. A short ethnographic residence in the country shows that a notion of a fixed and demarcated tribal unit, either as a linguistic grouping or as a political grouping owing a common allegiance, does not exist. There seems, in fact, to be no structural unit larger than the clan-settlement capable of exhibiting social cohesion.

The observations presented here refer to the small corner of this area occupied by the Tale settlements. The Tallensi are well-known to their neighbors for the ritual festivals which they celebrate. These festivals occur between the cessation of the rainy season in September and the commencement of the next rainy season in April. In this paper I shall deal only with a single facet of the festivals, their significance as a mechanism of social cohesion. To appreciate this, some knowledge, however over-simplified, of Tale social and political structure is necessary.

The Tallensi, being anciently settled agriculturalists, are inexorably bound to place. Local group and kinship group tend to be coterminous, hence the social classification of people is primarily in terms of the settlements. Intra-clan social relationships are of a uniform type all over the country. Relations between settlements, that is political relations, however, are of a different order. In keeping with the dominant trend of Tale social life towards decentralization and divergence, rather than centralization

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1 The present paper is based on material collected during a field expedition carried out in 1934–35 under the auspices of the International Institute of African Languages and Cultures.
and convergence, every clan puts an egocentric interpretation upon its
relations with other clans. These interpretations are compounded of preju-
dice, conventional opinions, traditional attitudes, and personal values;
but they conform to a pattern nevertheless. Lacking a central machinery
of government and a common allegiance, the Tale settlements have no
permanent political relations with one another. Even the Administration
has been able to impose only a limited and superficial degree of coördina-
tion. Traditionally and to this day the heads of clans coöperate in certain
contingencies or in certain periodically recurrent ritual situations only.
For the rest local autonomy is absolute. The upshot is that the political
relations of the Tallensi are a sum of the political relations of each settle-
ment with its neighbors. This means first, that geography is an important
factor in their politics, and second, that rigid political frontiers do not exist.

A glance at the political geography of the clans with which the present
paper is mainly concerned is therefore essential.

These Tale settlements fall into two major groups. On the one hand are
the clans known as the Namoos, on the other the Tallis clans. The principal
settlement of the Namoos is at Tongo, which was founded, according to
the tradition stereotyped throughout Taleland, by Mosur, who fled thither
from the Mampuru country in the south, probably over two centuries ago.
Mosur was the ancestor of all the people of Tongo and of several settle-
ments colonized from there. Later immigrants from Mampuru established
coclans of Tongo.

The Tallis (i.e. the “real Tallensi”) can be subdivided into two groups,
those living north of Tongo, the principal settlement of whom is at Baari;
and those living on and around the Tong Hills, whom I call the Hill Tallis.
The Gbizug lineage is in a special category, as we shall see. Tallis traditions
attribute diverse origins to their clans. Four clans, including Baari, Gbizug,
and Wakyi, claim primacy of rank in virtue of the fact that their ancestors
emerged from the earth or descended from heaven, and were there when
Mosur arrived. The other clans are offshoots of these, or of immigrant
origin. The Tallis have an ingenious system of clan concatenation, based
on a fiction of kinship. Broadly speaking, every lineage of every clan is
linked to a lineage of every neighboring Tallis clan by a fiction of half-
brotherhood which binds them to reciprocity of certain privileges and obli-
gations, prohibits intermarriage, and especially unites them in a common
religious cult. The Namoos clans conform in all respects to the classical
definition of a clan; the others must be designated thus for brevity’s sake
and for want of a term which will convey how they deviate from the
prototype.
Namoos and Tallis have a common cultural idiom, just as they have a single language. Their economic system is uniform and inclusive of both communities; their laws of land tenure, of inheritance and successions, of marriage and legitimacy are identical. The ritual practices and the mystical notions of their religious and magical institutions are the same in form and dynamical character, even if they sometimes differ in content, especially in the domestic cult of the ancestors. This common corpus of social definitions and of pragmatic organization is the inevitable correlate of the conditions of social and economic intercourse normally prevailing between the two communities. They have been intermarrying for generations, and this entails common legal techniques and principles, as well as a single type of domestic organization. They exchange commodities in trade, gifts, and in the discharge of kinship obligations: for every Namoos has numerous cognatic kinsmen among the Tallis and the Tallis among the Namoos. Kindred come into frequent contact with one another, not only on ritual occasions, such as sacrifices to ancestors and in funeral ceremonies, but also in ordinary social and personal affairs.

Upon this homogenous basis of social and economic relations between individuals and familial groupings is superposed a political structure the essential principle of which is a polar opposition defined and emphasized by the most stringent ritual observances. The head of the Tongo Namoos has the title Naa or Chief. The chieftanship (naam) was brought to Tongo by its founder, Mosur, from Mampurugu. It remains the absolute prerogative of his descendants. Every new chief of Tongo buys his rank from an hereditary elector who represents the chief of Mampurugu. The colonies and co-clans of Tongo elect chiefs of lesser grade. But no Tallis may hold the title Naa. Their heads of clans and lineages are typically entitled Tendaana, (literally "owner of the land"). The principal Tendaanaaships in the land are attained by rights of patrilineal succession. The chief of Tongo claims suzerainty over all the Tale settlements. Actually his executive authority reaches only to his own settlement, while a Tendaana's executive authority hardly extends beyond his own lineage. Balancing the chiefs' political claims, the Tendaanaas assert their precedence in virtue of their "ownership of the land." Moreover, each of the principal Tendaanaas claims precedence over all the others, such is the degree of local autonomy. Normally an occasion never arises where these rival claims can be pitted against one another. They are vaunted in the presence of the ethnologist, or in the ritual and domestic assemblies of kinsfolk.

Chief and Tendaana, Namoos and Tallis, are further separated by a barrier of taboos, many of them symbolic. The chief may not tread upon
the earth with his bare foot; he may not pluck a blade of grass or engage in agriculture, and so on. Tendaanas observe none of these taboos. They again may not wear any cloth garment, but only skins, for a white gown of cloth is put upon the chief at his investiture. Cloth garments, horses, guns, are traditionally tabooed to all Tallis, since they were characteristic of the Namoos. Chief and Tendaana regard these taboos as of the very substance of their respective offices, as moral obligations to their respective communities. So also property lost on the land or stray animals must be handed over to a Tendaana on pain of supernaturally inflicted death. But stray cows, dogs, or vagrant humans go to the chief. Most important of all, the religious practices of the Tallis are dominated by the cult of the earth shrines (tømbana)—sacred groves, streams, pools, etc.—to which the Namoos as a group have no access.

The polarity thus institutionally registered is paralleled by standardized political attitudes. In the sporadic fights of pre-European days Tallis and Namoos were traditional enemies. This is not only recollected in tales but vividly expressed in the military pantomime which accompanies every funeral ceremony. Tallis at their funerals fling taunts and challenges at the Namoos, and the latter retaliate at their funerals. When his patriotism is aroused, a member of one group will speak of the other group superciliously or derogatorily, forgetting for the moment, perhaps, that they are his own mother’s kinsmen.

Between Chief and Tendaana, between Namoos and Tallis, there are barriers, but not a gulf, an equilibrium and not an irreconcilable disjunction. The fulcrum of this equilibrium is the Tendaana of the Gbizug lineage. Geographically placed between the Namoos and the Tallis, he is the ritual and political mediator between them. The principal Tendaana of Baari can directly approach the Chief of Tongo; but the Tendaanas of the Hill Tallis or of non-Tale clans can only approach the Chief through the Gbizug Tendaana. In the old days it was he who made peace between the traditional enemies after a fight. A ritual and social equilibrium of high tension exists between Gbizug and Tongo, symbolized in the peculiar relations of the Tendaana and the Chief. Tradition, distorted by each side to its own aggrandizement, relates that it was the first Gbizug Tendaana who received and gave land to settle on to the ancestor of the Tongo Namoos. The final and most solemn of the rites by which a new chief of Tongo is inducted, rites which will be referred to again, must be carried out by the Gbizug Tendaana. Upon him rests the responsibility and the privilege of making the sacrifices for the most sacrosanct fetish of the Tongo Chiefship, that which safeguards the life and well-being of the Chief and thus
the prosperity of the land. Though it is housed in his compound, neither
the Chief nor any of his clansmen may set eyes on it; only the Gbizug
Tendaana and his lineage may. The Chief speaks of it with an awe which
is almost terror. The balance is adjusted by the Chief's rainmaking powers.
Only Namoos may own rain medicine, which was brought to Taleland by
their ancestor. When drought is prolonged, all the Tendaanas, led by the
Gbizug Tendaana, formally call upon and implore the Chief to see to it
that rain falls. So great is his rainmaking power that rain will fall at once
if he merely declares, in the name of his ancestors, that their wishes will
be satisfied. Such, in fact, proved to be the case in 1934.

Again, this ritual polarity is paralleled in social and personal attitudes.
The Chief and the Gbizug Tendaana, in private conversation, comment
with a mixture of pride, scorn, and fear each upon his opposite number.

Among the most striking mechanisms of this equilibrium are the ritual
festivals. The various settlements are always classified in terms of them,
and the natives never tire of dilating enthusiastically upon them.

Ritual and dance are the two components of every festival; the former
usually esoteric, the latter public though exclusive. During the rainy season,
which lasts roughly from April to September, ritual and group activities
are almost completely ousted by agriculture. In July-August the early
millet is harvested, but the major crops, the guinea corn and late millet,
still stand. The Tallensi have a lunar calendar, and the last month of the
rains is called the "Moon of Waters" (Kuom ŋmarig). An ethnologist can-
not, alas, be in two places at the same time. I was living among the Namoos
in September, 1934, and shall therefore describe the events of this and the
next month from their point of view.

Weeks in advance the approaching festivals are the dominant theme of
conversation for man, woman, and child. The ceremonial cycle is inaugu-
rated by the Baari Tendaana, who ritually "threw away the water,"
i.e. abolishes the rains on the first day of the new month. This is announced
by a burst of halloowing, which commences at Baari and sweeps across the
country, the signal that the festivals have come round again.

That night the people of Baari begin their celebrations. At Tongo, on
the next day, the youths and maidens are putting finishing touches to their
festive costumes. On the fourth day of the moon a deputation of elders
waits on the Chief, to remind him of his traditional obligation to summon
a diviner on the following day.

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2 The calendrical incidence of the ritual festivals and their relation to the annual pro-
ductive cycle are more fully set out in M. and S. L. Fortes, Food in the Domestic Economy
From this divination emerges, first, whom the ancestor spirits have selected to bring the sacred Gingaunq drum, after which the Namoos' festival is named, out of the Chief's court chamber. There are two Gingaunq drums: a small one, the sacred one brought by their ancestor Mosur, and a large one, subsequently manufactured merely to add liveliness to the dance. Second, the seance establishes what sacrifices have to be made upon the shrines of the original ancestors of Tongo. In this seance the Chief and his elders, the representatives, metaphorically speaking, of the clan conscience, show how vividly the hallowing of the tradition is felt by them. The sacrifices have a double intention; to thank the ancestors for a successful harvest of early millet and for the standing crop of guinea corn, and to avert quarrels and disputes which might occur when great concourses of people flock to the dance. It is for fear of quarrels which might lead to bloodshed and thus to unknown supernatural calamities that only a youth authorized by the ancestor spirits may inaugurate the dance. The divination, as befits the joyful occasion, is nevertheless cheerful.

The sacrifices are carried out immediately, and ritual then sinks into abeyance for a time. The nightly dance occupies the center of social interest, until the moon begins to wane. For a night or two the children have almost complete possession of the dedicated dancing field, practicing their awkward steps. With the waxing moon adult slacks to the dance, until in the end even young mothers come out carrying sleepy infants, and the drummers cannot be seen in the center of the great mass of people.

This first phase is aptly known as the Gingaunqdeema, "play Gingaunq." It is monopolized by the younger adults and adolescents, who try out the dancing songs. But the season gets its name mainly from the sexual effervescence associated with it. Women are the cause of the disputes which threaten the peace of the dance. Not a day passes without an elopement, planned on the dancing ground. Unfortunately, few of these marriages survive the festive period; and that, perhaps explains why there is a taboo against carrying out any of the legal formalities of marriage during this month. For Baari and Tongo, the Moon of Waters is a dangerous month, in spite of the festivity, because of the human passions aroused, as is evident from the fact that it is a grave sin to shed blood on the earth, even from a scratch or cut. Strangers, and in particular Tallis, flock to watch the dance, and of course to find lovers. But only Namoos may enter the dance.

A fortnight's lull ensues during which the guinea-corn is harvested. Then comes the Moon of Daq, so named from the final phase of the festival, and the "real Gingaunq," Gingaunq mepa. But I must resist the tempta-
tion to enter into an account of the dynamics of the dance itself. It is the first Gingaung incomparably enhanced. The older men, expert dancers, are in charge, though the younger ones and even women and children have full freedom to join.

A point of psychological interest is the choral refrain which provides the organizing slogan for the dance. It consists of a verse taunting a member of a co-clan of Tongo with some moral failing. To taunt the Tallis thus would be tantamount to declaring war.

The festival reaches a climax in the second week of the moon, when it squeezes out every other social activity. In the dance this is marked by a striking bit of pantomime which symbolizes the pride of the Namoos in their chieftainship and chiefly ancestors. Suddenly, long after midnight, when the moon is at its highest and the dance at a pitch of intensity, a dozen solemn figures begin to press slowly towards the center of the dance. Spectators and dancers make way for them. They stand in rank, shoulder to shoulder, unlike the dancers who form a file. The dancers shuffle, leap and stamp, but they bob gravely from foot to foot, erect, faces set like masks, chanting a low, wordless chant. They wear faded red caps and gorgeous, though sometimes tarnished, gowns—the garb of chiefs—and carry spears. They represent their grandfathers and ancestors who had been chiefs and men of rank. The natives delight both in the dramatic contrast, and in identifying the chiefs whom the mummers represent.

And now ritual returns. On the fifteenth night there is no dancing. On the contrary, not a single person stirs out of doors after dark. For that night all the ancestors of Tongo come to dance Gingaung, and he who hears or sees the phantom dance dies at once. Everybody hangs out his or her finest garments for the ancestors to borrow for their dance—spiritually, of course. For they keep a sharp look-out against human thieves, who have been known not to respect the occasion. Next day, towards noon, the Baari Tendaana calls upon the Chief of Tongo, bringing a small pot of consecrated beer. All present must drink of it, and the ancestors too, for a gourd-full is sent out to be poured on the grave of Mosur. It is a convivial scene, which instantly becomes serious as the Chief begins to address the Tendaana. This is the gist of his speech: The day has returned for us to meet as our ancestors used to, and to do as they did so that we shall have untroubled sleep, marry new wives, and beget many children. We have had a good harvest. May we all live to see Golib and sow our millet in that moon successfully. May my chiefly ancestors and Mosur permit this and permit us to gain new life so that next year at this time we again celebrate Daa. The Tendaana responds with a similar blessing, saying that Baat
Daa (the supreme boyar of the Baari people) and Mosur will jointly prosper the land. Then he departs. That day Baari celebrates Daa.

That same evening the large Ginggaun drum is escorted to Baari by the Gbizug Tendaana and the Wakyi Tendaana, following a ritual path. All Tongo flocks to Baari to dance with great enthusiasm on a traditional spot outside the grove of Baat Daa. Near the dance, but silently aloof, wait the Gbizug and Wakyi Tendaanas. A few hundred yards away, as if indifferent to the dance, young men of Baari assemble, wearing their ritual garb, a goatskin. When the Baari Tendaana and his ritual coadjutors arrive to join them, the Gbizug and Wakyi Tendaanas are summoned. Greetings are exchanged, and then led by the Baari Tendaana, the whole company files away in a most solemn and silent procession, brought up by the youngest members carrying pots of consecrated beer. With utter gravity this procession thrice encircles the dancers who continue to dance as if oblivious to this. Anyone who dares to break through the encircling procession is doomed to a supernatural death. The procession files away again, into the sacred grove. There the Tendaanas pour libations of beer to Baat Daa, calling down the very same blessings as had been invoked by the Chief earlier in the day. They drink the beer and send a small pot out to the representative of the Chief, who is among the dancers. Now the Tendaanas’ procession emerges, again solemnly encircles the dancers, and files away. At a tree which marks the path to Tongo they stop, and marching round it in a circle, chant a song the burden of which is Mosur boot kuliga (“Mosur wants to go home”). Upon this signal the allusion of which is obvious to all, the dance breaks up, the ritual is over.

That finishes the Ginggaun dance for a year. The next three days at Tongo are given over to the gayest festivities of the season, the celebration of Daa. This is a New Year’s festival, a fitting conclusion to the dancing season. Dressed in their best and gaudiest clothes, the whole of Tongo assembles at the Chief’s compound on the day after the rites at Baari. From far and near, Tongo women who have married away return to their homes, many bearing gifts of special food from their husbands. The Chief’s house throngs with his people and strangers. His elders bring him presents of guinea fowls and he, sitting in state, has a bullock slaughtered, part of which he shares amongst them. The Chief, it is true, gets the better part of this exchange of gifts, but good form is satisfied. The afternoon is devoted to light-hearted dancing in praise of the Chief.

The Gbizug Tendaana has also celebrated Daa, but very quietly. That evening, however, Chief and Tendaana meet in ceremonial. The scene is the dancing ground. This is a dedicated area which may not be cultivated.
For this was the site of the first Gbizug Tendaana's compound when he received Mosur; and it is here that the Gbizug Tendaana receives every newly elected Chief of Tongo. After his investiture the Chief must never set foot in it again.

Towards evening a brother of the Chief arrives at the dancing ground, wearing the red cap and the gown, the insignia of a chief, and followed by boys carrying pots of beer. He takes his seat on the flat rock on which every newly elected chief sits down to await the Gbizug Tendaana. For an hour or two he sits gravely there, receiving the humble greetings of the men of Tongo. He is Chief for the nonce. Then a messenger arrives to announce the Gbizug Tendaana. The latter, escorted by men of his lineage, arrives in the dark and greets the mock-chief. The beer is divided out among those present, and then the Tendaana retires a dozen paces. In a voice inaudible to the Namoos, he calls upon his ancestor, the first Tendaana, and asks for the blessings of a good harvest, wives, children, and happiness in the coming year. The mock-chief dismisses his people and takes the homeward path, followed at some distance by the Tendaana and his people. As soon as he arrives at the Chief's compound, he and every other person in the compound, including the Chief himself, hides behind closed doorways in the rooms. The Tendaana enters a silent and empty compound. He goes to the secret place where the sacrosanct Chieftainship fetish is kept, pours a libation to it, and asks blessings for the Chief and all the land. When he has finished he enters the Chief's room to offer him New Year greetings, and so departs. Thus is the Chief every year renewed by a symbolic drama reenacting his first installation.

Two more days of festivity remain, during which all Tongo celebrates the New Year. Great quantities of food are cooked; there is visiting and counter-visiting, especially by affinal and cognatic relatives, and every one congratulates his neighbor on having seen another year. But the major interest of Daa is the holocaust of sacrifices which accompanies it. Fowls, guinea fowls, dogs, sheep, goats are slaughtered by the dozen in sacrifice to ancestors and to medicine fetishes. The roots of the latter are renewed, and thus given a new lease of life. There is more dancing to celebrate this, and another visit of the Baari Tendaana to the Chief to invoke blessings for the coming year. When the moon ends a few days after, the Namoos have finally done with their Harvest and New Year festival. Before that the Chief may not eat of the new guinea corn, though commoners do.

Two points deserve notice in this series of ritual celebrations. First, they express clearly the ambivalent relationships between the Namoos and their neighbors, and the bridging function of the Gbizug Tendaana. It is as if they were joined in a mutual responsibility each for the other,
based upon a profound antagonism. Second, the rites evidently recreate and regenerate the religious, the magical, and the traditional bulwarks of the social life of the Namoos.

A week later the scene shifts to the Hill Tallis. On the first day of the following moon, Boγaraam ηmarig, the Tallis begin their harvest festival of the same name. Despite the difference of name, it is the Daa festival translated into the peculiar and esoteric ritual idiom of the Hill Tallis. Namoos and Tallis, as the exclusion of the former from the inner mysteries at Baari indicated, constitute two religious sects. The cult of the boγar, after which the Hill Tallis name their festival, dominates the religion of all the Tallis, but is known to the Namoos only by hearsay. Its hypertrophied development among the Hill Tallis is indeed completely hidden from their own sect-brethren at Baari and Gbizug. A boγar may be a grove of trees, a cave in the hillside, or a small natural enclosure made by trees and boulders. It is truly sacred, for it may be entered only for ritual purposes, and in the company of all the ritual officiants. A boγar’s communicants generally comprise a group of linked lineages or of several clans, each of which appoints one officiant. All the ritual of the boγar cult is built up on the notion of sacrifice, and every officiant or his representative must be present at every sacrifice. It is an axiom that a quarrel or dispute between communicants must be healed at once, by rites of reconciliation at the boγar. The boγar is the pinnacle of their ancestor cult, for it is there that all the ancestors of the communicants reside. It has tremendous magical power, amoral, like all Tale magic, and therefore applicable both for beneficent and maleficent ends. Thus its potency as fertility magic is widely renowned, and it can be invoked to bring death and destruction upon an enemy, by its communicants.

This, and much more, I learnt with very great difficulty, only after I had been initiated. For many months my enquiries were fruitless. The Namoos know the dates of Boγaraam, and the dances. Of the rituals they had, despite centuries of intimate contact, absolutely no knowledge. Indeed they refuse to hear of it, for they regard the cults of the Tallis with fear and aversion. The Tallis, on the other hand, never ceased to warn me against betraying what I had seen to my Namoos friends. It is, for them, the sin against the Holy Ghost punishable by instantaneous death, inflicted by the boγar. This, the most absolute cleavage between the Namoos and the Hill Tallis, depends upon the fact that the latter initiate their sons into their cult. Nothing corresponding to initiation exists among the Namoos or even Gbizug or Baari. The Boγaraam ritual is at the same time communion, initiation, and offering of first fruits.

The various boγar congregations commence the ritual of Boγaraam on
successive days, starting from the first day of the moon. The order of entry
is fixed by tradition, and claims to precedence in rank are based in part on
this. But preliminary sacrifices, consultations of diviners, collection of the
chickens and guinea fowls which will be required, and finally ritual prepara-
tion of beer, usher in the festival. From the "Moon of Waters" on, every
head of a joint family must eat out of special vessels, lest his food or water
be contaminated by the newly harvested guinea corn flour, ground beans,
or ocra. None of these foods are prohibited to women and younger members
of the family. The taboo depends not upon some notions of the magical
danger of eating unconsecrated first fruits, but upon the relationship of
the communicants to the boyar, who has the prerogative of tasting them
first.

I can not describe the esoteric rites of Bɔyarãam in detail. The main
events take place during one night and the next day. The initiand youths,
selected simply by the fact that their fathers can afford the numerous fowls
and guinea fowls and the beer necessary for the rites, are pounced upon at
night, undressed, and segregated throughout the rites. The communicants
wear skins, the ritual garb of the Tallis. Women and uninitiated boys are
rigorously excluded from earshot. Each elder brings fowls, guinea fowls,
guinea corn flour, and cooked ground beans, and sacrifices are made in
great numbers both at the houses of the Tendaanas and inside the boyar.
The ancestors are called upon and the boyar is invoked. "Your day has
come round again. We are about to eat the new guinea corn. Hence we bring
you this offering of guinea corn and beans and these, your boyarãam chick-
ens. Grant us untroubled sleeping. May our wives bear children, and may
we sow early millet successfully and meet again next year." This is the
theme of the sacrificial invocations. Every chicken thus slaughtered is
observed with concentrated anxiety. It must come to rest on its back, else
the donor has sinned in some way and must at once go to a diviner to dis-
cover how. For the initiands long life, success in marriage and in farming,
and safety from enemies is begged. It is a very serious thing if an initiand's
fowl is rejected by the ancestors; and all proceedings are suspended until
the sin is traced and atonement made.

The ritual is sensationally dramatic, and entirely different in style and
emphasis from Namooß ritual. There is a constant interweaving of the three
motifs, of communion, initiation, and first fruit offerings. The tone fluctu-
ates between intense and almost terrified ritual participation and casual
conviviality, when the beer is being divided.

The boyar is first entered mysteriously in pitch darkness; but the "high
spot" is what might be called the administration of the sacrament to the
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initiands next day. They have been treated throughout with veiled con-
tumely, like infants and outcasts, seated on the bare ground, their backs to
the ceremonial. When the sacrifices are over and the beer consumed, the
initiands are brusquely dragged up, each in turn, to the altar of the bɔγar.
Kneeling before it, quivering with fear, his eyes blindfolded by the hands of
one officiant, his hands clasping a chicken whose neck he saws off on a rough
stone with the help of another officiant, he is made to swear repeatedly "If
I tell anybody may I die." This then is the secret of the impregnability of
the Tallis bɔγar cult both to the Namoos and to the ethnologist.

Before they leave, the meat and flour of all the sacrifices are divided out
according to strict rules of prerogative; the initiands being fed with a
tiny piece of meat and flour dumpling each. The rôle of each participant
kinship group is defined in terms of the officiant it elects, the contributions
it makes to the sacrifices, and the share of meat, beer, and flour it receives.
The natives stress this organizational balance more jealously than details
of ritual; for it is the structural framework which preserves the ritual collab-
oration. Finally, all the communicants, including those newly initiated,
are daubed on brows, arms, and knees with consecrated red mud, the con-
crete emblem of the beneficent magic they are carrying home.

Bɔγaraam, like Daa, is the occasion when married daughters return
home with gifts of food and guinea fowls from their husbands, and men who
have migrated to distant parts try to return for the ceremonies. Dancing
follows after the ritual day. But as each settlement dances by itself, the
Bɔγaraam dancing is neither so impressive nor so great a focus of social
interest as Gingaun. This time the Namoos are onlookers, for they may
not enter the Tallis dances.

The Bɔγaraam festival of the Hill Tallis thus appears to be homologous
with the Gingaun and Daa of the Namoos, and the Daa of Baari, in its
calendarical reference and its functional context. For Chief, Tendaanas, and
elders, the fathers of the community who bear the greatest burden of re-
sponsibility for its welfare, the ritual is the dominant theme. For those of
lesser social responsibility, the dancing and festivity, the fellowship of
jubilation, are of major value. For everybody, these are festivals of reunion
in which family and wider agnatic connections, the unique fact for the in-
dividual of having been born into a certain family and clan, receive special
emphasis.

But what must chiefly interest us, in this paper, is to observe how these
festivals differentiate and bind the Namoos and the Tallis. On the one hand
it seems as if each community consolidates itself socially and morally by
the very act of repulsing its neighbor. Behind its stone wall of exclusiveness,
even to the date of its festival, each community celebrates its release from the hazards of the past year and especially of the food-growing season, and fortifies itself by magical and religious techniques for another year. Nothing could more strikingly demonstrate the factors of dichotomy and antagonism in the polarity which we saw previously to be fundamental to Tale political structure than the difference between the rites of the Hill Tallis and those of the Namoos. It is true that every Tale ceremonial activity expresses the exclusiveness of the group performing it; but in these festivals the expression of difference has a political validity because it is reciprocal. The difference is published in the dance and dramatized in esoteric ritual.

But Namoos and Tallis are culturally equivalent communities, dwelling in close juxtaposition, having intimate economic and social relationships with each other. This, it seems to me, sets a limit to the degree of antagonistic differentiation tolerable; and thus the other factor in the polarity of Tale society comes into existence—the equilibrium between the two communities. The very opposition, as it were, engenders dependence. This aspect of Namoos-Tallis relations comes out clearly in the ceremony at Baari, in the visits of the Baari Tendaana to the Chief, and in the ritual reenacting of the Chief’s induction at Tongo. It is notable how these rituals vividly insulate each group from the other, while at the same time uniting them in common responsibility for the welfare of the country.

The same compulsion to cooperate in the magical safeguarding of the land and the people obtains between the Namoos and the Hill Tallis. It is expressed in the Golib festival, which unfortunately space does not permit me to describe fully. The purpose of this festival, which occurs in the last month of the dry season, is to ensure a successful sowing and harvesting of early millet and to call down the blessing of fertility in general. In it all the clans of the Hill Tallis collaborate ritually and unite to dance together. In their ritual collaboration the distribution of offices and sacred apparatus among the several clans is so remarkably equal that each one of them can claim with ostensibly full justification to be more important than the others. It so happens that a bitter personal feud is at present raging between one of these officiants, on the one hand, and the Chief of Tongo and another of the Golib officiants, on the other. But none of them could escape his obligation to cooperate with his enemy in the appropriate ceremonial situations. Here, perhaps, is a clue to the problem of why ritual forms and religious situations should be the institutional media selected for the expression of the equilibrium in Tale society. They have a compulsive power which a pragmatic institution orientated to the demands of the objective world could never have.
G'ôlib is a dangerous month, during which both Namoos and Tallis are subject to numerous taboos, mainly aimed at averting quarrels and strife at the dances and in everyday relations between the groups. It is forbidden to shed blood on the earth, lest supernatural death overtake one, to take a wife, to carry out the final ceremonies connected with delayed funeral rites, to wear cloth garments in any of the Hill Tallis settlements, and so forth. Marriages are the cause of most inter-group litigation, and funeral ceremonies represent social situations in which large numbers of people from different settlements congregate and often give rise to disputes. Throughout, the Namoos are spectators of the dancing and excluded from the ritual. But on two occasions they meet the Hill Tallis in ceremonial interaction. In the inaugural rites the Hill Tallis, led by their Tendaanas, march to all their sacred groves in succession blowing sacred whistles in honor of them. When they reach the sacred groves which lie in Tongo, every Namoo man, woman, and child hides indoors. A Namoo who sees these rites of the Hill Tallis risks the destruction of his entire home and family by the outraged sacred groves. Once again, therefore, are the Namoos and Tallis mutually insulated or segregated by the symbolism of unchallengable ritual, the living affirmation of tradition. But there is a ritual reintegration of the groups. The final rite of G'ôlib requires the coöperation of the Chief of Tongo. Again led by their Tendaanas, the Hill Tallis come down to the Chief's house to dance; and there Tendaanas and Chief join together to beg for blessings upon the land, for the safety of the people and the abundance of crops.

To revert to the harvest festivals, the magical and religious value of the ceremonial to the group performing it must not blind us to its socially integrative functions. The former is mainly determined by the seasonal context of the festivals and is evidenced preeminentiy in commonplace techniques, such as the sacrifice. The latter, which is determined primarily by the context of the social structure, is evidenced in the ceremonies peculiar to the occasion. These differ completely in content, as between Namoos and Tallis, a fact which the natives would explain by citing the historically different origins of the two groups. This suggests the principle common to all the integrative ceremonies. It is the dramatization of the nodal, hence traditionally sanctioned, relationship in the social structure of each group—the Chieftainship at Tongo, the right of access to the bôyar among the Tallis. This fundamental bond is resuscitated in both the symbolical and the direct phases of the ceremonies. The Chief and the Gbizug or Baari Tendaana thus reimpose on each other those responsibilities which bind them to a common task; and, by the very same rites, the Chief and his
people redefine each the place of the other in a social system the essence of which is the Chieftainship. The Hill Tallis, again, reestablish the responsibility of each initiated man for the preservation of their traditional cult, by imposing it upon the next generation, and thus reaffirm the loyalty of each to the other, and of sons to fathers.

To sum up, social cohesion as I understand this concept, both within and between the major communities which constitute Tale society, is no ultimate attribute of that society, but is achieved by specific social mechanisms such as those I have described. To me its most significant feature appears to be the fact that this integration is engendered as an equilibrium between opposed groups, over-riding, it seems, the tendencies to conflict inherent in the system. It is worthy of note that this principle of a balance of powers is exploited also within each community to overcome the competitive autonomy of its constituent groupings.

Whether only a single general process is involved, accommodation to an immigrant group, is beyond my power to determine. One could presumably envisage the entire pattern of ritual and political values as a "reaction" to the "foreign body" represented by the Namoos clans. It seems not unlikely also that the equilibrium thus achieved is itself the barrier that prevents the diffusion of the esoteric rites of one community to the other.

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WAS THE EXTINCT GIRAFFE (SIVATHE-RIUM) KNOWN TO THE EARLY SUMERIANS? By EDWIN H. COLBERT

INTRODUCTION

DURING the course of archaeological excavations at Kish, Iraq, the Field Museum-Oxford University Joint Expedition discovered an object which, when cleaned and prepared, proved to be a copper rein ring so constructed as to fit on the tongue of a chariot. It was found at the 3500 B.C. level, associated with the remains of a chariot, and nearby were found skeletal remains and teeth of Equus.

The rein ring is especially interesting because it is surmounted by a small figurine of an antlered ruminant—a most unusual association, since rein rings from Kish commonly carry figures of equids as decorations. The figurine in question was supposed to be representative of a peculiar kind of stag, and this identification has been generally accepted for it. There are certain reasons to think, however, that the statuette may actually depict Sivatherium, an extinct genus of the Giraffidae. The grounds for this supposition will be presented in the subsequent paragraphs of the present paper.

My thanks are due to the Field Museum of Natural History for permission to use the photographs appearing in this article, and particularly to Mr Henry Field for his interest in this subject and for helpful information that he supplied. Some teeth of the equines found in the deposit at Kish have been studied and identified by Mr S. H. Chubb of the American Museum of Natural History.

DISCUSSION

As was mentioned in the Introduction, the figurine on the rein ring from Kish was at first supposed to be that of a stag. To quote from Dr Berthold Laufer, formerly Curator of Anthropology at the Field Museum:

Copper rein rings were known from Kish heretofore, but the previous ones were surmounted by the figure of a species of Equidæ or simply by metal loops for the reins. In this case the figure was an unsuspected member of the Cervidæ, and a long and heavy rope from the animal's muzzle indicates that stags must have been captured alive and tamed by the early Sumerians.¹

If the figurine does represent a deer, the two small, conical knobs or prongs on the forehead, directly above the eyes, are difficult to explain. No

¹ B. Laufer, Tamed Deer in Ancient Times.
known deer have frontal growths such as these. In *Cervus eldi* or *Cervus axis*, the thameng and axis deer, there is a very heavy anterior prong or "brow tine" on the antler, and it projects forward over the eye. But then this brow tine is definitely a part of the antler, and it does not seem likely that any primitive artist having the skill necessary to model the figurine now under consideration would represent a brow tine as a separate up-growth from the forehead. In addition, it may be pointed out that in the thameng, the sambar, or the axis deer, forms having a large and heavy brow tine, the beam of the antler is free from any proximal secondary tines, whereas in the figurine from Kish there is a strong inwardly projecting tine or point at the base of each "antler."

There is a small Indian antelope, *Tetracerus quadricornis*, which has an anterior pair of frontal horns as well as the typical bovine parietal horns. But *Tetracerus* is a very small animal, and the horns are simple prongs, not at all like the skull structures shown in the figurine from Kish.

Now let us consider the resemblances between the figurine and the Pleistocene giraffe, *Sivatherium*.

1. *Sivatherium* was a very large animal having body proportions similar to those of a large ox; that is, the limbs and the neck were not elongated as in the modern African giraffe. The figurine represents an animal with a normal ruminant body.

2. In *Sivatherium* the skull carried two sets of horn cores, a relatively small conical pair directly over the eyes and a large palmate pair at the back of the skull. This same arrangement of "horns" or "antlers" is shown by the figurine from Kish.

3. The small conical horn cores of *Sivatherium* are of frontal origin, and such would seem to be the case in the figurine.

4. The large, palmate horn cores of *Sivatherium* are of parietal origin, that is they are located on the occiput. In the figurine the palmate "horn cores" or "antlers" would certainly seem to be located on the back of the skull, as in *Sivatherium*, rather than on the frontal as is the rule in the Cervidæ.

5. The posterior horn cores of *Sivatherium* are typified by inwardly projecting points at their bases, and the same features are shown in the figurine.

6. In the posterior horn cores of *Sivatherium* the bases are transversely broadened, and somewhat flattened on their anterior and posterior surfaces, giving them elliptical cross sections. A similar condition would seem to be shown in the posterior antlers of the Kish figurine.
Rein ring surmounted by a figure of an antlered ruminant, from Kish, Iraq. (Field Museum of Natural History coll. Photograph reproduced by permission of the Field Museum.)
At left: detailed view of the head of the figurine shown in Plate 21. At right: reconstruction of the head of *Sintherian giganteum* Falconer and Cauley. (Drawn by Margaret Matthew Colbert.)
7. In the statuette from Kish it would appear as if the nose is swollen and somewhat dependent, as might have been the case in *Sivatherium*, a genus characterized in part by its abbreviated nasal bones. On the other hand, it may be possible that the peculiar appearance of the nasal region in the statuette can be explained as the representation of a halter on the muzzle of the animal.

Thus there would seem to be a series of rather strong resemblances between the figurine discovered at Kish and the Pleistocene giraffe, *Sivatherium*. Whether these resemblances are real or merely fortuitous is a highly debatable question. Perhaps the Sumerian artist was giving free play to his imagination, and by chance happened to make an animal that looks strikingly like *Sivatherium*. On the other hand, the separate conical projections over the eyes, and the general form of the palmate horns or antlers at the back of the head in the model, are so very much like the horn cores of *Sivatherium* that there immediately comes to mind the suggestion that the artist really might have seen a *Sivatherium*.

The possibility that *Sivatherium* might have persisted on much later than its typical Pleistocene occurrence in India has recently been strengthened by discoveries in India by Dr Hellmut de Terra and in Africa by Dr L. S. B. Leakey. In India Dr de Terra has found numerous Palaeolithic artifacts in the Boulder Conglomerate zone of the Upper Siwaliks, showing that man lived there during Middle Pleistocene times. *Sivatherium* is found in the Upper Siwaliks, just beneath the Boulder Conglomerate zone, and it is quite probable that it may have persisted on into the Boulder Conglomerate beds. In Africa Dr Leaky has found a horn core strikingly like that of *Sivatherium*, together with other typical Upper Siwalik fossils, in association with human artifacts.

Therefore it would seem to be evident that *Sivatherium* might have been known to Palaeolithic man. Moreover, Dr Leaky’s discovery would seem to indicate that *Sivatherium* migrated from India, the place of its origin, to Africa. This migration must have followed a route through Asia Minor.

Is it not possible, then, that somewhere in Asia Minor *Sivatherium* persisted until comparatively recent times? This question is at present more or less in the realm of speculation, but it need not be rejected as entirely hypothetical, especially in view of the evidence afforded by the little figurine found on a rein ring at Kish.

The accompanying figures illustrate the statuette with comparison to a restoration of *Sivatherium*.

Mr Chubb’s study of the equid teeth from Kish shows that both horses and asses were domesticated by early Sumerians.
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AMERICAN MUSEUM OF NATURAL HISTORY

NEW YORK CITY
OLD beliefs die hard” is an expression the truth of which is constantly being brought home to us. It is as true in science as in any other branch of mental activity. Ideas which have held sway for a lengthened period are not easily overthrown, whatever be the contrary evidence. Many such outgrown beliefs still hold in the field of American archaeology. We find this to be especially so when we turn to South America, not entirely for want of modern investigation, but in great part owing to the difficulty of obtaining the results of what has already been done, much of it being published in foreign languages and in books or magazines which are not within reach of most students.

One of these old beliefs attributes to the Incas the greater part of the pre-Spanish cultural remains found in those regions which formed part of the Inca empire, including Equador, Peru itself, Bolivia, northern and central Chile, and the northwestern provinces of Argentina. Modern studies, however, have shown that only a small proportion of such remains are truly Inca, and that in all these countries, which the Inca invasion overran and partly absorbed, there were earlier cultures, some of which were just as advanced, and in some cases even more so, than that of the Cuzco tribe. It may even be said that the Inca civilization obtained many of its elements from these neighboring cultures.

These facts are well known and admitted as regards the coast cultures of Chimu, Nazca, or Paracas and the mountain cultures of Tiahuanaco, Recuay, and Chavin de Huantar, as the principal museums are well provided with pottery and textiles of all these ancient civilizations. But besides these, there are many other local cultures not so well known, but equally characteristic.

It must be remembered that before 1350 A.D. the Incas had not yet left the Cuzco Valley and its immediate neighborhood, and that only in the reign of Yupanqui Pachacuti (1400 to 1439 A.D. according to Garcilaso) was initiated that series of conquests that little by little built up the Inca empire.

The conquest of Chile was one of the last undertaken. During the reign of Tupac Yupanqui (son of the former monarch), about seventy years before the arrival of the Spaniards, the Inca armies dominated the northern part of the country as far south as Coquimbo, but only in the reign of Huainà Capac, twenty-five years later, did they reach central Chile.

The Incas found in Chile a series of cultures, not so advanced as those of Peru, but nevertheless well developed; in many respects similar to that
of the Pueblo Indians of the southwestern states. Of these, the most northerly was that of the Atacameños, who occupied the region between Tacna and Arica in the north and the desert of Atacama in the south.

![Pottery drawings](image)

**Fig. 1.** a–c, Pottery of the indigenous culture. d, A Chincha-Atacameño pot.

This people spoke a language, now disappeared, quite distinct from Quechua or Aymara, and the investigations of Professor Max Uhle have shown that during a period prior to the appearance of the older Peruvian cultures, they overran the whole of southern Peru and the highlands, as far north as Cuzco, including the drainage basin of Lake Titicaca. The whole of this district is full of geographical names derived from the Cunza

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1 Fundamentos Etnicos y Arqueología de Arica y Tacna (Quito, 1922).
or Atacameño language. As the archaeology of a large part of this zone has
been intensively studied of late years, the stratified sequence of the cultures
found in it has been investigated and a relative chronology established up to
the time of the Spanish conquest. Uhle considers that the earliest of these

cultures began during the first centuries of the Christian era, and in many
parts their sequence has been continuous up to the present time. In these,
however, there have been found no remains that might be attributed to
the Atacameños, and it seems necessary to accept the fact that the latter
migrated towards the south before the advance of these higher cultures,
leaving nothing behind them but the geographical denominations they had
imposed and which still endure, and probably the domestication of the
llama. Uhle thinks that some of the motives of Tiahuanaco art are also to
be attributed to this people, derived from their ancient basketry. He says:

Fig. 2. Figure of the Chincha-Atacameño period. (Height 10\frac{1}{2} inches.)
Very early the Atacameños, marching from south to north, occupied as the dominating race, all the north of Chile, at the same time spreading over the south and west of the Bolivian highlands as far as the river Desaguadero and the drainage basin of Lake Titicaca. They imposed the stamp of their individuality also on the southern regions of Peru as far as the latitude of Ica on the coast and near to Ayacucho in the interior; by which they prepared the substratum of nations ready to receive the influence of the civilizations introduced from the north.\textsuperscript{2}

![Pottery of Tiahuanaco type found in San Pedro de Atacama.](image)

Farther on, speaking of the origins of the Tiahuanaco culture, he supposes that this was formed of three elements of different character: the element Proto-Nazca and Chavin, to which it owed its architectural ideas, the technique and stylistic details of its principal industries, and especially its figurative ideas; the Aymara, the influence of which appears in its special religious system; and the Atacameño, probably the author of the step-like figures which hold such an important place in the Tiahuanaco style. . . . The Atacameño denomination of many localities in the vicinity of Lake Titicaca is therefore prior to the civilization of Tiahuanaco.\textsuperscript{3}

\textsuperscript{2} Op. cit., p. 45.
The earliest material remains of this people have been found in a few coast localities: Arica, Pisagua, Cobija. These show them to have been basket makers, but there are no signs either of agriculture or pottery. Textiles of vegetable fibres were found, and loosely twisted cords of llama wool were wound round the heads of some of the mummies in the form of turbans, and many strands of the same cords, tied at both ends, were sometimes used as sashes. This seems to show that the llama was already domesticated. Mummies were wrapped in skins of animals and sea birds. The dead were laid on their backs at full length in shallow graves; the sitting posture seems to have been introduced with other Tiahuanaco influences at a later date. Bows and arrows were as yet unknown and their place was taken by spear-throwers. Their basketry was of the coiled type and frequently ornamented with intricate designs woven in with colored fibres. Uhle thinks that these decorative elements were afterwards adopted into Tiahuanaco art.

No remains of this period have as yet been found in the interior, and we may judge that this people, before they became agricultural, lived principally by fishing and hunting.

The oldest graves discovered in the interior of the region show undoubted influences of the Tiahuanaco culture, which, according to Uhle's
chronology, which we provisionally accept, flourished between 600 and 900 A.D.

For many years past objects have appeared from time to time in private collections representing this cultural period, but no serious search had been made to discover graves or cemeteries which would throw a clearer light on the matter. Periodically, during thirty years, I have been making archaeological excavations in many parts of the region without finding such deposits, and Uhle, although he has worked over a great deal of the ground, was only able to locate a despoiled cemetery at Pisagua, on the coast. However, in May of last year I was lucky enough to discover, during an exploration of the central Loa Valley, an untouched cemetery dating from this period, and was able to uncover twenty-six graves. Their contents were in general badly preserved, owing to the fact that at a later time the ground containing the cemetery had been irrigated, and the damp had penetrated the graves, destroying most of the more perishable material. But what remained showed undoubted Tiahuanaco influences. Later on, in the vicinity of San Pedro de Atacama, I came across the debris of another cemetery of the same date, although it had been completely sacked by treasure hunters. Nevertheless I was able to pick up some fragments of broken pottery which showed the same well known Tiahuanaco style in their decoration.

In this period the Atacameños had developed or acquired an incipient agriculture, and I found many ears of maize in the tombs, as also agricultural tools of wood and stone. They had also acquired a knowledge of those industries that seem to go together with agriculture. They made very good pottery, some of it decorated with painted designs in two, sometimes three colors. Spinning and weaving llama's wool were also well developed. Basketry, generally without decoration, still continued, but not to the same extent as formerly, as it was partly replaced by pottery. Bows and arrows had taken the place of spear-throwers, which had practically disappeared. Wooden snuffing-trays were occasionally found having the same shape as the stone ones found in Tiahuanaco itself, some of them bearing the same designs as those carved on the monolithic gateway and on many of the artifacts of this metropolis. Snuffing-tubes were principally of bone, as during the former period, but furnished with a wooden mouthpiece. This shows that there had been a change in the fashion of taking snuff. Instead of aspiring it directly from the deposit, the new style was used of blowing it into the nostrils of a second person, hence the mouthpiece.

Very few copper objects were found and not one of bronze. A review of the South American bronzes that have been analyzed shows that not a
single one of them is earlier than the Chincha-Atacameño period, three centuries later. Our studies on this point have been quite conclusive, and coincides with those of Jijon y Caamaño.

The burials during this cultural stage show the introduction of the Peruvian custom of entombing the bodies in a sitting posture, with the legs drawn up to the chin.

No ruins have as yet been discovered that can be ascribed to this period, so that nothing may be said of its architectural style. On the other hand, we found an extensive irrigation system, long since abandoned, in the vicinity of the cemetery at Chorillos. This may have belonged to this epoch, as the maize and stone spades found in these graves show that agriculture was practiced. The same thing occurs at Tchekar, near San Pedro de Atacama, where we found the other cemetery of the same age.

The sudden termination of the Tiahuanaco empire, the causes of which remain unknown, seems to have put an end to the wide-spread influences of the metropolis, giving place to the formation of a number of local cultures of an inferior type. This happened also in the region occupied by the Atacameños. During two centuries at least, from 900 to 1100 A.D. approximately, this people developed a culture of their own, quite different in many respects from the former, different also from those of their neighbors. Uhle calls this the period of the Atacameño indigenous culture, and was able to study it during his extensive excavations in the neighborhood of Tacna and Arica. I myself have discovered graves of this period in many places, both on the coast and in the interior. They are generally shaped like wells, but widen out below the initial shaft. The mummies are nearly always seated with their knees drawn up under the chin and the hands crossed over the shins. The greater part of them are enveloped in blankets or mantles, bound round with woolen or fibre cords, forming a mummy pack. Round the corpse are placed the bowls or jars which contained foods or drinks, also rugs, blankets, baskets, wooden objects, sea shells, and many other articles. Owing to the excessive dryness of the climate (it never rains) everything is well preserved and this makes it possible to reconstruct the economic and industrial life of the epoch.

Although the culture is fairly well developed, the industries still present some primitive traits, and the employment of stone tools and implements continues. Some Palaeolithic types still persist, mixed with others characteristically Neolithic, many of them highly polished. The pottery is coarse, but of many varied forms. A small proportion is decorated, there being two distinct styles; one with black designs on a red ground, which seems to be the older, and the other covered with a white slip on which
geometrical figures are painted in black and red. The principal designs are combinations of triangles and of stepped figures of large size which seem to have been derived from the decadent Tiwanaku style. These designs were applied in vertical bands, hardly ever horizontally. The decorated pieces are of varied shapes, but jugs with a handle on one side are the most common.

Spinning and weaving were well advanced. The natives made cloth of different textures, the blankets being very thick and coarsely woven, but the tunics were fine, of different colors, and generally decorated with black, white, and red stripes. Sashes were also common: there were two classes, one, worn by men, thin and narrow, with a diagonal weave, and the other, used by women, thicker, wider, and longer, and with a double warp and weft, which reproduced the same design in alternate colors on both sides. The textiles were mostly of llama wool, but occasionally vicuña wool was used for the finer pieces.

Agriculture and irrigation were common features. Many of the canals are still visible in sites now abandoned. Wooden spades are common in the graves, and the older type, made from a flat stone with a wooden handle, are still frequent. The ground was dug up with heavy pointed sticks, and the clods were broken up, after being turned with the spades, by massive wooden tools having somewhat the form of a scimitar.

The use of snuffing-tablets and tubes still continued but had been somewhat modified in form. The former, instead of having the square handle of the Tiwanaku style, were now furnished with a double handle rudely carved into figures of human beings or animals. The tubes were also carved from a single piece of wood and frequently had rough carvings in relief. Many other small objects, especially tubes and boxes for containing colored earths were carved out of a single piece of wood. Coiled basketry of many shapes and generally without decoration were still in general use.

The breeding of llamas must have been carried on in an extensive way, as is shown by the great number of woolen objects found. That they were used as beasts of burden may be easily gathered from the numerous large woolen double bags used for the loads and by the extraordinary number of wooden crochets employed as fasteners for the cords that held the load in place. Wooden bells probably used to hang round the necks of the leaders of the troop, are numerous, some of them of large size.

The ruins left of this period are peculiar. The ancient villages were not generally of any great size and the different habitations were grouped together in a continuous form, without either doors or windows. They were
never more than ten feet square, and the walls, built of dry stone, were seldom more than three or four feet high. Entrance was made through holes in the roof and access was by pathways along the top of the walls. The people could not possibly stand upright in these dwellings, hence it is probable that they were only used as dormitories. By day they lived out of doors, under sheds made of branches, as do their descendants of today.

About the beginning of the twelfth century, new influences entered into the Atacameño culture, which accelerated its later development and greatly changed its artistic style. These influences were introduced by the expansion towards the south of the Chinchas, who apparently overran the whole territory of the Atacameños, and probably also that of the Diaguitas inhabiting the provinces of Atacama and Coquimbo, where their influences are clearly noticeable.

After the fall of the Tiahuanaco empire, as we have said, a number of local cultures sprang up, some of which reached a high state of development. One of the most important was that of the Chinchas, a people who little by little dominated the whole of southern Peru and northern Chile. The influences of their culture extended into the highlands as far north as Cuzco and there formed the basis of the later culture of the Incas. Uhle, writing on this point, says:

In the period following that of the decadence of Tiahuanaco, the inhabitants of Chinchas and Ica had developed a regional civilization. The Chinchas were a conquering race during the period that preceded the Inca empire. What we did not know exactly was how far they extended south, although objects of their industry were known as far as the region of Arequipa. With the discoveries made near Tacna, their extension farther to the south cannot be doubted.

Near Taltil, Auguste Capdeville excavated a cemetery containing Chinchas-Atacameño pottery that does not differ from that found in Tacna.

In this manner the Inca style appears as a product of Chinchas and Chinchas-Atacameño influences fused into a new and different style.4

Throughout the whole territory, Chinchas-Atacameño cemeteries are numerous, especially in the Loa Valley. During our explorations we have personally excavated many of them, as did also Uhle.

Although the types of objects found in them are similar to those found in the last period, the style is much more elaborate and artistic. This is especially so in the decoration of the pottery. Up to now, all designs were of simple rectilinear geometrical motives. In this epoch curves appear for the first time, such as single and double volutes, spirals, meanders, hooks,

circles, etc. Other elements were square hooks, saw-tooth edges, zig-zags, crosses, ovals with enclosed figures, pyramids with numerous small steps, triangles with grecques attached, and combinations of all these elements. Groups of schematic figures of human beings and animals are frequently found amongst these combinations, especially on the bowls.

The style also differed from the earlier in three essential points: in the small size of the motives employed, in their covering the whole surface of the vessel, and in their being placed in horizontal zones or belts. The colors used were white, black, and red, the slip being generally white, occasionally red, and the designs painted in the other two colors.

A highly polished black ware, without decoration, was also made, and the two types are frequently found together.

The wooden objects were also much more elaborate. The figures forming the handles of the snuffing-tablets and tubes were real works of art, beautifully sculptured, representing human figures, animals, birds, and monsters. Many of the tablets, instead of two, have three figures forming the handle, and some of them are inlaid with small discs or squares of colored stones.

The textiles are finer and are frequently embroidered with geometrical designs or human figures. The colors are more numerous than in former periods, including black, white, brown, fawn, red, blue, green, yellow, and orange. These colors were produced in different shades.

In this period the poncho first makes its appearance. This quite disproves Montell’s theory that it only appeared with the horse after the Spanish conquest. In the Museo Nacional of Chile, we have five ponchos found in Chincha-Atacameño graves, and several others were found in Angualasto in the Argentine province of San Juan.

Real bronze appears in South America during this epoch. In earlier periods only copper was used, and up to the present there is not a single proof to the contrary. Now however bronze objects became numerous throughout the whole territory where the Chincha-Atacameño influences reached. The whole of the forthcoming evidence shows the truth of this statement, but for want of space we cannot treat the point in detail here.

This cultural stage began to decline in Peru with the conquest of the Chincha-Empire by the Incas in the middle of the fifteenth century, but continued in northern Chile till after the arrival of the Spaniards.

In the Atacameño territory the culture of the Incas left few traces. In their march to central Chile, the Incas did not occupy this region, but contented themselves with leaving a few garrisons at strategic points along the main route towards the south. As a consequence we cannot speak of an
Inca period in the Atacameño culture, and but few objects of typical Inca style have been found among their remains. It may be said, therefore, that the ancient Atacameño civilization was entirely pre-Inca, and that the Tiahuanaco stage, the local stage, and the Chincha-Atacameño stage are perfectly distinguishable one from the other and their differences in style well established.

We have not here treated of the architectural distinctions of these periods, as we spoke of them in a former article published in this journal. It is true that nothing was said of the type of building in vogue during the Tiahuanaco epoch, and that because no remains or ruins that could be ascribed to that period have yet been discovered. The differences to be observed in the two later styles are, however, quite clearly marked. The ruins of the few gabled adobe buildings found along the old highway from Bolivia to the south are the only signs of Inca occupation. Some of the older towns were occupied during two or more periods and the difference in the architecture of each epoch is to be noticed immediately.

Museo Nacional de Historia Natural
Santiago de Chile

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THE KINSHIP SYSTEMS OF THE
SOUTHERN ATHABASKAN-SPEAKING TRIBES  By M. E. OPLER

THIS paper attempts to summarize evidence pointing to the classification of the kinship systems of the Southern Athabaskan-speaking peoples into two norms, for convenience termed the Chiricahua type and the Jicarilla type. Under the first type can be listed the kinship systems of the Chiricahua, Mescalero, and Western Apache. Under the second can be listed those of the Jicarilla, Navaho, Lipan, and Kiowa Apache.

Such a classification was not possible until a relatively short time ago. Until recently, the Southern Athabaskan tribes of the Southwest, with the exception of the Navaho, were among the least known peoples of the American continent. Particularly was this true of the details of their kinship systems. When Spier, a few years ago, undertook to classify American Indian kinship structures, he could include comment concerning but one of these seven tribes.¹

During the past four years the writer conducted in the field a comparative ethnological study of four Southern Athabaskan-speaking tribes of the American Southwest: the Chiricahua, Mescalero, Jicarilla, and Lipan Apache.

These tribes speak closely related languages of the same linguistic stock. They inhabit the same general geographic region. Most aspects of their cultures, when treated comparatively, show a common core of belief, institution, and practice.

This homogeneity, this adherence to a single Southern Athabaskan pattern, is no less true of most of the facets of their social life. In each case the basic social segment is the extended domestic family with matrilocal residence, consisting of parents and unmarried children, married daughters, their husbands and children. In all these tribes a number of such domestic families tend to become associated and move together, to recognize as their base a specific landmark after which they often call themselves, and to allow themselves to be advised and directed by the most important man of their small, mobile community. This cluster of domestic families I have called the local group.

The nature of their food quest and of the territories through which they roamed precluded a strong sense of tribal unity. They were semi-nomads, following the wild food harvests in an economically inhospitable region. Their numbers were therefore few, the territories over which they ranged

extensive. The people living in one section of the tribal range would seldom meet their tribesmen of a more distant quarter. Consequently those local groups close enough to each other to form alliances for defense and aggression and able to attend each other’s social functions, signified their common interests by the formation of loose confederations which I have termed bands. The band, at a time of united action on the part of the several local groups which composed it, was led by a recognized chieftain, the most prominent of the leaders of the local groups.

![Map of Southern Athabaskan-speaking tribes](image)

**Fig. 1.** Approximate location of the Southern Athabaskan-speaking tribes of Arizona, New Mexico, and adjoining territory.

The tribe was a rather nebulous entity to its members. The band was a man’s immediate concern. True tribal names are rare for the Southern Athabaskans, but band names are the rule. The local groups of one band were sometimes distantly enough separated from the local groups of another band so that the distinction took on cultural significance. Thus one Chiricahua band, the one nearest to the Plains, had accepted a number of Plains-like traits of material culture, while the other Chiricahua bands
showed no evidence of these. Occasionally slight differences in dialect and vocabulary can be equated with band boundaries.

A survey of the practices and institutions which surround marriage and the maintenance of the family likewise reveal marked similarities for these

**CHIRICAHUA**

**MESCALERO**

**WESTERN APACHE**

U-my child

(w. ap.)

Fig. 2. Systems of the Chiricahua type: Chiricahua, Mescalero, and Western Apache. The native terms corresponding to the letters entered in these charts are given in the accompanying table. More than one letter indicates that the speaker may refer to the relative by more than one term. (Errata: In the Mescalero system, for sister's son’s child read A, for her daughter's child read C.)

several tribes. All of them practice the sororate and levirate and subscribe to identical ideologies in respect to the disposal of the mates of the deceased. A surviving spouse falls under the absolute control of his dead mate's family, whose members may force him to marry a sibling or cousin of the
Fig. 3. Systems of the Jicarilla type: Navaho, Jicarilla, Lipan, and Kiowa Apache. More than one letter or number indicates that the speaker may refer to the relative by more than one term. (Errata: In the Navaho system, for father's brother read G, 2; in the Jicarilla system, for sister's child read O', O; in the Lipan system, for all members of the grandchild generation read C; in the Kiowa Apache system, for brother's son read V.)
<table>
<thead>
<tr>
<th>Chiricahua</th>
<th>Mescalero</th>
<th>Western Apache (White Mountain Group)</th>
<th>Navaho</th>
<th>Jicarilla</th>
<th>Lipan</th>
<th>Kiowa Apache</th>
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<td>C*</td>
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<td>E†</td>
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(little used see 2)

| F | cídè'dé' | cídè'dé' | cídè'dé' | cídè'dé' | cídè'dé' | cídè'dé' |
| G | cibé'jé' | cibé'jé' | cibé'jé' | cibé'jé' | cibé'jé' | cibé'jé' |
| H | cimá' | cimá' | cimá' | cimá' | cimá' | cimá' |
| I | cívóyé | cívóyé | cívóyé | cívóyé | cívóyé | cívóyé |
| J | ciká' | ciká' | ciká' | ciká' | ciká' | ciká' |
| K | cikís | cikís | cikís | cikís | cikís | cikís |

(archaic)

| L | cilá' | cilá' | cilá' | cilá' | cilá' | cilá' |
| M | cívé' | cívé' | cívé' | cívé' | cívé' | cívé' |
| N | ciyátcè' | ciyátcè' | ciyátcè' | ciyátcè' | ciyátcè' | ciyátcè' |
| O* | cidadí | cidadí | cidadí | cidadí | cidadí | cidadí |
| O* | cidadí | cidadí | cidadí | cidadí | cidadí | cidadí |
| P | cidjé | cidjé | cidjé | cidjé | cidjé | cidjé |
| Q | citsí' | citsí' | citsí' | citsí' | citsí' | citsí' |
| R | szé'dé | szé'dé | szé'dé | szé'dé | szé'dé | szé'dé |
| S | cínhá'á:c | cínhá'á:c | cínhá'á:c | cínhá'á:c | cínhá'á:c | cínhá'á:c |
| T | cíjá' | cíjá' | cíjá' | cíjá' | cíjá' | cíjá' |
| U | citcái | citcái | citcái | citcái | citcái | citcái |

| V‡ | citcái | citcái | citcái | cijá' | cijá' | cijá' |

* C and C', O and O' are related terms.
† The Jicarilla variant of E looks quite different due to the t-k shift.
‡ V, where it occurs is often used as an alternative to M, "my son," and in systems of the Jicarilla type to differentiate between one's own son and the son of the sibling of the speaker's sex. For simplicity it has not been used in any diagram except Kiowa Apache and there because M has not been recorded for this tribe.
deceased, or may dismiss him at will. Sororal polygyny was permitted by all of these tribes except the Lipan.

It was not unnatural to expect, in view of the common denominator which emerged wherever comparison was attempted, that the kinship systems of these groups would prove to be varieties of a single type. Astonishingly enough, as has already been stated, the evidence indicated the presence of two distinct types of kinship structure for these peoples. In what follows we shall attempt to delineate the salient features of these kinship types and to classify the kinship systems of the seven Southern Athabaskan-speaking tribes with regard to them.

THE CHIRICAHUA TYPE

A. Chiricahua Kinship System. In Figure 2 is diagrammatically represented the Chiricahua Apache kinship system, which is the theoretically ideal form of one of the two types of kinship structure which we shall encounter in our survey of Southern Athabaskan kinship. For convenience this system, or a variation of it, shall be called the Chiricahua type.

The principal features of the Chiricahua kinship system are as follows:

1. All terms except those expressing parent-child relationship are self-reciprocals; a Chiricahua addresses his relative by the same term which that relative has used in speaking to him.

2. There are four separate terms, one for each grandparent, used as self-reciprocals for grandchilden. A grandparent’s siblings, regardless of sex, are classified with the grandparent.

3. The mother’s siblings are classified under one term. There is a separate term for mother.

4. The father’s siblings are classified under one term. There is a separate term for father.
5. There are separate terms for stepfather and stepmother.

6. No terminological distinction is made between siblings, parallel cousins, or cross-cousins. In ego's generation two terms are used self-reciprocally, one which means sibling, parallel cousin, or cross-cousin of the same sex as the speaker, the other which is addressed to sibling, parallel cousin, or cross-cousin of the opposite sex from the speaker. There are no regular terms which indicate age distinctions in ego's generation.

7. Only true children are called son and daughter.

8. The behavior patterns observed in respect to blood kin designated by these terms are free from joking relationships. Reserve and respect characterize the relationship between siblings or cousins of opposite sex. Upon mutual agreement, cousins of opposite sex (cross or parallel), but not siblings, may practice total avoidance.

B. Mescalero Kinship System. The essentials of the Mescalero Apache kinship system are given in Figure 2. It is evident at a glance that it has much in common with the Chiricahua system and can be classified as a variety of the Chiricahua type. Except in the few details which will be summarized here and in the matter of secondary usage which will also be explained, the notations made for Chiricahua kinship apply to Mescalero kinship as well.

In following the kinship chart, it will be noticed that the Mescalero distinguish the siblings of the grandparent according to sex, whereas the Chiricahua do not. Thus, father's father's sister is classified with father's mother and not with the father's father as the Chiricahua usage would have it, etc.

Among the Mescalero the mother's sister can be treated terminologically in one of three ways. She can be addressed by the same term as is used for the mother's brother. That, we remember, is the Chiricahua usage. But more often she is addressed by another term, the term that is also used for stepmother. Third, and this is a less common and almost secondary usage, she may be classified with the mother. The last practice, while not of great importance for the Mescalero, is of decided interest to us; it foreshadows a tendency (the merging of parent and his sibling of the same sex) which becomes the rule among the Jicarilla, Lipan, and Kiowa Apache. This tendency is likewise present in the classification of the father's brother. This man may be classified with the father's sister or with the father. Moreover, when the father's brother and mother's sister are classified with father and mother respectively, the terminology in the first descending generation undergoes a change. The children of siblings or cousins of the same sex are then called son and daughter.
THE JICARILLA TYPE

A. Jicarilla Kinship System. Field-work among the Jicarilla Apache brought to light the details of the second type of kinship system to be found among the Southern Athabaskans. Figure 3 indicates its prominent characteristics. Its outline will be recognized as the conventional Dakota-Iroquois system of the literature. For the purpose of this paper I shall designate it and its variations as the Jicarilla type system.

The distinguishing features of the Jicarilla Apache kinship system follow:

1. There is no self-reciprocal terminology with one exception. Occasionally the term for mother’s brother is used by him as a reciprocal for sister’s child.

2. There are two grandparent terms, one for the grandparent of each sex. We also find a separate grandchild term.

3. The mother and her sister are classified together. There is a separate term for mother’s brother.

4. The father and his brother are classified together. There is a separate term for father’s sister.

5. There are terms for older brother, older sister, and younger sibling.

6. Parallel cousins are siblings. There is a separate term for cross-cousin and an additional term for cross-cousin when male is addressing male.

7. Children of siblings or cousins of the same sex are called son and daughter. Children of siblings of the opposite sex are addressed by other terms. The practice, characteristic of systems of the Dakota-Iroquois type, of addressing relatives of the first descending generation according to the sorting of the sexes in the speaker’s generation, prevails.

8. Jicarilla kinship is marked by a number of joking and restraint relationships. Grandparents and grandchildren joke in a coarse vein, with much allusion to sex and mating. On occasion the grandparent acts as teacher and strict disciplinarian to the Jicarilla child.

Between avuncular and nepotic relatives a joking relationship based on good-natured rivalry obtains.

To siblings of the opposite sex a feeling of reserve is felt. A cross-cousin of the opposite sex may be avoided entirely; and if the avoidance does not take place a somber “joking” relationship is inaugurated instead, in the course of which each accuses the other of sorcery.

With a cross-cousin of the same sex a joking relationship exists which is marked by the pranks played at the other’s expense and the liberties indulged. The cross-cousin baiting went as far as mate stealing.
B. Lipan Kinship System. The Lipan Apache kinship system, which is outlined in Figure 3, differs from the Jicarilla system in only one particular: cross-cousins are not distinguished from parallel cousins and siblings. For the Lipan all three are addressed by sibling terms, and are older brother, older sister, or younger sibling. The freedom with the mate of the cross-cousin of the same sex, which we observed for the Jicarilla, takes the form of jesting about sex and marriage with the mate of any relative of the same generation and sex among the Lipan.

SUMMARY FOR THE NEW MEXICAN APACHE

At the conclusion of the writer's field work among these four tribes, it was clear to him that not only did he have data concerning two different types of kinship structure, but that the groupings and processes involved in each were based on contrasting principles, point for point and generation for generation. The terminology of the Chiricahua type is self-reciprocal; that of the Jicarilla type is not. The Chiricahua type is marked by four grandparent terms; these tend to be reduced in number in the systems of the Jicarilla type, and in the latter a single term for grandchild is the rule. The Chiricahua type tends to merge the siblings of the parent and separate the parent; the Jicarilla type tends to merge the parent and his sibling of the same sex and to distinguish the parent's sibling of the opposite sex. In the Chiricahua type no age distinctions in ego's generation are observed; for the Jicarilla type such distinctions are always made. Children of siblings of the same sex are not ordinarily termed son and daughter in the Chiricahua type; in the Jicarilla type there is the marked tendency to so call them. The systems of the Jicarilla type are accompanied by an unusual development of joking relationships; those of the Chiricahua type are not.

WESTERN APACHE KINSHIP SYSTEM

(White Mountain, Northern and Southern Tonto, San Carlos and Cibecue Apache)

A chart of Western Apache kinship is given in Figure 2. It clearly belongs to the Chiricahua type. All the terminology is self-reciprocal. Mother's parents and their siblings are called by one term, but the terms for

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2 The Western Apache data cited in the paper are from unpublished writings of Mr Grenville Goodwin, who has for several years been doing ethnographic field work among the Western Apache. Correspondence with Mr Goodwin has also been utilized. Western Apache kinship shows but slight variation throughout its range, and for the sake of simplicity the White Mountain Apache usage is followed here.
father's parents and their siblings are grouped in the Mescalero manner. For the first ascending generation the classification follows the most common Mescalero usage. Father's brother and sister are called by the same term. There is a separate term for father. Mother's sister is referred to by a term that also means stepmother. There are separate terms for mother and mother's brother. The term for stepfather is the one also used for father's siblings. As is true for the Chiricahua and Mescalero only actual children are called son and daughter.

It is in ego's generation that the greatest difference between the Mescalero-Chiricahua systems and the Western Apache system is noticeable. In the latter, age distinctions among siblings are recognized; there are terms for older and younger sibling. Cross-cousins are distinguished from parallel cousins and siblings, cognates of the Jicarilla cross-cousin terms being utilized to designate these relatives. One unique feature, recorded only for the Western Apache and Navaho among the Southern Athabaskans, is the difference in terms addressed to children by each parent. The Western Apache father uses a term for son and another for daughter. The mother addresses her child of either sex by a still different term.

The Western Apache do not practice the joking relationships which typify the Jicarilla type systems. For them joking is a clan matter and is directed towards the members of the father's clan. (The Western Apache and the Navaho are the only Southern Athabaskans with sibs, both having matrilineal sibs.) The Western Apache act with great reserve in the presence of siblings or cousins of the opposite sex but do not avoid the latter. Though cross-cousin terminology is found, there is an absence of the behavior pattern which is associated with this terminology among the Jicarilla.

**NAVAHO KINSHIP SYSTEM**

The Navaho kinship system, an outline of which appears in Figure 3, is in many ways the most interesting of the entire series, for it looks to both of the types we have described for its terminological classifications. At a number of points the Navaho is permitted a choice of classifying relatives either according to the Chiricahua type system or the Jicarilla type system. In those cases the best and most common usage seems to follow the Jicarilla type pattern, and accordingly it is with the Jicarilla type that I have

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*In addition to the published material on the Navaho, the writer has had the benefit of correspondence with Dr Walter Dyk on the subject of the paper and has very kindly been given access by Dr Edward Sapir to his recently obtained Navaho kinship and genealogical material.*
classified Navaho kinship. Moreover the behavior patterns belong to the Jicarilla type.

There is relatively little self-reciprocal terminology in Navaho kinship, a factor which adds further weight to its Jicarilla type identification. There is just enough self-reciprocal terminology, however, to prove suggestive of a former Chiricahua type orientation. There is a term that is used for father’s parents and their siblings and reciprocally for sons’ children. There is another term for father’s siblings which is used reciprocally for brother’s child. Most interesting of all is the term for mother’s father, which is, on occasion, used reciprocally for daughter’s son, man speaking. This follows the Chiricahua type pattern of having a single term used for grandparent and reciprocally for the suitable grandchild. But the Navaho also have a separate and more frequently invoked term which means daughter’s child of either sex and acts as the reciprocal of mother’s father. This last and favored usage follows the Jicarilla type system.

The position of the father’s brother and mother’s sister will illustrate the contrary tendencies of the Navaho system in respect to the two kinship types we are considering. The father’s brother may be classified with the father’s sister (Chiricahua type) or with the father (Jicarilla type). If the former mode is chosen the term is used reciprocally for brother’s child (Chiricahua type). Should the latter practice be invoked, the brother’s children, man speaking, will be addressed as son and daughter (Jicarilla type). The classification of father’s brother with the father seems to be the usual Navaho practice.

In like manner the mother’s sister may be classified as mother, and then the sister’s children (woman speaking) are called son and daughter (Jicarilla type). Otherwise the mother’s sister is addressed by a separate term and she addresses her sister’s children by still other terms. (Note the departure from Chiricahua type self-reciprocal terminology here. When the Mescalero and Western Apache use this very term for mother’s sister they retain it as a self-reciprocal and address it to the sister’s child.)

In the speaker’s generation the Navaho system closely follows the distinctions observed in the Jicarilla system. Parallel cousins are siblings. Siblings are distinguished as to age and sex. There are separate terms for cross-cousins.

A grandparent-grandchild joking relationship has been mentioned in the literature on the Navaho.4 It is certain that the Navaho share the

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avuncular-nepotic and cross-cousin joking relationships which have been mentioned in connection with systems of the Jicarilla type.6

One point of interest, though outside the scope of this article, should be mentioned here. It is the influence which the recognition of lineages was beginning to exercise upon the terminological separation of generations. Dr Sapir’s genealogical data indicate that females of both the mother’s lineage and the father’s lineage, in spite of generation differences, can be classified together under the regular Navaho term for mother. Relatives so classified included father’s sister, father’s sister’s daughter, and, besides mother’s sister, a man’s sister’s daughter and his mother’s sister’s daughter.

KIOWA APACHE KINSHIP SYSTEM

The last Southern Athabaskan kinship system we have to place is that of the Kiowa Apache, outlined in Figure 3.6 To allocate this kinship system is not a difficult task, for with the exception of the grandparent-grandchild relationships, which are all covered by one self-reciprocal term, the system is identical with that of the Lipan. It clearly belongs to our Jicarilla type. One minor variation reminiscent of Chiricahua type practice should be noted. The term for mother’s brother is occasionally used as a self-reciprocal for sister’s child, man speaking.

In behavior patterns the Kiowa Apache fall neatly into the Jicarilla type. The joking relationship between grandparents and grandchildren is present, and its elements are those which have been described for the Jicarilla. The joking relationship between avuncular and nepotic relatives is in force. Restraint in relations toward siblings of the opposite sex is the rule. Freedom with a sibling of the same sex is practiced, and takes the form, in the case of men, of sexual privileges with each other’s wives.

So far as the writer knows, the Jicarilla and the Kiowa Apache are the only Southern Athabaskan tribes to use blood-kin terms to the mates of relatives in the first ascending generation. For both of these tribes the groupings and the very terms applied to these affinities are much the same.7

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6 Ibid., pp. 72–73, and correspondence with Dr Walter Dyk.
7 M. E. Opler, A Summary of Jicarilla Apache Culture (American Anthropologist, Vol. 38, pp. 202–23, 1936). The eastern distribution of the use of blood-kin terms for affinities among the Southern Athabaskans, and the fact that the usage is limited to tribes having the Jicarilla type system, suggest that the proximity of groups with Dakota-Iroquois type kinship systems may have been important for the development of the Jicarilla type system among the Southern Athabaskans.
CONCLUSIONS

If the reader will follow the kinship charts as they have been arranged in Figures 2 and 3, he will note that they offer a series of transitions from the Chiricahua to the Jicarilla type. Mescalero kinship shows a slight tendency of choice or variation in the direction of the Jicarilla system. In the Western Apache system the tendencies toward the Jicarilla type are more pronounced, especially in ego's generation, but the balance is clearly with the Chiricahua system. Navaho kinship falls just the other way. It has a number of points of resemblance with the Chiricahua type, particularly in terms of secondary usage or alternative usage, but its strongest affiliations are with the Jicarilla type. From this point on, as shown in our diagrams, the systems are solidly of Jicarilla type character.

It may be asked whether this logical or theoretical sequence appeals to the writer as the clue to the actual developmental process. Is the Chiricahua type system, or something like it, the older form of Southern Athabaskan kinship? With the differentiation and dispersion of the Southern Athabaskan tribes has a development in the direction of Jicarilla type kinship occurred? There is much comparative material of a linguistic and ethnological nature to be sifted before a confident answer can be rendered; only a guess can be hazarded at this moment. That guess, in the writer's opinion, would be in the affirmative.8

It is significant that the most isolated and least differentiated Apache tribes, the Chiricahua and Mescalero, the ones least affected by the culture of the surrounding areas and tribes, and the ones which presumably have had the best opportunities to preserve older elements, are the exemplars of the Chiricahua type system. Within the kinship systems there is internal evidence which tends to support the same conclusion. In Chiricahua kinship there is no trace of secondary usage suggestive of the Jicarilla type system. If the process of change had been the other way, if the Jicarilla type system is basic and the Chiricahua type system a modification of it, we must accept the view that all traces of Jicarilla type classification have been eradicated from Chiricahua kinship. But no such "disappearance of the useful arts" need be posited for the developmental trend we have suggested. In every system of the Jicarilla type there are to be found, as secondary and alternative choices, classifications of relatives which agree with the principles of the Chiricahua system.

8 No attempt is made in this paper to decide whether the process of development towards a Jicarilla type system, by the tribes which shared it, is to be viewed as an independent, inner growth or as a result of contact with tribes to the east and north. Only the direction of the development and not its probable causes are treated in this short summary.
There is another interesting interpretation which is suggested by the identity of the tribes which fall together into the same kinship type classification. The Mescalero, Chiricahua, and Western Apache territories formed a solid phalanx of boundaries which stretched from the Pecos west into Arizona. The kinship systems of these three tribes fall under one heading, as we have seen. Around the contiguous territories of these three tribes are fringed the ranges of the four other tribes, whose kinship systems fall together into the opposite category. To the north are the Navaho, east of them the Jicarilla, farther east in the plains, the Kiowa Apache, and southeast the Lipan. The ethnographic evidence is too lengthy to dwell on here, but in his field work the writer found remarkably abundant and consistent parallels between the Jicarilla and Navaho in every aspect of culture. Again, when he came to study the Lipan, he found that their myths, terms for cultural artifacts, etc., as well as their kinship system, showed much closer relation to Navaho and Jicarilla counterparts than to those of the Chiricahua and Mescalero. Moreover, the Jicarilla, Lipan, and Kiowa Apache dialects share a phonetic shift from an aspirated $t$ of other Athabaskan languages to an equally aspirated $k$.

The conclusion from ethnographic evidence seems inescapable that there was an early dichotomy of Southern Athabaskans, and that the Navaho, Jicarilla, Kiowa Apache, and Lipan on the one hand, and the Western Apache, Chiricahua, and Mescalero on the other, are the fruits of further differentiation and movement after such a division. The cultural evidence for such a dichotomy makes intelligible the existence of the two kinship types. The implication seems to be that the impetus toward the development of the Jicarilla kinship type occurred largely after the division of the proto-Southern Athabaskan-speaking peoples into two main bodies, but while the tribes which now use the Jicarilla type system were still much more closely connected than was the case at the time of first white contact.

**Office of Indian Affairs**

**Albuquerque, New Mexico**
THE FEMALE SHOOTING LIFE CHANT;  
A MINOR NAVAHO CEREMONY

By LELAND C. WYMAN

IN the interest aroused by two major Navaho ceremonies, the Night Chant and the Mountain Chant, the far more numerous minor chants which are performed chiefly throughout the summer have been neglected. References to isolated practices in these chants may be found in various places, especially in the Ethnologic Dictionary of the Franciscan Fathers; but as a whole they have been described only in a few instances and these in semi-popular literature. The small chants are held far more often than the larger ceremonies, there being hardly a day during the summer when one cannot find one or more of them going on somewhere within driving distance. They are almost a part of the everyday life of the Indians, while the larger chants are more in the nature of special spectacles or occasional treats. An analogy in our own culture might be weekly church attendance with its intimate effect on our daily lives as compared with occasional attendance at a Christmas or Easter festival at some large cathedral in a more or less distant city. The minor ceremonies, therefore, offer a field of study which contains much ethnologic material bearing on the psychology of the Navaho people. A concrete example of this point is the fact that in ordinary conversation with the Indians there is frequent reference to the minor "sings" or to circumstances attending them, while reference to the major chants is much more rare. A family is usually going to have a "sing," or is having one, or has just had one, and the character, purpose, and probable consequences of the ceremony are freely discussed.

The writer was the patient during one of these smaller chants, the Female Shooting Life Chant (na'at'oe· ba'adji i'nádji há-tá'), which was

1 Grateful acknowledgment is hereby made to the Chemical Foundation of New York for financial assistance with a study of Navaho medicine, of which this paper is a part. The writer is also indebted to Dr F. H. Pratt of Boston University, Mr Clyde Kluckhohn of Harvard University, and Dr Gladys Reichard of Barnard College for constructive criticism and assistance in preparing the manuscript.


3 The group of chants known as i'nádji há-tá'l or life chants, and in which certain combinations of plants called i'nádji aze or life medicine are used (cf. Ethnologic Dictionary, p. 114), is an appropriate treatment for old sprains and strains or for pain anywhere in the body resulting from broken bones or any other concomitants of accidents, either recent or past. According to informants in the area studied there are six life chants, the male and female shooting (na'at'oe· bika'dji and na'at'oe· ba'a'dji), the male and female knife (be-ce· bika'dji and be-ce· ba'a'dji), and the male and female kase (kase bika'dji and kase ba'a'dji). The Franciscan Fathers have translated kase as "feather-shaft," saying that it is often designated as "béšhe, or knife
held early in August, 1935, in the southeastern part of the Eastern Navaho Jurisdiction, near Pinedale, New Mexico. The chanter (hatá·li) was an English-speaking man of middle age, who later willingly recounted and explained all the details of the ceremony. Two years previously, while visiting him, I had suffered from an attack of bursitis resulting from an old gun-shot injury to the knee. At that time he offered to try his medicine, and bandaged the affected knee with a moistened mass of ground dried herbs (i·nádzi aze'). The lameness disappeared a few hours after this treatment (as it had done before without treatment), and he informed me that according to the Navaho code it was then my duty to employ the hatá·li who had made the "cure" to "sing over" me. This would not only be one way of paying the doctor for his successful treatment, but the ceremony would prevent a recurrence of the malady and would make the flesh and bone healthy just as it was before the accident. The chant could take place at any time following the "cure."

PRELIMINARIES

The chant took place in the house (ho·γan) of the medicine man, the patient's own "modernized" ho·γan being unsuitable because of the lack of a smoke hole and central fireplace. The patient, having arrived on the scene during late afternoon, first gave the basket, the calico, and the fee for the medicine man to an intermediary (anyone present) to be delivered to the singer in private, and made arrangements for feeding the guests.

chant, or ináji, life chant" (op. cit., p. 364). My informants, however, translated kase as "stalk, like the stalk of a plant," and stated that it is distinct from the knife chant and one of the group of life chants. Such classifications of Navaho chants as have been published differ (op. cit., p. 361) and it is probable that regional differences may be found. An attempt to construct a satisfactory classification is in progress.

The male and female shooting varieties also occur in the group of diyink'e hatá·l or spirit chants, but these are appropriate treatments for sicknesses arising from the malign influence of lightning. The male and female branches of each chant differ somewhat in the songs and in the minor details of procedure (e.g., a smaller reed in the arrow fetish for the female chant), but otherwise they are essentially the same.

* Unless otherwise stated all the information given in this paper was obtained from this informant or from informants living in the area mentioned and in agreement with him. General statements, therefore, should not be interpreted as tribal generalizations but as characteristic only of the region studied or of the hatá·li practicing therein. The possibility of regional or individual variations in Navaho ethnology must be recognized and has not been emphasized sufficiently in past writings.

The past tense is used when describing events observed by the writer during the ceremony and the present tense when stating information obtained from informants at other times.

* The patient is obliged to supply the following things: a fee for the singer (either in money or in goods; this may be from ten to fifty dollars depending on circumstances), a number of
About six P.M. he entered the ho·γan and sat at the west side, while an assistant⁸ (any participant), seeing that he was dressed properly, brought in and laid out upon one calico spread the fetishes for the altar⁷ on the west side of the ho·γan at the right of the patient.

The only requirements of costume other than ordinary clothes were a head-band, a turquoise and shell bead necklace, and moccasins. The head-band is worn so that the diiyin diné'é will recognize the wearer as the patient. It shows that "he belongs to this chant," but if he wore a hat he would be a stranger to the spirits and the medicine and prayers would not be effective. The medicine man always wears a head-band during a chant for the same reason (recognition). Shell and turquoise beads and moccasins are worn because they were made for the Navaho in the beginning; they are "their own" and must not be lost. In the ceremony they mark the wearer as of "the people."

In this instance moccasins had to be borrowed; so the assistant loaned his, but first placed corn pollen in them. This was because he had worn them during or after other chants that the patient had not had: the pollen would prevent the bad influences from these chants and from his previous illnesses from attacking the new wearer. The diiyin diné'é, upon seeing the pollen, would know that the moccasins were borrowed and would be careful.

The patient was instructed to observe certain restrictions for the duration of the ceremony: he was not to break bones or sticks, not to put his fingers in blood, and when turning or walking around anything he was always to turn or go from left to right, i.e. sunwise. For instance, in entering the ho·γan, which faces east, he must pass to the south of the central fire and upon leaving he must pass to the north, so as to encircle it. Sexual

pieces of calico which will be used in the altar and later serve as gifts for those who assist with the ceremony (approximately a dozen pieces of from four to six yards each, again depending on the affluence of the patient), a medicine basket to be used in the bath, and sufficient food-stuffs to entertain all the expected guests.

⁸ Here and hereinafter the term assistant refers to any able man who may be asked by the medicine man to perform some service. There are no regular assistants, but the singer asks anyone present to do certain chores, often choosing relatives, apprentices, or other medicine men who may be visiting. Different men are selected for different tasks, without any rule. He instructs these men as they perform the designated tasks.

⁷ The term "altar" is used throughout as a convenient designation for the collection of fetishes, medicine sacks, and other ceremonial equipment which is usually laid out near the seats of the hátá·li and of the patient in any chant. It is not strictly an altar since it is not primarily a place of sacrifice, but usage has made the term understandable in this connection. It really is a place for sacerdotal equipment arranged in proper order and orientation.
continence is also required during a ceremony, but the hatá·li knew that the patient understood this.

After these preliminaries, some of which would be omitted in the usual course of events where the medicine man comes to the ho·γan of the patient, the patient was free to do as he wished until the evening ceremony.

THE ALTAR

The fetishes which were laid upon the spread of calico constituting the altar will be described in order from north to south (butts pointing eastward).

1. tsin nídiní—bull roarer (wood that makes a noise). It is laid with the point towards the east. It is made of lightning-struck oak, covered with burned t'cél dílyésí (Gutierrezia diversifolia Greene) and pitch. The face (eyes and mouth) of turquoise show that it is alive. It was given to the people by the lightning spirit. Its sound represents thunder. It is used to chase away evil spirits (t'cì'índí). It is not whirled in the i'nádíí hatá·l, but its presence on the altar shows that there is danger in the vicinity for evil spirits.

2. k'a—arrow. A length of reed (lók'a·'), about eight inches long, filled with dirt from a buffalo track, dirt mixed with buffalo blood (from where a buffalo has been killed), and male and female bluebird and yellow-bird live pollen. This is corn pollen shaken from the feathers of these living birds. The top end is decorated with feathers and beads. There are two of these, male and female. The male is distinguished by having a turquoise bead or a stone arrowpoint tied to it (usually both), and the female by white shell beads. One reed must come from the east and the other from the west. Their disposition will be mentioned later. In the early days the diyin diné'ë used these as arrows. Spirit man used the male arrow and spirit woman the female. There were four arrows, but the diyin diné'ë gave two of them to the Navaho and kept the other two.

3. gíc—digging stick. An oak stick, about eight inches long, shaped like a spatula at one end and decorated like the arrows at the other. Male and female, marked as stated above. The diyin diné'ë used to dig out the roots of medicinal plants with these. There were also four of these, the Navahos receiving two.

4. aze· bid'dítí—medicine stick. Eagle feathers, wound from butt to

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9 Compare the description of crane bills (ibid., p. 407).
10 Cf. ibid., p. 407.
11 Cf. ibid., p. 407: "azé dádlítqíl'—medicine spoons."
tip with buckskin and then with colored wool yarns, and decorated with beads. Male and female, marked as above. The colored yarn is for decoration only, not being used before acculturation. It is used to protect the medicine in the medicine cup. There were four of these also, as before.

5, 6, 7. The arrow, digging stick, and medicine stick of the opposite sex from those occupying positions 2, 3, and 4. When the patient is a male, the male fetishes come first; if a female, the female fetishes precede.

(The decorations of colored yarns, beads, etc., on the three types of fetishes just described are not essential. They are purely decorative, and some of them may be gifts from grateful patients which are added from time to time.)

8, 9, 10, 11. elt'ló—live feathers (it is tied on). Fluffy eagle feathers, from living birds, with the butts wound with buckskin containing live pollen. Two male and two female, designated by turquoise beads and white shell beads. Formerly tied in the hair of patients by the diyin diné'é and so used today by the Navaho, to designate the patient. The male fetishes are for male, the female ones for female patients. One pair are used in the life (i'nádjjí) chants and the other in the spirit (diyink'é) chants. The former pair are marked on the ends with a little of the red paint employed in the life chant, this marking being done on every such occasion before the patient is painted; while the latter pair are similarly marked with the white paint used in the spirit chants. In a life chant the pair pertaining to it occupy positions 8 and 9, the order of sex depending on the sex of the patient, as above, while the other pair occupy positions 10 and 11. In a spirit chant this order is reversed.

12. tsitsos—whistle. Made of eagle bone, and decorated as are the other fetishes. The diyin diné'é used to blow this whistle. It is blown today in the diyink'é hatá'l, and when the diyin diné'é hear it they know that a ceremony is in progress. It is not blown in the i'nádjjí hatá'l but it must be on the altar. Blowing or not blowing it is a mark of distinction between the two classes of chants.

13. Some live eagle feathers. Two only are essential. These are used to sprinkle medicine on the patient in order to cool a fever.

14. besistódí—fine shaped arrow points (chipped flint). All the prehistoric stone knives or spear-points that the medicine man can obtain or can conveniently carry in his pouch. The diyin diné'é used to use these

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12 Cf. ibid., p. 411: "the charm."
13 Cf. ibid., pp. 403, 511.
14 Cf. ibid., pp. 406, 411.
as weapons. Their presence on the altar serves to scare away evil spirits (t’ci’índi).

All of the fetishes with wrappings also contain ground medicines, thus adding to their curative properties.

In practice the sacks of medicines and pollens are usually placed at the southwest corner of the altar, the grease paints at the south side, and the medicine cup at the southeast corner.

FIRST BIT’LE (NIGHT CEREMONY)

About nine p.m., all having gathered in the ho-γan (hatálí, patient, families, and guests), the house was consecrated.\textsuperscript{18} An assistant\textsuperscript{7} rubbed some corn meal on the four lower roof beams: first the east beam, then south, west, and north; and sprinkled a handful sunwise around the ho-γan. This is so that the diyin diñe’é will know that this ho-γan is to be the scene of a ceremony. They say to one another: “Let us all come together and go to this place, to see what is going on and to see if things are being done correctly.”\textsuperscript{18} White corn meal is used if the patient is male, yellow if female; if neither is available corn pollen may be used.

The seating arrangement was the customary one for any Navaho occasion. Since the altar lies near the wall at the west side of the ho-γan, the medicine man sits to its right, the patient to its left. Visiting medicine men or others who wish to help with the ceremony sit at the right of the hatáli. The male onlookers occupy the rest of the south side of the ho-γan and the women and younger children the north side. The number of visitors is entirely a matter of circumstance. If it is known that much calico has been given by the patient many individuals, both men and women, who know the songs will come to help sing and possibly receive a present of calico for their services. The amount of food to be dispensed is also a factor. So it happens that the number of visitors at one of these smaller ceremonies may be anywhere from two or three to one or two hundred. (The curiosity aroused by the fact of a white man having a “sing,” plus possibly the ex-

\textsuperscript{18} Cf. \textit{ibid.}, p. 329.

\textsuperscript{18} These chants were taught to the Navaho in the beginning by the diyin diñe’é, the holy people. When one of them is held today, the diyin diñe’é assemble at the beginning of the ceremony to watch and to listen. If something is done incorrectly or if some of the songs are omitted the holy people are offended. They say: “That chant is ours. This man did not learn it properly, or perhaps he did something wrong on purpose.” In that case they do not help to cure the patient. If the ceremony is correct, however, they are pleased and the patient is cured. Much of the procedure, therefore, is an attempt to follow that as described in the origin-legend of the ceremony.
pection of much food and calico, resulted in the presence of about a hundred Navaho visitors at the ceremony described.)

After the ho'γan was consecrated the medicine man made sure that the fetishes on the calico spread were in the proper order and turned the spread so that the butts of the fetishes were towards the patient. This was so that the diyin dine'ė would know that the man toward whom they pointed was the patient. Medicine was next prepared.

An abalone shell was made steady on a little heap of earth before the altar, with its lip to the east. It was filled with water, and dry powdered herbs from two small sacks were sprinkled on the water. This was stirred sunwise with the butt of the male medicine stick (aze' bidádatf), and tasted twice from the tip of the stick by the medicine man. Then the stick was laid across the shell with its butt pointing east.\(^{17}\)

An abalone shell (ditcili letsá—abalone bowl) or a turtle shell (djediya'hi bitsá—turtle box) is used as a medicine cup in most chants. In ancient times on this earth these animals were like people; they were diyin dine'ė. They used to chant, and when they went away they said: "As long as the Navaho live on this earth they shall use my shell for taking medicine."

The turtle shell should be used in all i'nádjí hatá'l, and the abalone shell in diyink'ė hatá'l only. However, if only one kind of shell is owned by the medicine man he may use it indiscriminately until he has the other prepared. Such was the case here.

Either shell is placed with the head end to the east, because the earth and sky face the east.

The medicine was i'nádjí aze' (life medicine), which consists of several plants, dried and ground together. This medicine is a common one, regarded as a general tonic and pain killer. It may consist of a large number of different plants, but there are certain basic ones which are necessary. These are le aze'—earth medicine (Eriogonum alatum Torr.), aze' hat'cini—black medicine (Lithospermum multiflorum Torr.) aze' litcf—red medicine (Lithospermum incisum Lehms.), aze' likai—white medicine (Oenothera Hookeri Torr. and Gray), aze' sakuz—cold medicine (Gaura parviflora Dougls.), and nibeyándi—winding plant (Eriogonum racemosum Nutt.). In an emergency two or four of these may be used, but it is better to have all six. Any others which belong to the class of i'nádjí aze' may be added if they are at hand.\(^{18}\)

Both sacks contained the same kind of medicine, "one sack for the medicine man and one for the patient." Two are kept in readiness, because if

\(^{17}\) Cf. Ethnologic Dictionary, p. 407.

\(^{18}\) Cf. ibid., pp. 114, 403.
the medicine man is singing over one patient, and he improves, the singer may be obliged to start another chant over another patient elsewhere; in which case he leaves one sack of medicine with the first patient and takes the other to the second patient.

The medicine is stirred with the aze· bidádatí (the male one for a male patient), so that the power of the fetish may be transferred to the medicine.

The medicine is tasted by the medicine-man because the diyín diné'é are watching to see what kind of medicine is given. If the medicine-man is willing to taste his own medicine the spirits will know that it is the right kind.

The aze·bidádatí is laid over the medicine cup until it is used, to protect it, so that bad influences will not get into the medicine ("so bad spirits will not spit in it, or drop something into it").

While the medicine is soaking the chanting is begun. Anyone sings who knows how; the greater the number of singers the more effective the cure. The singing is accompanied by shaking buffalo-hide rattles. These are oblong, contain pieces of the five precious stones, have a fluffy eagle feather attached to each upper corner, are decorated on each side with two parallel zig-zag lines representing lightning and several arrangements of holes representing certain constellations, and are fitted with a handle of oak or cedar wood which is decorated at the proximal end with either buffalo hair or beaver fur and porcupine quills and at the distal end with buffalo tail hair. Such rattles must be made during a chant. As many rattles may be used as are available. (Four were used here.) The medicine man shakes one and the others are usually shaken by visiting medicine men, although they may hand them over to any male visitor when they are tired. Sometimes they give one to a small boy who shows interest in learning how to sing.

The first arrow chant was sung (k'a biyi'n). There are three arrow chants, and if the medicine man conducts several of these ceremonies in succession he uses the first, second, and third in rotation on separate first nights.

During the thirteenth song of the chant the medicine man gave the medicine to the patient, holding the abalone shell in his right hand over the aze· bidádatí which was held in his left hand with its butt pointing to his right. The brew was administered four times, the last drink exhausting it.

The medicine is given over the medicine stick in order to apply the

19 Cf. ibid., p. 401.
power of the fetish along with the herbs; "it [the fetish] will give you a lift; it will heal sickness." The stick is held just so because the diyin diné’é are watching to see if anything is done incorrectly. They say: "Let us see if it is the right stick; if not, he will not hold it properly."

The four drinks of medicine are given to the patient by the diyin diné’é. Diné’h diyiní (holy young man) gives the first one, t’ciké’h diyiní (holy young woman) gives the second, ’ackí diyiní (holy boy) gives the third, and ’at’é’d diyiní (holy girl) gives the fourth.

After this thirteenth song was finished there was a short interval while the patient, taking the sunwise course around the ho’yan, went outside for a brief rest. Some of the visitors also went out for a respite after the patient had retired, for in any chant no one may leave the ho’yan until the patient has gone out for the first time. This is because the diyin diné’é will recognize the first one who leaves as the patient.

After the patient had reentered the ho’yan and taken his seat, eight more songs were sung; and this completed the first night’s ceremony, which had lasted about two hours. The women and children then retired to the shade house while most of the men prepared to sleep in the ho’yan.

The principal purpose of the first-night ceremony is to bring back the medicine which was given some time ago and make it new (hadita).

**TA’AGIS (BATHING)**

About eight A.M. the following morning the bath was prepared. Meanwhile the patient sat on the north side of the ho’yan, to be out of the way, and removed his clothes except for a gee string (in this and in other ceremonies which require undressing, men strip to a gee string while women retain a skirt).

An assistant was given a prehistoric stone knife from the pouch of the medicine man and a sack of pollen, and was sent for a piece of soap-weed root (*Yucca glauca* Nutt. or *Yucca baccata* Torr.).

He goes toward any one of the cardinal points and finds a perfect, full-grown yucca plant. Upon an adjacent plant he places corn pollen, on the east, south, west, and north, and in the center; and sprinkling some upwards, he prays: "Plant, I want your living medicine. I need your help to cure the sickness; I need your good health. You shall bring back good health to him (or to her)."

Digging around the perfect plant he exposes the roots and with the stone knife cuts a piece about four inches long and an inch in diameter, being careful to remember the bottom and top ends.

The stone knife is used because it was the cutting implement when there
were no metal knives, and the diyin diné’é used it. He then places pollen on the cut ends of the remaining roots, so they “will join again.” Taking the piece of root to a shady place, he pounds it carefully with two stones to remove the bark and then brings it to the hoγ’an.

In the meantime a pile of fresh earth from the cornfield had been placed at the west side of the hoγ’an and spread out to form a smooth platform. In front of this a medicine basket, which had been soaked in water to make it tight, was placed with the opening in the design towards the east. The spirits coming from the east enter through this opening. The medicine man poured water into the basket from the east, south, west, and north, into the middle, and then all around, and finally laid the piece of yucca root in the water with the top end towards the east. Then while the soap-weed root song (talawóc biyi’n) was being sung the assistant rubbed the root in the water and beat up a stiff suds.

On the surface of the suds the medicine man sprinkled material from six small sacks, applying each kind in the following order: twice across the basket from east to west and from west to east, twice across from south to north and back again, sunwise around the circumference, and in the center. The material, in the order used, was: da’ tsós (fog flakes) or white cornmeal, ground by a girl before marriage, mixed with frost crystals gathered from vegetation in the winter and dried in the sun; dry, powdered i’nédi jí aze’ (life medicine) from two sacks; blue pollen (tadití dotlíc) or powdered petals of Delphinium Nelsonii Greene; cattail pollen (tyel bitá¬ditl) from Typha latifolia L.; corn pollen (tadití). The fog flakes, blue pollen, and cattail pollen are used because the diyin diné’é formerly used them as corn pollen is now used. They are also in the class i’nédi jí aze’, and together with the actual i’nédi jí aze’ they impart curative properties to the bath. These materials are used for the bath in all chants of the classes i’nédi jí (life) and diyink’é (spirit).

A similar design was made on the earth platform with white corn meal, only the circle was about three feet in diameter, an opening was left at the east, and small crosses (+) of meal were made in the southwest, northwest, southeast, and northeast quadrants. The basket was then set in the eastward opening of this design.20

The order in which water is poured into the basket and the designs on the suds and on the platform signify the universe—the four sacred mountains at the cardinal points or quadrants, the sun in the middle, small mountains all around, with the earth and the sky inside.

20 Cf. ibid., p. 417.
The bath being ready, the patient walked in the sunwise route around the fireplace and stood behind (west of) the basket. The medicine man picked up some suds from the east, south, west, and north sides of the basket and applied them to the patient's body in the ceremonial order: bottom of right foot, bottom of left foot, right knee, left knee, chest, back, right palm, left palm, right shoulder, left shoulder; and finally suds from the center of the basket to the top of the head.

This ceremonial order is upwards because when you are prostrated by illness you wish to rise. Life proceeds upwards, for "you grow up from a baby; you must not go down. A baby lies, then sits, then stands, and then walks." Just so a sick man must rise and walk.

In all chants, procedures which involve both sides of the body are begun with the right side. Formerly a dead person's moccasins were reversed on his feet before he was buried, and clothing ties were tied crosswise. For the living, therefore, (and especially in a life—i'nádíjí—ceremony) the right side comes first and ties are made straight. This applies to putting on moccasins, trousers, tying shoestrings, etc.

The patient then knelt on the platform of earth, right knee on the southwest cross of corn meal, left knee on the northwest cross, right hand on the southeast cross, and left hand on the northeast cross. Then he washed, first his hair and then the rest of his body, downwards, with the help of the assistant who rinsed away the suds with fresh water.

In the bath bad spirits and disease in the body are washed downwards and away. The hair is washed first because the sun looks on one's head first. Soapweed root is used because the diyin diné'cé were wont to use it as soap.

The patient's necklace and other jewelry, which had been placed near the basket at the north, was also washed in the yucca suds. This is because he wishes to keep the jewelry and to get more; so it too must be purified.

The remainder of the bath water in the basket was emptied on the earth platform and the patient was dried with corn meal, first applied in ceremonial order by the medicine man and then rubbed all over the body by the assistant.

The corn meal represents daylight, for the patient is going to "meet the daylight" later in the ceremony. White corn meal is used for a male patient and yellow for a female.

The patient's jewelry was then placed upon him, after which he dressed and sat facing the east while the medicine man placed corn pollen on his tongue and sprinkled some to form a trail from the patient, by the north of the fireplace, to the door of the ho'yan. Pollen is eaten and sprinkled on the trail outwards to make the trail perfect; to make perfect the way before, behind, below, above, all around.
A dose of medicine, prepared in the abalone shell as before, was administered in four drinks as previously described. After this the patient was sent outside and told to wear a blanket and to wait in the shade-house until called.

The patient goes out to walk around so that the diyin diné'é who happen to be outside the ho'γan may see him. Some of these spirits may be outside the house and others inside, just as are the people who are attending the ceremony.

The bath being over, the earth platform was scraped up and carried outside in a blanket, to be deposited in a shady place.

**BIDJÍ (DAY CEREMONY)**

In a few minutes the patient was recalled to the ho'γan for the day ceremony (bidjí). This consisted entirely of singing, accompanied by the rattles.

Any one of several groups of songs may be chosen for this portion of the ceremony; thunder songs (íní' biyi'n), spider songs (nasdeje' biyi'n), etc. Thunder songs were used on this occasion.

After the bidjí, which lasted less than an hour, the patient was advised to get some sleep during the afternoon in preparation for the next event, the all-night ceremony.

**SECOND BIT'LÉ (NIGHT CEREMONY)**

As the time for the all-night ceremony approached, about nine P.M., the patient was asked to wait in the shade-house until called. Meanwhile the following preparations were made: the altar was placed before the patient's seat at the west of the ho'γan, between it and the fireplace, with the butts of the fetishes towards the east; and beneath it were placed the pieces of calico and the money fee for the medicine man.

The diyin diné'é used to put buckskins, arrow points, shell beads, turquoise, and pollen beneath the altar when they had a ceremony. Various of the diyin diné'é wanted to sing their songs, abalone wanted to sing his song, and in return for singing each received a present of buckskin or of other things. So it is today: some of the visiting Navaho wish to help with

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Some confusion exists with regard to the use of the term bidjí'. The Franciscan Fathers interpret it as 'the closing night of a ceremony' (cf. Ethnologic Dictionary, p. 379), and refer elsewhere to the term as the close of a ceremony. The Navaho use the term colloquially to refer to any 'sing,' day or night, and often say they are 'going to the bidjí,' 'meaning an all-night 'sing.' My informants, however, insisted that strictly it means only a day ceremony, whereas the term bit'lé (its night) should be used for a night ceremony. The translation of bidjí' is 'its day' and this is consistent with the interpretation 'day ceremony.'
the ceremony by singing, and since the medicine man, the patient, and the watching diyin dinéʼé appreciate their aid, the visitors receive presents of calico from beneath the altar.

The grease (tlah) for painting the patient was prepared—a large ball of red grease about two inches in diameter and a smaller ball of black about an inch in diameter. The ingredients and the reasons for their use will be given later. The medicine man streaked each of his cheeks with the red grease.

The patient was called to the hoγan, where everyone had assembled, and after taking his seat at the west he removed first his moccasins and necklace, placing them at the north of the altar, and then all his clothes except a gee string. The "first chant" (atsate) was then begun, and during it the patient was painted. As an assistant appointed to do the painting rose, the patient also stood. Black grease was applied, first with small dabs in ceremonial order and then plentifully across the chin from ear to ear. Red grease was rubbed all over the body including the face, the patient helping to paint his upper portions. Finally "bright sand" (destci) was applied to the face in five places from right to left, two spots on each cheek, and one on the nose.

As the assistant finished painting with each material he put some of it on his face and handed the remnants to the women at the north. Here they were passed along and any one who had pains rubbed some of the grease on the affected part and put some "bright sand" on the face. (The remnants were exhausted before they came to the men’s side of the hoγan.)

A lock of hair at the top of the patient’s head was greased with black paint to make it stick together, and to this lock one of the male live feathers (elt’ló) was securely tied and made to hang toward the right. The patient then resumed his seat until the first chant was finished.

The grease basis of the paints is largely sheep fat, with a little added fat of the wolf, mountain lion, buffalo, mountain sheep, and deer. The fat of the latter animals is used because they have no fear of bad spirits, and this lack of fear is transferred to the patient. The mixed fats are carefully preserved in the medicine pouch of the singer and used sparingly.

The black grease is made with charcoal from a lightning-burned tree, because bad spirits abhor lightning or its effects. Black is placed on the chin because nayé’nesγá’ni; and to’ba’djistcini were so marked when they went to visit their father, the Sun. They had therefore no fear, and their father recognized them. Just so will the diyin dinéʼé recognize the patient painted in this way.

The red grease is made with red clay (t'ci), given by the Earth Mother. Red earth (not mixed with medicine) was also used when the twins visited their father. Nayé'nesyá'ni was painted black but to'ba'djistcini was painted red on his face, and since their songs are used in this chant it is appropriate to use these colors. There is a more important reason, however, which is the keynote of the life (i'nádjí) chants. When one is sick he becomes pale. When one recovers his flesh turns red again. The red paint goes into the flesh, makes it red, brings back the healthy color, signifies "coming to life." The life chant is sometimes referred to as "painting red."

Ground life-medicine (i'nádjí aze') is also mixed with the red grease. Thus it acts as an ointment, the medicine penetrating the flesh. The paint must not be washed off for thirty-six hours, so that the medicine will have time to penetrate thoroughly. The twins also used to have "bright sand" on their faces. The above threefold combination of reasons for the use of the paint (what might be called commemorative because of its use by the holy twins; symbolic because permitting fearlessness, recognition, and return to the color of health; and therapeutic since it is an actual ointment) is a characteristic and significant feature frequently found in Navaho curative practice although not always combined in one ritual.

The live feather is tied in the hair because the diyin dinë'ë used to tie one in the hair of a person they were treating, and upon seeing it today they say: "Ah! There is the patient. He is the one who is having the chant."

When the first chant was over the medicine man gave pollen to the patient and made a trail of pollen for him to follow outside, as was done in the morning. The patient went outside for a brief rest, but this time he did not wear a blanket and he was followed by the assistant ("because the diyin dinë'ë did it that way in the beginning").

After the patient had reentered the ho'yán and taken his seat the first arrow chant was begun (k'a biyí'n), and during it he was given the four drinks of liquid medicine from the abalone shell, prepared and administered as before.

Then followed the second arrow chant, and during this the assistant gave the patient the first dose of dry powdered life medicine from the two sacks. The male and female digging sticks (gic) were used as spoons or spatulas, first the male and then the female for each of the two sacks, making four generous mouthfuls of the bitter, astringent mixture at each dose. This treatment was repeated nine times during the night, at about the middle of each of nine groups of songs which followed in succession. The patient was told that after the first treatment with dry medicine he could put on his clothes or a blanket and could smoke if he wished. He was required,
however, to sit up straight throughout the ceremony, and of course he must not doze.

The digging stick fetishes are used to remove the dry medicine from the sacks in order to simulate digging roots from the ground, thus showing the diyin diné’ë that the stuff is medicine dug from the earth.

The patient must sit erect with his feet and hands apart while taking the medicine, because the diyin diné’ë always held their body erect while taking medicines so that the body would become straight, not twisted with disease.

(On one or two occasions two mouthfuls of medicine from one sack only were given. The patient was informed that “two is the same as four,” and that either two or four mouthfuls could be given as desired.)

The groups of songs, during which dry medicine was given, follow in the proper sequence.22

Second arrow chant (k’a biyi’n).
Third arrow chant.
The slayer of enemies song (nayé’-nesyá’ni’-biyi’n).
Killing-fear songs (nayé’-dijyá’gisin). “Nayé’-nesyá’ni and his brother killed fear, so there was no more fear, and then they sang these songs.”
Arrow-point house (be’c be’ho’γγan).
Child-birth (awe’ behanisot). “When a woman is in labor this song is sung and the child comes.”

Thunder songs (ini’-biyi’n). Since these songs were sung at the bidji in the morning, they were omitted because of lack of time. The chant should have ten groups during which medicine is given, and songs sung at the day service may be repeated if desired. It is permissible to omit them, however, if there is danger of not finishing the ten groups before it is time for the songs which must precede the dawn.

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22 A feature of the singing which was evident after the chant was well under way was amusing in that it resembled the “battle of music” sometimes met with in modern dance-halls. Since the number of male visitors was so large that all could not sit on the south side, a smaller group sat on the north side near the door. The leader of the singing on the south side was the hatá’lí, while the group on the north was led by a visiting hatá’lí who was a very able singer. As a group of songs led by one side was finished the next group was started immediately by the leader on the opposite side, whereupon the singers on the first side would join in. There seemed to be a spirit of competition between the two sides, each trying to outdo the other in excellence and in starting the songs in proper sequence. Occasionally the leader on the north side would begin a song incorrectly, whereupon the older hatá’lí on the south side would vigorously take up the correct strain amid laughter and joking remarks. This spirit of competition is purely social, having nothing to do with the ceremony except in that it stimulates better singing. Certain features of the ceremonies, therefore, may not be so rigid or so solemn to-day as they may have been in the past, nor as the perusal of what literature there is would lead one to believe.
Spider songs (nasdje' biyi'n).
Bear songs (cac biyi'n).
Buffalo songs (ayáni biyi'n). These are the most important songs of this chant.

Attention to other details prevented an accurate count of the number of songs in each group. The medicine man later stated that in this ceremony over two hundred songs are used. Since it took about six hours to complete the nine groups listed above, and since each song lasted about three minutes, there may have been perhaps one hundred and twenty songs in all; that is, about a dozen in each group. This leaves, according to the medicine man's estimate, about eighty songs for the other parts of the ceremony, which is approximately correct. I have never met a medicine man who could tell off-hand exactly how many songs there are in any one chant. They say that they learn them in sequence and that each one suggests the next, so that they can sing the series through without knowing how many there are.

All the songs used were said to be specific to the ceremony and should be sung in order, with the exception of the introductory "first chant" and the closing chants described below which are used for any ceremony.

At about quarter of four in the morning, when dawn was imminent, the medicine man smeared a handful of white corn meal across the chin of the patient, then put some on his own chin. The principal singers also applied corn meal to their chins.

The patient is going to "meet the daylight," the meal representing daylight. Those who take part in the ceremony are likewise going to meet the daylight, and so are the diyin diné'é.

The patient then sat cross-legged, close to the altar, while the medicine man picked up all the fetishes on the altar and gave them to the patient in a bundle with their butts pointing eastward, telling him to be very careful not to drop one. The dawn songs—yikai biyi'n—(often called the "bluebird songs") were commenced, while the patient beat time to the singing with the bundle of fetishes held in both hands.

Every chant closes with these dawn songs. The patient moves the fetishes up and down in time to the singing because the yellow-birds and bluebirds move like that when they sing to the approaching daylight. They are full of health and move their bodies when daylight comes. Yellowbird and bluebird live pollen are within some of the fetishes, and the patient, now that the ceremony is nearly over, should also be full of health and so should move like the birds.

The butts of the fetishes are pointed to the east, towards the coming
daylight, because the patient wishes to "win the daylight" and thus win back his health.

The dawn songs lasted until about four-thirty. At their completion the assistant filled the medicine basket with water, and followed by the patient who was in turn followed by another man, he walked sunwise around the fireplace four times, sprinkling water on the ground as he walked. After the last trip around, the assistant emptied the basket of water outside of the ho-yan, and the patient walked out through the door and a little way to the east. There, facing the dawn which was just breaking, he extended his arms to the sides and four times brought his hands to his mouth as if gathering in the daylight and swallowing it. Then he walked back towards the ho-yan and was met half way by the assistant, who untied the live feather from his hair and rubbed away the grease from the lock of hair with pollen. The assistant took the feather back to the medicine man and the patient retired to the shade-house.

The water is sprinkled on the pathway of the patient so that he will be walking "cool" (without fever) from now on. The remainder is put outside the house in order to make the way cool outside as well as inside. Another man, following the patient, acts as a guard so that no bad spirits can pursue.

In the morning the diiyin diné'ę are ready to go to their homes in the east. After the daylight has arrived they will be gone. The patient is going to "swallow the daylight" in order to get back his health. He must do this while the diiyin diné'ę are still present; for they are watching, and if he does it correctly they will take him away from the bad spirits, from disease. If he does it incorrectly he will still belong to the sickness.

After the patient had left, the altar was dismantled and the fetishes were packed away in the medicine man's bundle, while a "song for packing up" was sung. Then songs of the four sacred mountains (dził biyín) were sung to close the ceremony.

After breakfast, when most of the guests were preparing to return to their homes, the medicine man gave a piece of the calico to each of the more important personages who had assisted with the ceremony and had stayed in the ho-yan all night. One woman received a piece because she had helped with the singing throughout the night. The man who had assisted with the bath received the medicine basket as his gift.

The patient was not allowed to sleep until past noon. The ceremony is not officially over until afternoon, and if the patient should sleep before this time he might have bad dreams inimical to health, and so the bad spirits would "win back the chant." Furthermore, although most of the diiyin diné'ę go home before daylight, a few might stay around until noon, as
do some of the Navaho who attend the ceremony. It would be improper to sleep in their presence.

POST-CEREMONIAL PROCEDURE

The patient must not wash until the next day, in order to give the medicine in the grease paint time to penetrate the flesh and cure the sickness. Otherwise no special behavior was required of the patient.24

On the following morning the patient bathed and put on clean clothes. He first washed his entire body with ket’lo, a lotion which the Franciscan Fathers have termed “ceremonial liniment,”25 although the sense of the term is untranslatable. Every chant has its ket’lo, a mixture of plants which are placed in the cold or luke-warm water that is to be used as a surface application. Sometimes it serves as an actual liniment, the medicine being supposed to penetrate the skin, but often it is merely a ceremonial wash. In this case the ket’lo consisted of tolt’cin (Mentha arvensis L. var. glabrata [Benth.] Fernald), aze’ ndote:ji (Monarda pectinata Nutt.), atá tsó’s (Hedeoma nana [Torr.] Greene), bił hasdle: (Brickellia grandiflora [Hook.] Nutt. var. petiolaris Gray), and aze’ ndote:ji ntsa:gi (Marrubium vulgare L.). In the bath it is not the intention to wash away the medicine, but to wash the color off and leave the medicine. The ket’lo, being a sacred brew, helps to accomplish this.

After the bath in ket’lo the patient was allowed to scrub away the grease-paint in hot soap and water. He was also enjoined to be careful to wash all the paint from his clothing and blanket before he exposed these articles in public again. In other words all of the sacred paint must be disposed of, now that the ceremony was over. This ended the patient’s part in the procedure, and he could now resume his usual activities.

DISCUSSION

The following analysis of the motivation involved in this ceremony is based on the reasons for such features of the chant as were explained by the hatá’li after the ceremony was over. It might differ, therefore, from an analysis based on data obtained elsewhere, under different circumstances, or from other hatá’li. It is offered as a tentative schema of the conceptual basis of the Navaho system of therapeutics.

24 It was interesting to note that although the medicine man had been a close friend of the patient for several years and was always willing to give any information asked for concerning sacred practices, he would not allow any photographs to be taken of the patient until the paint had been washed off. He said it was not “proper.”

A major motive which ran throughout the ceremony as a dominant theme was the desire to attract the attention of the holy people (diyin diné'é) and to do everything correctly so that they would not be displeased, thereby winning their assistance in curing the patient. The general conception in the dealings with the spirits in this chant seemed not to involve the placation of angry deities who had harmed the patient, but rather winning the favor of more or less disinterested deities and thereby securing their aid. This motive may be called ingratiatory. Although it was a main theme applying to the correct performance of every act, very few if any isolated acts were purely ingratiatory.

A frequent objective throughout the chant was to make sure that the diyin diné'é would recognize the patient as such and would know what was going on. This recognition motive may be regarded as preparatory to ingratiation. Having made everything clear to the holy ones, then one attempts to please them.

Within this general scheme of recognition followed by ingratiation, a threefold combination of motives is apparent. One may be called commemorative. Things are done because they were done by the holy people in the beginning, thus recalling them to mind and conforming to the precedents established. Such practices have usually been called symbolic, but it is felt that there is a definite distinction between those which are purely commemorative and those which are strictly symbolic—that is, expressing by resemblance. The commemorative acts would, of course, be highly ingratiatory.

True symbolic motives were frequent. Three infrequent minor motives may be distinguished and classed as sub-headings of the symbolic: protective, intimidatory, and symbolic-therapeutic.

The third motive in this scheme may be called the actually therapeutic. These three and the sub-branches of the symbolic are found variously combined. An example of the threefold combination in one practice is seen in the use of the red grease-paint, as described above.

A curious aspect of the Navaho explanation of the procedures is the frequent reference to the similarity of the behavior of the holy people and that of the Navaho themselves. The diyin diné'é like to attend a ceremony; they want to sing, they appreciate the aid of the assistants as do the patient and the medicine man, some are inside the ho'yan and some outside during the chant as are the attending Navaho, most return home at dawn but some may stay around as do some of the mortal guests, and so on. It would seem that the Navaho have based their conception of the character of their deities largely on their own inclinations and habits. Their attention to the social attributes of the holy ones and to the etiquette of entertaining these unseen guests serves as an example of the basis for the often repeated statement that the Indian is more intimate with his deities than is so-called civilized man.
The distribution of the motives in the various parts of the ceremony is logical. The ingratiatory and the therapeutic are present throughout. Most preliminary acts are recognitive in order to first gain the attention of the holy people. The reasons for the materials, construction, and arrangement of the fetishes of the altar are largely commemorative and symbolic. In the bathing the symbolic predominate. The day ceremony consisted entirely of singing, and since the text of the chants was not recorded, an analysis of the motivation of the chanting cannot be presented. During the night ceremonies the commemorative, symbolic, and recognitive are all frequent. In the post-ceremonial procedures the protective and therapeutic prevail.

It is probable that the Navaho does not mentally classify these procedures, but regards all of them as of ultimate therapeutic value. He evidently does regard some as effective without the help of the diyin diné’é (medicines are sometimes given without having a chant), and others as depending upon ingratiation. The former group would be carried out with the patient primarily in mind and would include the actually therapeutic, some of the symbolic, and the sub-branches of the symbolic; that is, the protective, intimidatory, and the symbolic-therapeutic. The latter group would be performed with the diyin diné’é primarily in mind, for their secondary effect upon the patient, and would include the recognitive, ingratiatory, commemorative, and most of the symbolic. A white physician, on the other hand, would designate the actually therapeutic measures as one group and lump all the others as pseudo-therapeutic, though of possible psycho-therapeutic value. He might distinguish among the latter those which are concerned with attempted healing and those which are more purely religious, such as the commemorative.

Boston University School of Medicine
Boston, Massachusetts
BOOK REVIEWS

NORTH AND SOUTH AMERICA


To take the author at his word, this book was written because his previous elaborate publications on the Crow were unread, "buried," to use his own words. Administrators of institutions may welcome this statement as confirming their own private opinions and as confessing to a waste of research funds. Perhaps the present book may be taken as demonstrating that all the important results of field work can be published privately to the profit of both author and publisher.

Dr. Lowie's chosen audience "embraces anthropologists . . . sociologists, historians, and other social scientists eager" to learn something about Indians. Seemingly as an afterthought he cites the layman as a possible reader. What we are to expect, then, is an academic treatise. At the outset the author explains his field method—the traditional procedure—and defends it. He acknowledges that he uses interpreters, that after years of study he cannot handle the language with precision, that he has neither lived as a Crow, taken a Crow wife, nor seen all that he describes. That the author was moved to say as much may be unintelligible to the layman and sound strange to the sociologists, but it is worth noting. Since anthropologists are now setting out to prove that individual differences and personality are merely phases of culture, using the psychoanalytic approach, it is well to note that, if after twenty years of effort the author's ear is still dull, the interpreter of the future must play a new rôle.

The traditional procedure in a review is to furnish an abstract, but since practically all of the information in this book has been published, a mere outline will suffice. Two brief chapters introduce the reader to some aspects of political organization and kinship. Next there is a long chapter on the life cycle of the individual, about half of which is concerned with sex, but does not go deeply into that subject. One tenth of the book is given to material culture, labor, art, and amusements. The specific topics are selected and so may serve to give the reader a sample of the activities which took up most of every day in the life of these Indians. The final two-thirds of the book deal with those activities which sociologists regard as pertaining to leisure time and, though the author does not say as much, it is clear that his chief interest lies in the doings of adult males. Thus many weird and startling personal names are scattered through the text but they designate males; one must search carefully to find the few names of females. Thus the reader may well carry away an impression that the Crow lived in a man's world and one in which technology, food, and government were but incidents in the life of men. True, the author tries to offset this by rising valiantly to the defense of the male, minimizing his mysticism and leisure on the one hand and the realistic toil of the women on the other. But the emphasis in the book is upon mysticism, magic, and ritualism, all dominated by the male.
There is a brief discussion of what is called war, the formalized ritualized predatory behavior of males. However, since war belonged to the past the author could but report upon the surviving thrilling stories of the war trail. Of these limitations the reader is duly warned. Mythology comes in for fuller treatment. Such material is always voluminous and the publication of it the most difficult to justify in a scientific budget. The myths selected are virile examples but perhaps a little too literal for even the academic person unaccustomed to such rendering.

The climax of the book is reached in descriptions of the more elaborate ritualistic ceremonies, as the Bear Dance, Sacred Pipe, Tobacco Society, and the Sun Dance. These terms should be familiar to the readers of this journal. Thus, the Bear Dance, as presented by the author, refers to a concept which the Crow make dominant, viz., that of a living entity within one's body. In the account of the ceremonial pipe we see influences of the hako, but the author avoids comparative comment. Here he may have had the layman in mind. The Sun Dance is the final chapter, and in this case the reader is offered some opinions as to the conceived social functions of the procedure. The descriptions of these ceremonies are exact and largely objective, according to the ideals of the author's profession, but in books for outsiders the reviewer wishes some of the power and feeling of the participants in such ceremonies could be conveyed. Somehow the reader should come to understand that the seemingly passive, poorly costumed, non-staged Indian ceremony is visualized by the participants as a brilliant pageant. As with the child to whom a broom is really a horse, a few simple objects are but hints of what is unfolding in the minds of the participants.

At the outset the author promises to depict human social behavior, but for a scholar, distinguished in comparative and historical research, to go far in that direction is expecting too much. However, he comes nearest a behavioristic point of view in the chapter dealing with "wife stealing" and "throwing wives away." The casual reader may pass this chapter by as merely some additional curious information about sex, but we suspect the author had it in mind to sketch an interpretative picture of audacious misbehaving virile males. We wish he had been more dynamic and impressionistic, but we suspect that a scientist cannot write in that way without feeling that he is parting company with the truth. The last chapter—the brief conclusion to the volume—rises to an appealing level. It is a case in point. It reaches for the emotions, is highly selective, is weak as an informing document, but achieves its objective; it grips the reader.

We suspect this book will interest, but not greatly move non-anthropological readers. The anthropological reader, on the other hand, may get a thrill out of it, since he can bring to it a rich interpretative background which can supply the mental pictures necessary to a vivid living presentation of Crow life. To him it will be "the book of the month."

It may be advantageous to compare this book by one of the world's great anthropologists with one by a non-professional writer. One of the closest parallels is McClintock's volume on the Piegan of Montana and Canada, "The Old North Trail."
McClimtuck is a layman who studied the Piegan for many years; his point of view is more humane and altruistic than that of an anthropologist. Yet his book is painstakingly exact. It gives something lacking in the more academic treatise on the Crow, verbal snapshots of everyday living, as McClimtuck saw it. He makes no effort to reconstruct the past, nor to slyly implant academic concepts in the reader's mind, but succeeds in giving the feel of everyday Indian life. His descriptions of ceremonies are not composite but specific; yet are rich in detail. This is not to say that a professional anthropologist should write such a book, for in the opinion of the reviewer he cannot and be true to his traditions.

Finally, the question arises as to the desirability of every professional field worker producing a book, primarily for anthropological readers, but highly descriptive, not trying to write a thriller to make money, but presenting a serious, not too technical last view. Many would prefer such volumes to the usual detailed memoirs. Even the latter are not entirely satisfactory to the investigator; he would like to see the original field notes. Perhaps the ideal of the future will be to publish brief summaries followed eventually by a book. If, then, all field notes were typed and a few sets made available for circulation when needed, the costly memoir might be omitted. Thus, if Dr Lowie should deposit in two or three central libraries, typed copies of his voluminous notes, his seriously exact but readable book would render it undesirable to exhume his buried memoirs.

CLARK WISSLER

AMERICAN MUSEUM OF NATURAL HISTORY


Sinkyou Notes. GLADYS AYER NOMLAND. (Same series, Vol. 36, No. 2, pp. 149–78, 4 pls., map, 1935. $.35.)

Varia Anthropologica. (Same series, Vol. 35, Nos. 3, 4, and 5, pp. 23–48, 2 maps, 1936. $.25.)

The first monograph is a complete ethnographic description of the Wintu (not to be confused with the Wintun, the linguistic kin and southern neighbors of the Wintu), who dwell mainly in the mountains north of the Sacramento Valley of California.

It departs from the usual ethnographic account in two important respects. First, as it "lays stress upon behavior and attitudes of mind" and, as "artifacts are merely tools of behavior," technical descriptions of artifacts are relegated to a section at the end. This arrangement is felicitous, for it permits a more direct and less involved account of economic activities than is ordinarily achieved. Second, an attempt has been made "to distort as little as possible the personal and anecdotal nature of the material as it was procured in the field" in order to record variations in individual attitudes and behavior as well as to describe the "type culture." This procedure has served particularly well to elucidate shamanism, which is that of the acculturated rather than strictly aboriginal Wintu. As the author recognizes, however, it is of
doubtful value in lending insight into aspects of culture which are no longer functional. Another merit of this method, which, in the reviewer's opinion, is equally important, is the information it provides concerning the reliability of any material. Too often, the doubts and sources of error in collecting information are not revealed in published accounts, so that writers may easily "make ethnography."

Although it was impossible to record individual variation in most aspects of Wintu culture, the reviewer regrets that regional variation is not more fully indicated. The Wintu are subdivided roughly into nine areas, whose differences are sketched in three introductory pages but not always specified in the subsequent topical treatment of Wintu culture.

Miss Du Bois has produced an excellent monograph in spite of the almost complete disintegration of native Wintu culture. Although her study is predominantly descriptive and only incidentally comparative, it shows the Wintu to have stood between central and northwestern California in their subsistence, utilizing salmon, deer, and acorns, and in their material culture, having an almost equal number of traits of both northern and southern affiliation. The socio-religious structure, however, like that of most California hills peoples, lacked a formalized pattern or readily definable configuration. Its socio-political institutions were flexible and often amorphous, and it failed to share the specific ceremonies of either northern or southern neighbors. Such society placed a premium on individual personality, thus enabling the shaman to attain social and political as well as religious importance.

The three aged fullbloods from whom ethnographic notes on the Sinkyone were gleaned were the last survivors of a small Athabaskan-speaking tribe formerly located on the coast of northwestern California near the headwaters of the Eel River. The pages include a fair account of hunting and fishing, brief notes on social, political, and religious customs, description of a few artifacts, and eleven myths. Because none of the informants was good, the material is necessarily thin on some subjects and lacking on others.

"Varia Anthropologica" is a collection of three short papers. "A Karuk World-Renewal Ceremony at Panaminik" by Philip Drucker contains a description of Panaminik world-renewal ceremonies obtained from two men who had participated in them prior to 1910 and gives brief comparative notes.

"Karok Towns" by A. L. Kroeber and "Wiyot Towns" by Gladys Ayer Noland and A. L. Kroeber enumerate the towns and houses of two tribes of northwestern California and give a revision of population estimates.

JULIAN H. STEWARD

BUREAU OF AMERICAN ETHNOLOGY


This little volume gives a clear picture of the social and religious life of the Ojibwa Indians of Parry Island, for which we must be very grateful. A point which
is decidedly interesting is that the marriage of parallel cousins is forbidden but that of cross-cousins is permissible and common (p. 98). Inferentially, I presume that the latter is theoretically not mandatory. These facts correspond exactly to what obtains among Indians of Great Whale River.

It is to be regretted that apparently a phonetic key is not given. It makes it impossible for me to interpret padreudang (p. 25) and bodreudang, besreudang (p. 35). Of course r preceded by a consonant is a combination that certainly is lacking in most Algonquian languages. The dialect clearly eliminates certain medial vowels at times (meksigisiz, p. 12; andjibnes, p. 35; osawinimki, p. 51; buankik, p. 61; Djiskiu, p. 60) as well as (apparently more rarely) initial ones (djik, p. 26).

Apparently *kk appears as (k)k, which also occurs in some other dialects; o for *a before m surely must have a very limited distribution. I wonder if yaskwagami and daskwagami, both meaning "Muskat people" (p. 1) are not misprints for *jaskwagami and *djaskwagami respectively; cf. ojask, "muskrat" (p. 26); note also Ojibwa (Jones) wajack,1 Algonkin (Cuoq) wajack;2 Menomini (Bloomfield) u'sas, Fox (Michelson) k'sac'kwa, Proto-Algonquian *u'cackwa, Ojibwa (Baraga) watjaskh, Algonkin (Lemoine) wadjack, Cree (several sources in uniform transcription) watask, Montagnais (Lemoine) utsiskū.

On p. 24 bitokomegoog is Potawatomi rather than Ojibwa, and it may be noted that, as Jenness tells, some of his informants were Potawatomi. Probably wabnik "giant beaver" (p. 38) is a misprint for *wabnik ("white beaver"). Mishomis, "the sun" (p. 109), is rather "grandfather." Very clearly nodawe, "people who pursue in canoes" (a designation of Mohawks) is a popular transformation of nodawe, "Snakes," a designation of Iroquois (p. 101). So too sinebaun, "Stone medicine-men," an appellation of the Assiniboine, is a popular etymology for "Stoney Sioux." On p. 47 modos is either a misprint for *manidos, or needs elucidation. On p. 52 "Thunder retires" is an impossible translation of Giwe-yon. The Indian equivalent of "manido power" on the same page would have been very desirable.

It is a great pity that there is no attempt to correlate the terms of relationship (p. 115) with the known schedules of Baraga, Morgan, and Speck.2 It should be stated that although some terms seem to differ slightly according to the sex of the speaker, this is either not really so, or it is due to recent innovations, for in such

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1 Altered initially.
2 I take the opportunity of pointing out that Baraga's nindangwe is a misprint for nindangwe. Cuoq's Algonkin schedules (Proceedings and Transactions, Royal Society of Canada, Vol. 10 [1892], 1893, p. 66) should also be consulted. I cannot undertake to review in detail Speck's schedules of Timagami Ojibwa; but I will point out that a certain amount of merging of terms occurs; internal evidence as well as evidence from other Algonquian languages shows that certain terms are recorded phonetically faultily with the result that a non-specialist would not know that they are cognate, with, however, in some cases, a diminutive suffix. The term given for "grandmother," and the vocative thereof, are linguistic puzzles; the "preferable" term for father's sister is an innovation; the historically old term is nizi'g̱wa; the term for mother's sister has been extended in meaning.
cases they go back to the same Proto-Algonquian archetypes. I note that the terms for daughter-in-law and son-in-law apparently are absent. It is simply astounding that all male cousins are merged into a single category and similarly all female cousins. Such a situation is nearly unique among the Ojibwa.\(^2\) Note too that although parallel- and cross-nephews are distinguished with both male and female speakers, with male speakers parallel- and cross-niece are merged completely into the old term for brother's daughter, whereas they are distinguished with female speakers. Yet it may be noted that in this latter case sister’s daughter is the same as sister's son plus a diminutive suffix (the slight difference in prior portion, if really true, can be proved to be a recent linguistic innovation; in the same way dozam- is the same as do'zem); the term for brother's daughter with female speakers is historically the old term for brother's daughter with male speakers. It is very obvious that a number of very radical innovations have taken place; and this supposition is confirmed by internal evidence, for, as we have seen above, marriage of parallel cousins is forbidden but marriage of cross-cousins is not only permissive but actual; and yet both parallel- and cross-cousins are designated by the same terms. And we are told that exogamy of patrilineral "clans" is disappearing. Are we to attribute the merging of the kinship terms referred to above to this, or is it due to white influence? In this connection I may say that on different occasions I have tried to prove that as far as Algonquian tribes are concerned kinship terms are primarily linguistic and disseminative phenomena, and that old terms may be replaced by new ones, and that the actual system may be altered. For this last cf. also Hallowell. The localization of the Menomini (p. 1) may be traditional, but it is incorrect. I am not convinced that Speck’s theory of hunting territories, which is challenged on pp. 5 and 6, is wholly erroneous; it may be wrong in any given instance.

Lest it be thought that what I have pointed out is inconsistent with my opening sentence, I hasten to say that the book is full of new and well-digested material; and from a study of this it is evident that the ethnology of the Parry Island Ojibwa is much closer to that of the Ojibwa of the United States than to that of the Ojibwa of the north.\(^3\)

Truman Michelson

Bureau of American Ethnology

Report on the Excavation of Jemez Cave, New Mexico. Hubert G. Alexander and Paul Reiter. (Monograph, University of New Mexico and School of American Research, No. 4. 67 pp., 20 pls., 17 figs. Santa Fé, 1935.)

The excavations which produced the data for this report were conducted in the autumn of 1934 and the spring of 1935. They constituted the first cave-shelter work in the Jemez region and the collection of perishable materials obtained is more

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\(^2\) Printed by the courtesy of the Smithsonian Institution.
complete than that from any other site in the section. As a consequence considerable information has been added to the knowledge of the material culture of that part of the Pueblo area. This is particularly true for such objects as sandals, textiles, wooden implements, and food materials.

The report consists of an introduction, a section on the fieldwork, a section on the specimens, and a summary. The introduction tells of the events leading up to the investigations, including mention of recent ceremonial use of the cave by Jemez people. Discussion of the fieldwork includes data on the location, size, and shape of the cave; a consideration of the character of the fill; tells of the method of excavation, describes the stratigraphy of the deposits; and briefly mentions structural remains. The section dealing with the cultural objects constitutes more than half the publication, a not undue proportion, with special emphasis on the perishable materials.

In the summary the writers conclude that the occupation may be divided into three postulated periods; the first and last being of considerable duration, while the middle one was comparatively short. The first was characterized by sporadic use of the cave as a temporary shelter. From this level evidences of contact with the San Juan, including some Basket Maker items, were obtained. The latter material suggests to the investigators the possibility that the site, because of its peripheral location, affords evidence of diffusion from the San Juan Basket Maker center toward the southern Basket Maker sites. The second period was one of semi-permanent occupation and is dated, on potsherd evidence, as circa 1250-1300 A.D. It also had San Juan contacts, late Pueblo III, and is believed to have preceded the establishment of the nearby community houses. The third period saw the cave revert to its earlier status as a temporary shelter with sporadic occupancy by the late prehistoric and historic peoples.

The report as a whole shows the results of a well conceived and carefully adhered to plan of procedure. The writers are to be commended for making their data available so soon after the completion of the work. While this reviewer can appreciate the circumstances governing their decision to enumerate only briefly the more common cultural remains, pottery, bone, and stone, it does not seem “that sheer duplication would now result from a minute description of these” because detailed discussions of Jemez variations are planned for a later date. In too many instances we have waited long and vainly for discussions “planned for a later date.” It is much easier to give complete and detailed consideration to a small number of specimens from a site such as this than to a large mass of material from a number of digs. Furthermore, material obtained under such carefully controlled conditions frequently throws welcome light on obscure factors in larger studies. From the mechanical standpoint it must be said that the illustrations, the text figures particularly, do not maintain the high standard set by the rest of the report. This is a feature generally beyond the control of the authors and due allowance should be made for that fact. In the present instance, however, the all around quality would have been improved by more “finished” drawings.
The new monograph series of the University of New Mexico and the School of American Research is a welcome addition to Southwestern publications. After its many years of work in the area the School of American Research undoubtedly has many manuscript reports filed away. The new series would be a good outlet for these items, as well as papers on more current investigations, and it is desirable that they be included in the publication plans of the editors.

BUREAU OF AMERICAN ETHNOLOGY

FRANK H. H. ROBERTS, JR.


The material described in this publication was obtained from 100 sites lying in the area between the Little Colorado River on the east, the San Francisco Peaks on the west, Deadman’s Wash on the north, and Walnut Creek on the south. The district covers approximately 25 by 35 miles. One of the outstanding factors is the paucity of specimens from so large a number of sites. The author explains this on the grounds that each individual house was occupied by a single family and the possessions of such a group were probably not numerous at best. Also, many of the ruins indicated that the houses had been leisurely abandoned, the inhabitants taking all of their more valuable property with them, leaving only such common things as manos, metates, and hammerstones. All but one of the sites investigated are in the open and perishable objects are missing. The single exception was a cave where a few articles (textiles, wooden tools, basketry) belonging to the period were found. They give a clue to the types of each in use but do not present a comprehensive picture of that phase of the material culture. Study of the specimens revealed evidence of extensive trade between the inhabitants of this region and those who lived in other parts of Arizona, in New Mexico, and along the Gulf of California.

The report covers all phases of the material culture—stone, bone, and wooden implements, textiles, food, clay work, pottery, ornaments, petroglyphs, and human burials—except that of the houses and house types. A separate bulletin (same series, No. 4, 1933) by H. S. Colton and L. L. Hargrave is devoted to that subject. The section on pottery is not as complete as some of the others, since that class of objects was described in detail in an earlier paper by Hargrave (same series, No. 1), but Miss Bartlett gives a good summary of the forms pertaining to that horizon. In the descriptions of the specimens all of the essential data are briefly, yet clearly, presented. Useful comparative material is included wherever necessary and the end of each major section has a helpful summary. The paper shows the results of careful study and a thorough grasp of the subject. It is a good reference work for Pueblo II for the restricted area with which it deals.

BUREAU OF AMERICAN ETHNOLOGY

FRANK H. H. ROBERTS, JR.

This is one of the most important ethnographic works on Mexico in many years, representing the first study by trained ethnographers of a relatively active culture outside the areas of high civilization. The material has been collected with considerable care and thoroughness, although not all of the Tarahumara are included in the study.

The presentation and the analysis of the data by both authors, however, are less satisfactory. Ethnographic work in Mexico requires special preparation beyond that necessary north of the Rio Grande; and the deficiencies of the authors are evident, particularly in the treatment of problems of acculturation and in the casual use of documentary data.

The first section, by Zingg, deals with material culture, giving some consideration to regional variations. Especially good is the treatment of economics, showing the complete integration of cattle into the basically agricultural life of the Tarahumara. The disruptive effects upon the fabric of society, inherent in the scattered locations of arable land and the necessity of close-herding cattle over a wide area, are ably presented. The sections on ethnobotany and ethnozoology probably represent as extensive a collection of this type of data as it will be possible to make in Mexico. Introduced Spanish techniques seem to have been merged into native patterns. The description of technology is obscure in places and would have been improved by drawings.

Bennett's section reveals the family as the dominant social unit. The kinship system was apparently collected without recourse to the genealogical approach. The pattern is similar to that of most of the northwest Mexican Uto-Aztecs, with emphasis on age distinctions between siblings. A joking relationship between relatives by marriage, with ceremonial functions, is of interest. The centripetal forces in the scattered communities are provided by ceremonies and by political institutions. The latter are primarily Spanish in origin, although possibly amalgamated with pre-Hispanic usages, particularly in the legal concepts and the use of moralistic speeches. An interesting merger of Spanish secular and religious officials passes unnoticed by Bennett.

Religious concepts are badly disrupted, but ritualism is better preserved. In the description of these, aboriginal and Catholic elements are segregated with a definiteness that one suspects is more neat than accurate. Shamanism still flourishes, but witchcraft is declining.

The final section, by both authors, deals with historical developments and the relations of the Tarahumara with peoples of surrounding areas. A case is made for the development of the proto-historic culture out of an early Basket-Maker-like substratum. Sequential lists of traits give the changes indicated by archaeological data as well as those due to Spanish influence. Fuller use of early Spanish documents,
especially the Jesuit Pfeffercorn's early work on the Tarahumara, would have improved this section.

An effort to link the Tarahumara with the Pima-Papago is unconvincing, as the pre-Hispanic traits shared by the two belong to a wide area extending beyond even the Uto-Aztecs, while the post-Hispanic similarities are widely distributed in Hispanic America.

The comparative work suffers from an uncritical use of documentary material, particularly, I suspect, of Bancroft's "Native Races of the Pacific States of North America." Thus the Cahita are erroneously assigned such traits as adobe, stone, and multiple story houses, and the domesticated turkey, while round houses, mat houses, palisades, and pitahaya wine are omitted, among other traits. The use of traits of Spanish origin for comparative purposes is also questionable.

The book is marred by ineffectif writing, Zingg's section in particular being frequently redundant and often ungrammatical and confusing. The publishers have committed an unusual number of typographical errors. One must also note with regret the copyrighting of a scientific work.

The several strictures in this review should not obscure the value of "The Tarahumara." Its very importance in a neglected field has made it necessary to point out faults which could be ignored in a book of lesser worth.

UNIVERSITY OF CALIFORNIA AT LOS ANGELES

RALPH L. BEALS


Falkner's classic work, now extremely rare, is here reprinted complete in a full-size facsimile edition from the original edition of 1774. The six-page Introduction includes pertinent bibliographical data on the original and translations thereof, and a sketch of the life of Falkner himself. Eighteen pages of notes give excellent critical and explanatory material on the geography, fauna and flora, tribal divisions, linguistics, and ethnology of the Pampa and Patagonian regions. The Tehuelche (Tsonecan), Puelche (Gennaken), and Araucanian words scattered through Falkner's pages are conveniently gathered together in three separate vocabularies. A very full index completes the volume.

All in all, the editor and publishers have done their work well, and have made debtors of all of us interested in South American ethnology.

CATHOLIC UNIVERSITY OF AMERICA

JOHN M. COOPER

OCEANIA AND AFRICA


The purpose of this book is, as the author proclaims, to demonstrate whether and to what degree temperamental differences between the sexes are innate, biolog-
ically determined, or to what extent culturally conditioned. An inquiry into the educational system of three tribes is connected with the main problem. These tribes have not hitherto been studied. They are (1) the Arapesh, a hill tribe near the coast of Aitape, in the Mandated Territory of New Guinea, (2) the Mundugumor, on the Yuat (Dörferfluss), the second southern upstream tributary of the Sepik River, and (3) the Tchambuli, lake dwellers in the middle Sepik region.

About half the book is devoted to the description of the Arapesh. Unfortunately the reader is not informed how much time Dr Mead spent with each tribe. This is no impertinent curiosity if one considers the difficulties in obtaining reliable information from Papuans.

In this, as in her previous books, the author does not content herself with a mere record of the tribes visited but aims at a moral which would be applicable to modern American society. She tries to demonstrate the absence of innate temperamental differences between the sexes, and to prove that these are "socially produced" (p. 310) and "do not occur in societies which disregard them," such as the societies described in the book (p. 164). The author concludes by demanding of our society that "no child should relentlessly be shaped by one pattern of behavior, but instead there should be many patterns," and the world should learn "to allow each individual the pattern which was most congenial to his gifts" (p. 321).

The attempt to mingle anthropological research work with educational planning makes the book attractive for large groups of people, but may sometimes lead the student in anthropology astray. Not only that, but the attentive reader will soon discover perplexing discrepancies wherever the author endeavors to fit her keen observations into the theoretical scheme she undertakes to prove.

A few points may illustrate this. The author asserts of Arapesh society that there are no temperamental differences between sexes, neither when the behavior of individuals is observed, nor where "their cultural beliefs" are concerned (pp. 141, 164). How do these statements conform with Dr Mead’s own reports in her book? We hear that little boys are allowed to "roll and scream in the mud up to the age of 14 or 15 without sense of shame" (pp. 50–51), while "the girls' expression of anger is checked earlier." The girls join the work of the family before boys do and are "useful for carrying, weeding, gathering food and carrying firewood." In preparing the harvest or a feast "crowds and work become closely associated." But the little son accompanies his father, "to whom he is temperamentally attached, and goes with him or his elder brother on a hunting expedition or into the bush to gather herbs or vines or to cut wood for housebuilding." One such small boy and one or two older relatives "is the pattern group for little boys' work" (pp. 58–59). Moreover, "the little boy has a model of violent expression before him [in the person of his father] that the girl lacks" (p. 52). For "it is appropriate for big men to simulate anger and defiance in their public speeches, to wield a spear, stamp their feet, and shout" (p. 51). (This trait is also in contradiction to the asserted "mildness" of the Arapesh.) The initiation ceremony is restricted to the boys (p. 61 et seq.). The differences in temperament and cultural moulding as described by the author result in a pronounced division of labor;
cooking everyday food, bringing firewood and water, weeding and carrying—these are women's work, cooking ceremonial food, carrying pigs and heavy logs, house-building, sewing thatch, clearing and fencing, carving, hunting, and growing yams—these are men's work; making ornaments and the care of children—these are the work both of men and women.

In spite of these distinctions, which the author herself points out, she concludes that there are no differences between the sexes. She is not herself convinced of her conclusions, because she mentions "the restlessness of men," and their desire for visiting brothers and cousins, which becomes a "constant cause of jesting reproach of the men by the women" (p. 59, cf. pp. 5, 18, 21). The scene which occurred when Dr Mead showed a life sized doll to the Arapesh people is characteristic.

The men . . . recognized it [the doll] as a mere representation, and one of them voiced the prevalent attitude towards women's concerning themselves with such things. "You women had better not look at that thing or it will ruin you entirely."

Later the men became gay and danced with the doll in their arms. But the women schooled since childhood in the acceptance of marvels and the suppression of all thought about them, never quite accepted the fact that it was only a doll (pp. 70–71).

Does this mean a "single pattern" for both sexes, a minimum distinction between men and women (pp. 63, 141, 313)? Should polygyny (p. 109) not imply polyandry with the other sex, if a cultural and social distinction between the sexes be denied (pp. 161, 164)? Is beating wives a symbol of equality among the sexes (pp. 112–16, 140–47, 153, 161)? (Only one case of a woman beating a man is mentioned.) How should the remark that a woman "had borne her deposition quietly," and that this was "one of the virtues that the Arapesh most value in women" (p. 117) be interpreted? What is meant by the statement that supernatural guardians are supposed to punish the breach of the rules which occur when men "fail to keep separate the natural potency of female functions and the supernatural forces that aid and abet men" (p. 136)?

Whatever the author cites in favor of her pattern of equality between the sexes seems to weigh in favor of another hypothesis. Does it not prove that the relations between the sexes is founded upon a complementary interlocking of temperamental peculiarities to which the cultural and social behavior conforms? And that this behavior is shaped by the historical background and the actual conditions of life, and leads to an almost inexhaustible variety of singular cases?

The author tries to picture Arapesh society as an idyl of "no violent, no possessive individuals, a people incapable of developing the ego" (pp. 73, 141–42, 310). The true Arapesh society seems, however, to be a bit different according to the author's own report. We hear of screaming boys and violent children (pp. 50, 143, 145), of the drama of sex relations (pp. 112–14), of quarrels over women (p. 79), of temper fits (p. 153), man and wife attacking each other with axes (p. 161), of violent half-brothers (p. 153), of men beating their wives (pp. 147, 151, 153), of a woman beating her husband (p. 149), of a quarrel which followed the abduction of a woman (pp. 137–38), of a mother trying to strangle her baby and stepping on the
head of another (p. 151), and of a brother trying to use force upon his sister (p. 150). After the description of a quarrel the author says: "this gives a fair picture of the violent, unreasonable rages to which persons like Agilapwe [a man] were subject" (p. 160). We hear that revenge for the dead is taken (p. 155), that the Arapesh "resort to sorcery practices whenever slight expression of hostility occur," that the "trustful loving attitude" can be shattered by a blow, and that as a result hostility in adult life becomes overt, "its expression is random, unpatterned, uncontrolled" (p. 155). I am unable to bring these examples in harmony with the author's opinion that the "violent person among the Arapesh cannot find ... any expression of the internal drives" (p. 314). The Arapesh feel instincts and moods must be curbed (pp. 45-46, 52-54, 61, 135), in spite of their "natural tenderness." Restrictions in eating are observed between the age classes (pp. 76-77). Education, particularly at puberty, is institutionalized (p. 62 et seq.).

The theory of the non-possessiveness of the Arapesh is somewhat weakened by Dr Mead's report on the training which children receive in regard to property (p. 59), by her mention of the "violent and possessive" people (pp. 141, 145, 154-55), and by her account of a theft (p. 148). While she states that the Arapesh are a "people incapable of developing the ego" she depicts a ruthless ego (p. 141) and selfishness (pp. 83, 144).

Persons who by their behavior do not fit into the scheme are called "aberrant" (pp. 142, 157). Their percentage is not recorded.

I do not see how the number of "aberrant cases" is compatible with the "day dream of innocence" (pp. 140-41) which we are supposed to experience upon becoming acquainted with Arapesh society. Some traits recorded, together with other facts, seem to disentangle the problem. Dr Mead mentions the lack of aggressive- ness and initiative among the children, their passivity and receptiveness; that they are extremely "vulnerable to quarrelling" (pp. 56, 143-45, 154). She speaks of a man interested in knowledge (p. 145). I should imagine that the majority of these people are "introvert." This would also account for their tendency to retreat before the more active coast tribes of "Papuanized" Melanesians, and from whom they apparently accepted a number of customs (such as initiation). Lack of discipline accounts for the violent fits which are reported of boys as well as of adults.

A number of slips occur in the book. The Mundugumor are described as "cannibals and headhunters" (pp. 167, 199, 211). Ordinarily the one excludes the other. Headhunters eat perhaps lips, ear, nose, or the like, but they cannot be classified as "cannibals," who consume ceremoniously the whole body. The ghost that appears at the initiation feast is called "tamburan" (pp. 63 ff.) and the author ventures some footnotes about the Arapesh language in connection with this word. It is, however, not Arapesh, but common Pidgin English and was probably used by Dr Mead's boy. The word originated in the speech of Blanche Bay (Rabaul), Gazelle Peninsula, New Britain. A host of expressions of New Guinea Pidgin English come from there, for the Blanche Bay boys were those first recruited. On p. 18 we are sweepingly told about "New Guinea gardens." They differ greatly.

I cannot continue with my comments. I have only selected a part of the Arapesh
reports, for the author seems to have concentrated upon them particularly. A complicated study like that presented should have required, in my mind, more careful deliberation and interpretation. The book might have gained had it been completed in less of a hurry, and had the stay in New Guinea been of a longer duration. A clear separation of the factual information from the author's hypotheses would have been useful. We are not even told how the material was collected. Natives hide much from the ears and eyes of the Whites, and during a short stay circumstances may or may not occur to distort the picture. Fieldwork among Papuans and Melanesians is no easy task. It is hard to get into contact with the people, although perhaps no other place in the world affords a better opportunity of obtaining a real picture of primitive life. Therefore the responsibility of the field-worker is augmented. It should be realized that at least a year's stay with one tribe is required to yield promising results.

After all these remarks, I do not want to leave this book without acknowledging its merits. It provides a store of information about women and sex, children and education. It offers a fascinating description of daily life and of psychology of human beings. It correctly stresses the highly developed individuality of persons (pp. 141, 143, 195-96), and the differences of the tribes. It is well written and will recruit friends for anthropology.

New Haven, Connecticut

Richard C. Thurnwald


These ethnographic sketches of Negro and mixed Negro-Pygmy groups in the northeastern corner of the Congo and a part of Ruanda are a by-product of Dr Schebesta's study of the Pygmies of the same region, which he published in 1932 as "Bambuti, die Zwerge vom Kongo." The author treats especially the BaBali, BaBira, BaChwa, and Nkundu, but touches lightly on a dozen other tribes. He still follows the informal narrative style of "Bambuti," seldom lingering for more than a few pages at a time on systematic ethnography; but he describes the country with much charm, and his anecdotes are very well pointed.

Schebesta explains the present distribution of tribes and languages in the Ituri forest by a series of migrations, the later intruders separating the earlier peoples and sometimes engulfing them.

In his linguistic classification, he agrees broadly with Czekanowski, but suggests that the Mamvu-BaLesse dialects, which unite the Mombutu on the north with the BaMbuba on the south, are neither Bantu nor Sudanic, but were originally a Pygmy speech which was learned by tribes who had been pushed southward into the forest. He believes that Pygmies occupied this region long before the Negroes—an assumption generally made for the Congo, and which has yet to be proven. Some of the Negro groups are probably very old on their present ground, while many have recently come in from the grasslands on the north and east, to escape from the
Mangbetu and other aggressors. This change in environment appears most clearly in the BaNande, many of whom, having entered the Ituri from the grasslands on the southeast, have cleared away small portions of the forest to make a grassland setting for themselves.

The Ituri is a meeting ground not only for Pygmies and Negroes, for Sudanic and Bantu languages, but also for several well marked cultural streams whose sources lie outside the area. The two main intrusive factors are those of the Mangbetu and the WaNgwana: the former asserting itself in head elongation, rectangular shields, style of art, some external traits of chieftainship, and a number of other particulars; the latter in slavery, improved house types, nominal Islam, Swahili speech, and similar sophisticated features brought by Arabized Negroes from the East Coast. Mangbetu culture has affected especially the Medje, BaLika, WaBudu, and a part of the BaNdaka. When we discount the Mangbetu influences, we find that the remaining elements are mostly those of the real forest culture, as is illustrated by the BaRumbi, who separated from the Medje before the latter had received Mangbetu influence.

Tradition traces the origin of the BaBali Leopard Society, with its iron-clawed murderers in leopard guise, to the district where iron ore is most plentiful and the blacksmiths most adroit. Schebesta suggests that the society has grown up since the coming of the WaNgwana and the Europeans, the chiefs of the old-style initiation groups using it to compensate for their loss of power, and to give vent to the cannibalistic and murderous proclivities which must be repressed under the new regime.

Dr Schebesta’s photographs deserve a high mark for technical excellence and richness of subject matter. They should be a lesson to British ethnographers, whose Negroes too often sit for their portraits, perform inconsequential tasks indistinctly, or simply stand around. Prize shots: BaNyari salt-leaching, an Nkundu forge in action, and the profile of a Medje girl whose head has been squeezed into a beautiful oval by binding in infancy.

One may hope that “Vollblutneger und Halbzwergen” will be translated into English as promptly and successfully as was “Bambuti,” for the pleasure of all who like to read of Africa.

WALTER CLINE

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PHYSICAL ANTHROPOLOGY


Mr Henry Field’s publication on his series of 667 Arabs from Mesopotamia includes a chapter on the prehistory and history of the Kish area, a statement of anthropometric techniques employed, raw data, a statistical summary, and an appendix. In the latter are presented the results of 500 blood group samples and a
preliminary report of Mr Field’s 1934 expedition, during which 2032 males and 151 females were measured in Northern Arabia and adjacent countries. Sir Arthur Keith contributes an introduction, which is the only text to the metrical data, while Mr Stephen Langdon writes a brief but lucid summary of the history of Kish.

Mr Field believes that Mesopotamia, or at least its southern section, was covered by water during the glacial epoch, and thus explains the absence of Paleolithic or even of early Neolithic implements in the alluvial plain. He feels that Northern Arabia was, during the Paleolithic, a fertile region, the home of the “Proto-Mediterranean” race, tall and dolichocephalic, which with the desiccation of its homeland spread to the west into Palestine, Egypt, and elsewhere, and to the east into Mesopotamia.

Sir Arthur Keith finds the Arabs small headed, and decides that they belong to the general belt of brown-skinned humanity stretching from Egypt to India, rather than to the European races proper. He postulates that during the Paleolithic a solid belt of deeply pigmented non-whites reached from Negro Africa across Arabia to India and out into Melanesia, and that subsequently “Caucasian” peoples invaded Arabia, and that thus the modern Arab is a mixture between intrusive whites and aboriginal blacks who were related on the one hand to Hamites and on the other to Dravidians. The most characteristic trait of the Arabs, he states, is their inherent variability; their most typical physical feature the nose. Equally notable is the combination of a long upper face height with a shallow jaw.

In reading this volume the reviewer was struck with several things which merit attention.

(1) The two series of Kish Arabs measured by Mr Field and by Professor Buxton, supposedly parallel samples of a single population, differ profoundly in four dimensions—minimum frontal, total face height, upper face height, and nose height. These differences range from 10 to 48 times their probable errors. The two men used radically different techniques. Although Mr Field made the mistake of measuring the sagittal face diameters with spreading instead of sliding calipers, nevertheless of the two sets of means, his are the more plausible. On the other hand Professor Buxton’s minimum frontal is more plausible than Mr Field’s. Thus the whole subject of the face is thrown into uncertainty.

(2) Fifteen year old boys were included in the adult stature series.

(3) The author, in the Ba’ij Bedawin series if not elsewhere, apparently mistook *arcus senilis* for partial eye blondism.

(4) The raw data occupy 328 pages of this expensive publication, whereas less than one tenth of the space would have been needed had they been presented in the more useful and more usual tabular form.

In spite of these objections, which he makes no attempt to minimize, the reviewer feels that this work is a definite contribution to our knowledge of the Northern Arabs, and sincerely admires the author’s success in measuring large numbers of Northern Arabs, which is not an easy anthropometric assignment.

*Book Reviews*
The Human Foot: Its Evolutionary Development, Physiology and Functional Disorders.
DUDLEY J. MORTON. (xiii, 244 pp., 100 figs. $3.00. New York: Columbia University Press, 1935.)

This estimable work on the human foot should stimulate greater interest and further investigation along the original lines begun by the author.

Dr Morton sees each progressive change in the anatomy of the foot in the light of its functional significance—form and function being inseparable and perfectly correlated. In tracing its history he shows the foot as part of an apparatus enabling the animal first to swim, then to crawl, to run, and finally to climb. He makes detailed comparisons between the human foot and various primate feet and presents a clear case for the emergence of the terrestrial human foot, structurally adapted for support and bipedal locomotion from an arboreal, anthropoid ape form. Little if anything new is offered concerning the development of the foot but this evolutionary viewpoint has led to new interpretations of its functioning and is the basis for analyzing functional disorders.

This work makes valuable reading for both the general student and the specialist, and any of its three parts might well be read separately. Its numerous illustrations, most of which represent the original work of Dr Morton, are so enlightening and well chosen that the text might almost be considered supplementary.

The non-biological phenomenon of gravity is stressed as playing either a fundamental or complementary role in the evolution of the foot. A new and accurate method of recording weight distribution by an instrument designed by Dr Morton marks an advance in treating the subject, as does the careful and detailed working out of the mechanism of the movements of the foot in walking and running. In showing that the locomotor skeleton and its workings can only be understood through the application of physical laws, a nice correlation between the physical and biological realms is made.

Using the foot as a characteristic distinguishing man from his nearest living relatives is a new and helpful addition to the more usual evidences of skull and dentition. Turning to physiology when morphology could not be relied upon to explain foot disorders, Dr Morton found the definite part played by each portion of the foot in stance and locomotion to be the basis for determining them; this differs from other methods which place the emphasis upon form and posture. Disorders implied by the loose term "fallen arches" are due, not to general weaknesses of the parts of the foot as claimed heretofore, but to morphological imperfections which ultimately result in a more or less complete breakdown of the entire foot structure. Here again gravity as well as structural defects, is considered; other contributing factors are the "environment of civilization" and various constitutional disorders.

For examining foot disorders Dr Morton strongly advocates the use of X-rays, a method heretofore little employed, and also examinations of the stance, gait, external evidences, and "wear" of shoes. Contrary to the popular idea, he believes the unshod foot perfectly designed for support and that foot disorders are due to the interdependence of bones, ligaments, and muscles, rather than to the two more
common explanations—weaknesses of muscles or ligaments. He exposes the fallacy of that very common explanation of foot disorders—"anterior transverse metatarsal arch"—and challenges the superiority of the parallel position of the foot claimed for the American Indian, and its popular opposite, the 90° out-toeing. He recommends a 30°-40° angle of out-toeing so that the "width of the area of the foot contact be equal to its length."

The last section of the book contains much of therapeutic and practical value. 

Elsie Viault Steedman

Hunter College

GENERAL


This volume is perhaps the most interesting of Professor Thurnwald's great work and the one best adapted to familiarize the reader with his basic position. In principle this may be briefly defined as representing the golden middle path. Though rightly acclaimed as a "functionalist" intent on understanding the relations within a given culture, the author deprecates neglect of history, which seems to him indispensable (pp. 297 et seq., 315 et seq.). His attitude is thus generically close to that of many Americanists.

Since the present volume discusses the broad problems of progress, retrogression, and the relations of the individual to society, an adequate review would require many pages. Suffice it to draw attention to a few points. Notwithstanding his sociological bent, Professor Thurnwald emphasizes the part played by individual personalities in social development (p. 307). It is gratifying to register the widening agreement on this cardinal question. So far as I can see, this is a matter on which the author is in full accord not only with Professor Boas but also with Father Schmidt, from whose conclusions he often dissent.

The author is equally convincing when he warns us, as in preceding volumes, against confusing hypertexted developments of an institution with the normal or incipient form. Thus, the grotesqueries of the couvade in certain South American tribes are not basic, but must be viewed along with the varied restrictions affecting both parents. Similarly, "marriage by capture" appears in proper perspective only when bride-abduction is considered jointly with cases of groom-kidnapping (p. 248; cf. Vol. 2, p. 105 et seq.).

But possibly the most stimulating sections of the book relate to neo-evolutionism. If I correctly interpret contemporary thinking, there is a widespread trend toward rehabilitating the concept of cultural evolution in a modified form. When Father Schmidt traces the sequence of matrilineal institutions, he is certainly an evolutionist of a kind. And when he explicitly defends the propriety of making logical deductions "from the very nature of things and men," of arranging phenom-
ena "in a certain series of phases of development," he may be avoiding the term but certainly not the concept of "evolution." Far from twitting him with this apparent inconsistency, I welcome the idea irrespective of its specific application. For anthropologists are at times confronted with phenomena which are organically related and some of which indubitably precede others. Contact of aborigines with whites has repeatedly precipitated Messianic cults under conditions excluding mutual borrowing. Accordingly, there is here not merely the timeless functional relation between the angles and sides of a triangle but a causal nexus with temporal priority of one feature. This is, I believe, what Thurnwald means in referring to socio-psychological processes which independently lead to like structural types in remote regions (p. 253 f.). His merit lies in emphasizing the reality of this process and at the same time distinguishing it from the unilinear evolutionism of Spencer and Morgan (p. 236). It certainly seems significant that thinkers with such diverse starting points as Father Schmidt, Thurnwald, and Radcliffe-Brown should converge towards the acceptance of a qualified evolutionism.

In the reviewer's judgment one of the most hopeful lines of future research lies in the demonstration of correlations which to date have been more frequently asserted than proved; and in the partial investment of such "adhesions" with chronological meaning. With reference to Thurnwald's own theories I should merely venture to offer one critical comment. His work bristles with facts and historical references of intrinsic interest, but it seems to me that they are not always applied at the crucial point. The result is the appearance of schematism where it could easily be avoided. Thus, the rise of individualism (pp. 23–32) is so important a point in itself and has such broad theoretical implications that the fullest documentation would have been in place. The same applies to the discussion of stratification (p. 251 et seq.).

To summarize my impressions, I keenly appreciate the range of Professor Thurnwald's knowledge, the fusion of theoretical and ethnographic interests, his tolerant attitude towards both the sociological and the historical approach, his discriminating views on evolution. I merely hope that he will bring his ample historical reading to bear more definitely and directly on his generalizations.

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University of California

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DISCUSSION AND CORRESPONDENCE

CULTURAL RELATIONS OF THE GILA RIVER AND LOWER COLORADO TRIBES

Included in the first issue of papers in the new series entitled Yale University Publications in Anthropology is the excellent presentation and discussion of data bearing on "Cultural Relations of the Gila River and Lower Colorado Tribes" by Professor Leslie Spier.¹ What he seeks to demonstrate as stated in his own words is the following:

The time-honored supposition has been that the Maricopa, having moved from the west to a position adjacent to the Pima, have been culturally as well as politically dominated by the Pima. This is far removed from the truth. I will attempt to show that not only was Maricopa culture of the historic period overwhelmingly one with that of the Yumans on the Lower Colorado, but that the Pima, at least the Piman groups on the Gila, also affiliated strongly in the same direction. So far as Piman influence on the Maricopa goes, and it seems to have been relatively small, it was balanced by an equal counter-influence.

The first part of his thesis, viz. that Maricopa culture of the historic period is overwhelmingly one with that of the Yumans on the Lower Colorado he establishes beyond peradventure; in 1931, Professor A. L. Kroeber had already tacitly assumed this by including Maricopa among Colorado River Yuman tribes.² Had Professor Spier utilized Cocopa material also he could have made an even stronger case. The second part of his thesis seems to me to be far less firmly established by his evidence. Perhaps this is in part due to his having included Papago data with the Pima, for presumably Papago culture was less like the Maricopa culture than was the culture of the river Pima.

His data are presented in six columns as follows: (1) Elements common to Maricopa, Pima-Papago, and Lower Colorado Yumans. (2) Elements common to Maricopa and Lower Colorado Yumans. (3) Elements common to Maricopa and Pima-Papago. (4) Elements peculiar to Maricopa. (5) Elements peculiar to Lower Colorado Yumans. (6) Elements peculiar to Pima-Papago. To have achieved complete clarity of presentation there should have been a seventh column: Elements common to Lower Colorado Yumans and Pima-Papago. As it is he presents such elements in columns 5 and 6, though these elements are peculiar to neither Lower Colorado Yumans nor Pima-Papago. Since part of his plan was to prove that the Pimans should be grouped in the Lower Colorado province, it seems strange that "no attempt has been made here to list specific resemblances of Lower Colorado and Piman traits beyond what was common to the two and the Maricopa" (p. 14).

In spite of the clear presentation of the data and the preliminary discussion of them, the reader is unable to formulate precisely the interrelations of the three groups. Professor Spier modestly considered his marshalling of data as inadequate.

¹ Yale University Publications in Anthropology, No. 3, 1936.
² The Seri (Southwest Museum Papers, No. 6), p. 40.
for a statistical expression of the inter-relations between the cultures (p. 14) because it does not "do full justice to the extent and the character of resemblances and dissimilarities." Nevertheless, as one peruses his tables it becomes obvious that he has set down the salient features of each culture, so that a statistical expression of the interrelations based upon this presentation is unlikely to be radically altered by additional data. I have attempted an analysis of his data only in what follows. Those elements which he limits to one or two groups are counted as absent in the remaining groups. His queried occurrences are not counted.\(^3\)

The basic culture upon which the local elaborations have been erected comprises 100 positive elements shared by the three groups. (Universal negative elements, i.e. those lacking in the three groups, are not considered, since they are legion.)

The opposite side of the picture is presented by the unique traits of each of the three groups. In the following table negative as well as positive unique traits are enumerated. The unique negative traits seem as important as the positive ones. The positive occurrence of an element in two of the groups and its non-occurrence in the third group is undoubtedly of considerable cultural significance, in spite of the fact that some of the absences are environmentally determined.

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<th>TABLE 1. UNIQUE TRAITS</th>
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<td><strong>Positive</strong></td>
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</tbody>
</table>

The preponderance of unique traits (132) in Pima-Papago culture indicates its aloofness from the culture of the Yuman tribes, even though it participates in the 100 positive elements which are basic to the three groups. The median position of Maricopa culture is proclaimed by the low number of unique traits (38). Colorado River Yuman culture has a relatively high number of peculiar elements (80) despite the fact that the inclusion of the Yuman Maricopa in the calculation lowers the number of peculiar elements very appreciably. The extent of this depressing Maricopa influence is indicated by the Colorado tribes sharing 235 elements (177 positive, 58 negative) with Maricopa, as against the sharing of only 181 (152 positive, 29 negative) by Maricopa and Pima-Papago. The situation is further emphasized by Maricopa having 110 positive elements and Lower Colorado tribes 121 positive elements lacking in Pima-Papago.

Leaving Maricopa out of consideration, the relative aloofness of Pima-Papago and Colorado River cultures is attested by their sharing only 104 positive elements (100 of these are the above-mentioned "universal positives") and 33 negative elements.

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\(^3\) It is impossible to present the details of my count of Spier's elements here. Some of his elements were split into two or more; e.g., "bow: D-shaped; self-bow" became the elements D-shaped bow and self-bow; "breech-clout for men, kilt for women, of willowbark" became willowbark breech-clout for men, willowbark kilt for women.
Table 2 presents the details of the analysis of the data. Positive traits or presences are represented by +, negative traits or absences by −. The 100 universal positive traits are omitted. Note how the relation of shared to non-shared traits reverses in proceeding from A(Colorado, Maricopa) to C (Colorado, Pima-Papago). C bespeaks the discreteness of Lower Colorado and Pima-Papago cultures.

**TABLE 2. RELATION OF PAIRS OF CULTURE GROUPS**

<table>
<thead>
<tr>
<th>A. Colorado, Maricopa</th>
<th>B. Maricopa, Pima-Papago</th>
<th>C. Colorado, Pima-Papago</th>
</tr>
</thead>
<tbody>
<tr>
<td>C + M + *</td>
<td>M + P + *</td>
<td>C + P + *</td>
</tr>
<tr>
<td>77</td>
<td>52</td>
<td>4</td>
</tr>
<tr>
<td>M + C −</td>
<td>P + M −</td>
<td>P + C −</td>
</tr>
<tr>
<td>89</td>
<td>63</td>
<td>109</td>
</tr>
<tr>
<td>C + M −</td>
<td>M + P −</td>
<td>C + P −</td>
</tr>
<tr>
<td>33</td>
<td>110</td>
<td>121</td>
</tr>
<tr>
<td>C − M −</td>
<td>M − P −</td>
<td>C − P −</td>
</tr>
<tr>
<td>58</td>
<td>29</td>
<td>33</td>
</tr>
<tr>
<td>Total shared</td>
<td>Total shared</td>
<td>Total shared</td>
</tr>
<tr>
<td>135</td>
<td>81</td>
<td>37</td>
</tr>
<tr>
<td>Total not shared</td>
<td>Total not shared</td>
<td>Total not shared</td>
</tr>
<tr>
<td>122</td>
<td>173</td>
<td>230</td>
</tr>
</tbody>
</table>

* Exclusive of 100 universals.

Expressing the interrelations of each pair of cultures by a percentage coefficient, the different relations are more readily perceivable. Dividing the total shared traits (positive and negative) by the total of all traits (shared and not shared), the following percentage coefficients are obtained.

- Colorado, Maricopa: 52.5%
- Maricopa, Pima-Papago: 31.9%
- Colorado, Pima-Papago: 13.9%

When the 100 universal positive elements are included, the percentage coefficients, although higher, have the same ranking. Thus:

- Colorado, Maricopa: 65.8%
- Maricopa, Pima-Papago: 51.1%
- Colorado, Pima-Papago: 37.1%

These percentage coefficients substantiate Professor Spier’s judgment that “Maricopa culture of the historic period [was] overwhelmingly one with that of the Yumans on the Lower Colorado,” but seem to negate his attempt to show that “the Pima groups on the Gila also affiliated strongly in the same direction.”

Instead of a single Lower Colorado, Pima-Papago culture there would seem to be two separate cultures which abut upon one another and consequently share elements in some degree, as do adjoining cultures the world over. Pima-Papago appears to be the northern edge of a culture probably extending southward well into Mexico. Professor Spier suggests (p. 13) that “the Lower Colorado culture province should thus be expanded to include the Gila River tribes, Yuman and Piman.” The analysis gives warrant for including the Gila River Yumans, but apparently not the Pimans.
Speaking of impressions, apart from statistical analyses of cultures, Professor Spier writes (pp. 13–14).

A more careful reading of the accounts of the Pimans, especially in light of my fuller understanding of Lower Colorado culture [no doubt in connection with his Maricopa work], makes it clear . . . that the Pimans should rather be grouped in the Lower Colorado province.

Focusing his work on the Maricopa he was impressed by the resemblances of Maricopa culture to both Lower Colorado and Pima-Papago cultures. What more natural than to assume that Pima-Papago and Lower Colorado cultures constitute a single province?

My impression in working with the Papago, having earlier studied the Cocopa and Kamia of the Lower Colorado, was that Papago culture was something quite different from the River culture. Indeed, it suggested central Californian culture in certain respects. Evidently one’s fields of earlier work influence the judgments formed when one works with new cultures. Well, so much for impressions.

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Further Notes on the Cultural Affiliations of Northern Mexican Nomads

In the American Anthropologist (Vol. 37, pp. 702–706, 1935) Dr M. E. Opler presents data regarding traits which he has found existing among the Apache. Although he does not specify, it is to be inferred that he refers primarily to the Chiricahua and Mescalero groups. Comment by the writer on his communication is called for only because Opler utilizes the traits he mentions to call in question suggestions contained in a recent publication on “The Comparative Ethnology of Northern Mexico before 1750.”

Dr Opler’s first point is a criticism of the north Mexican study for not including the data which he presents. If I am not mistaken, the “Ethnology of Northern Mexico” was written before Dr Opler even began his work among the Apache. Such data was nowhere available in print, I believe.

Of greater importance is Dr Opler’s point that the nomadic peoples, of which he considers the Apache to have been typical for all northern Mexico, have been much more influenced by southern factors than is indicated in the “Ethnology of Northern Mexico.” As he has found more traits of this character than are mentioned in the documentary evidences dating before 1750, he questions the hypothesis that cultural connections between the Southwest and Mexico were largely by way of the agricultural peoples occupying the western corridors.

Unquestionably the nomadic peoples had many more traits than are shown in “The Comparative Ethnology of Northern Mexico.” Further, an attempt to draw

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1 Ibero-Americana: 2, Berkeley, 1932.
tentative conclusions from the preliminary data presented in that paper was possibly premature. Unfortunately for Dr Opler's specific contention, however, what is true of the nomads is also true of the agricultural peoples: the Spanish sources omit reference to many aspects of culture. For example, I do not know of a single reference to the metate in Spanish literature for all northern Mexico. Yet the agricultural peoples obviously had the metate, a fact borne out by both ethnographic and archaeological evidence. Indeed, modern ethnographic work among the agricultural peoples shows clearly that the number of southern elements to be found beyond those mentioned in the documentary literature is enormous. Consequently, the discrepancy in culture between the agricultural peoples and the nomadic groups would remain, despite Dr Opler's additions from the Apache. To say "the alleged meagerness of the nomad culture largely vanishes" is to fail to appreciate the cultural situation.

A number of Dr Opler's specific criticisms also call for comment. He states that if the Apache practiced agriculture before 1750, the picture of the non-agricultural northern province would be modified. This possibility was specifically envisaged in the "Comparative Ethnology" as follows: "It is likely that some peoples reported as non-agriculturists may have been so reported simply because the only sources we have are from observers who happened to visit them at the wrong time of year." Even so, the sporadic agriculture of the Apache is hardly comparable with the intensive agriculture of the Cahita or of the Tarahumare.

Other points also perhaps should have been more carefully pondered by Dr Opler before he considered the Apache as typical of the nomadic group and possible transmitters of southern elements to the Southwest. The first question which should be answered is: To what extent were the traits he lists acquired through southern contacts and to what extent through contacts with the Pueblo or other peoples of the Southwest? Would he deny Pueblo influence on the Apache? And if there has been Pueblo influence, would it not account for Apache possession of many of the traits? Furthermore, were the Apache in a position geographically to have acted as transmitters of traits to the Pueblos? Dr Sauer's analysis of tribal distributions in northern Mexico would indicate that in 1750 or near that time, the Chiricahua were certainly not in their present habitat, my own paper being in error on that point. Inasmuch as the contacts between Mexico and the Southwest, in part at least, lie far back in time, were the Apache even in the Southwest at the period of early contacts? These questions certainly need an answer before Dr Opler's case for the Apache as carriers of culture to the Southwest can be completed.

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2 Page 705.  
3 Ibid.  
5 Carl Sauer, The Distribution of Aboriginal Tribes and Languages in Northwestern Mexico (Ibero-Americana: 5, Berkeley, 1934).  
6 Of interest on this point are also recent data on the cultural status and geographical position of the Apache in Alfred B. Thomas, After Coronado (Norman, 1935). See also William E. Dunn, Apache Relations in Texas (Texas State Historical Association Quarterly, July, 1910, April, 1911).
Against another imputation by Dr Opler, I have little defense. I refer to his implication that the treatment of the tabular summary in the “Comparative Ethnology” constitutes a statistical approach. No doubt statisticians would be pleased to deny this, but whether the techniques were present or not, the use of percentages involves at least a spiritual liason with the attitudes of statistics and so is open to the same criticisms.

As honest confession is said to be solacing, let it be said that the “statistics” in the “Comparative Ethnology” were worked out with one purpose in view. That purpose was to present in summarized form conclusions which had been reached as a result of an intensive working over of the cumbersome collection of data during which I had acquired a familiarity with them which it was impossible to expect of any but the most meticulous readers. Frankly, if it had not been possible to make these “statistics” present the judgements which had already been formed, they would not have been included in the paper.

The difficulties of any numerical approach to ethnographic data were, I believe, fully recognized, and to some extent were set forth in the footnote to page 139.

With one or two exceptions, in compiling this table [table 2], as well as tables 1 and 3, no trait occurring in less than three of the subareas named has been considered for comparative purposes, except traits occurring in southern Mexico, the Southwest, or South America and in only two of the north Mexican subareas. This limits the number of connections which would normally be shown between adjoining areas and emphasizes those between more distant areas—which is after all the principal interest of this paper. Naturally, these tables are little more than preliminary drafts to show the nature of the problems existing, but they do corroborate to a surprising degree the subjective evaluations reached by the author before they were prepared. They also have the usual fault of such tables in failing to weight adequately the various traits. An attempt has been made to weight agriculture by considering each plant cultivated a separate trait. Ideally, of course, each complex trait should be broken down into its simplest parts.

To this should, naturally, have been added recognition of the difficulty of equating similar traits in separate cultures.

With regard to those traits which Dr Opler questions in his list, his doubts would have been resolved to some extent had the traits been more competently defined in my own paper. Rain ceremonies of the Apache type may have a connection with those mentioned in Mexico, but they are hardly comparable with the essentially agricultural rites of the Pueblos and the Mexicans. The four day ritual fire of the Apaches during puberty ceremonies seems to equate much more closely with the four day fire during confinement of the Pueblos and Aztecs than with sacred temple fires. Data on sacred trees is too scanty in Mexico to judge the Apache case, but it hardly sounds like Mexican tree worship. The burning of aromatic herbs by the Apache is essentially a purificatory rite, while incense, as it is used in Mexico, is primarily an offering. Generically the concepts may be the same. A cactus or yucca leaf fence hardly seems to equate with a palisade of logs.

The really important point, however, is the relative meagerness of nomad cultures as exemplified by the Apache. Dr Opler has shown that the Apache possessed
DISCUSSION AND CORRESPONDENCE

a larger number of the traits in the selected list given in "The Comparative Ethnology of Northern Mexico before 1750" than could be credited them by use of the early sources. He has not shown, however, that the relative meagerness does not exist. He has merely shown that early documentary sources are inadequate substitutes for the work of the professional ethnographer. To this one may heartily subscribe. It is certainly true of the agricultural peoples of northern Mexico.

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A REPORT ON A LINGUISTIC EXPEDITION TO JAMES AND HUDSON'S BAYS

The following is a summary of the principal results of a linguistic expedition to James and Hudson's Bays in the summer and early fall of 1935, financed by the American Council of Learned Societies.

East of Hannah Bay, Cree leaves off and Montagnais-Nascaipi begins. As I surmised previously, the so-called Rupert's House Cree and East Main Cree are not Cree at all but Montagnais-Nascaipi dialects.² Beginning with Rupert's House, taking in Waswanipi (for Cooper proved Waswanipi is a Montagnais-Nascaipi dialect, and a y-dialect,³ though Davidson said when speaking of them "who speak a supposedly Cree dialect,"⁴), Mistassini, Nichigun, and continuing along the east coast of James and Hudson's Bays to a little beyond the Great Wale River, we have Montagnais-Nascaipi dialects in which original l is replaced by y. The dialects at Fort George and the Great Whale River form a dialect sub-group in which ë is entirely or largely replaced by a. The dialects of the Great Whale River and Fort George touch that of Fort Chimo in which original l is replaced by n (based on unpublished material of Turner), thus agreeing with the dialects of Davis Inlet and the Northwest River. Mistassini touches the dialect of Lake St. John in which original l is maintained. Moose Cree, as is well known, is a Cree dialect in which original l is maintained. The exact position of Cree at the Albany River is a little uncertain. Documentary evidence tends to show that it is a Cree dialect in which original l

¹ Printed by permission of the Smithsonian Institution.


is replaced by \( n \), but owing to the present-day diverging usage of Anglicans and Romanists it is impossible to be sure without going there in person. The few Albany Cree I met elsewhere disagreed, some claiming the language was the same as Moose Cree. However, the dialects at Attawapiskat, Wenusk (both unreported upon till now), Severn, and York Factory on the west side of James and Hudson's Bays are Cree dialects in which original \( l \) is replaced by \( n \), thus agreeing with the dialect at The Pas, Manitoba. Kesagami Cree, south of Moose Creek, is practically extinct, though it is still possible to obtain isolated words. It is an \( r \)-dialect, that is, one in which original \( l \) is replaced by \( r \). Owing to lack of data it is impossible to know whether Kesagami Cree is genealogically to be connected with Tête de Boule, also a Cree \( r \)-dialect (so Cooper and Michelson, as proposed to Davison), or whether the change from \( l \) to \( r \) is merely a parallel one. Similarly there are insufficient data to know whether the Cree dialect at Isle à la Crosse, also an \( r \)-dialect, genealogically belongs with Tête de Boule or not. The northern and eastern boundaries of Plains Cree, in which original \( l \) is replaced by \( y \), are at present nearly unknown; so we cannot bring it into relation with our other groups. According to Bloomfield, it extends (at least) as far north as the city of Prince Albert. It is, of course, well known that the Cree of the English River is a dialect in which original \( l \) is replaced \( tk \); the exact boundaries are unknown.

The Ojibwa spoken at the Albany River differs from that spoken in Wisconsin, Minnesota, etc. It may be noted that \( a \) is replaced by \( e \). There are no data available on other Ojibwa bands in this northern country; so it is useless to speculate on the interrelations.

The Eskimo spoken at the Great Whale River is distinct from that at Fort Chimo and that spoken on the Belcher Islands. The Eskimo spoken on James Bay is said to be the same as that spoken on Belcher Islands.

BUREAU OF AMERICAN ETHNOLOGY
WASHINGTON, D. C.

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NOTES AND NEWS

CONGRESS FOR THE STUDY OF THE FOLK-TALE AT LUND

At Lund (Sweden) on November 6–8, 1935 took place a Congress for the Study of the Folk-Tale. The Congress, which was held at the invitation of the Gustav Adolf Akademi för Folklivsforskning, was limited to twenty-five invited specialists. In the three days' session under the chairmanship of Dr C. W. von Sydow of Lund and Professor K. G. Westman of Uppsala various problems of folk-tale collecting, classifying, and comparative study were considered.

At a preliminary meeting at Uppsala a group of folklorists and anthropologists had met and brought to Lund proposals for the formation of an International Institute for the Folklore and Anthropology of Northern Europe and the European populations of America. Of this institute the field of folk-tale research is to be only one division. It is proposed that eventually the International Institute shall widen its scope to include all of Europe. The principal immediate goal is to make available at a central point all the various folklore archives, such as are now scattered in Dublin, Oslo, Copenhagen, Dorpat, Helsingfors, Prague, Gothenberg, Uppsala, Stockholm, Lund, and Edinburgh. The Folk-tale Congress agreed to cooperate with the new institute.

A committee was appointed to represent the folk-tale interests—for America, Professor Archer Taylor and the undersigned. The first meeting will take place in Edinburgh in June, 1937, under the auspices of the Scottish Anthropological Society. A new international quarterly journal, "Folk," is proposed.

STITH THOMPSON

APPROPRIATIONS FOR GRANTS-IN-AID BY THE NATIONAL RESEARCH COUNCIL

At its meeting in April, 1936, the Committee on Grants-in-aid of the National Research Council made the following grants in the field of anthropology.

Roy F. Barton, Institute of Anthropology, Ethnography and Archaeology, Academy of Sciences, Leningrad, USSR, "the somatology and material and social culture of the Ifugao tribe of the Philippine Islands;" T. M. N. Lewis, University of Tennessee, "archaeological investigations in Tennessee;" Dunbar Rowland and Moreau B. Chambers, Mississippi State Department of Archives and History, "archaeological survey of Mississippi;" William M. Shanklin, American University of Beirut, Beirut, Syria, "anthropological study of the living Near East races;" William S. Webb, University of Kentucky, "archaeological survey of the Tennessee River basin."

Since the funds which have been placed at the disposal of the National Research Council during the past few years for the making of research grants have been discontinued, there will be no further meetings of the Committee on Grants-in-Aid.

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GRANTS-IN-AID AND FELLOWSHIPS OF THE SOCIAL SCIENCE RESEARCH COUNCIL

Grants-in-aid were awarded by the Social Science Research Council inter alia to Roy F. Barton (see above); John P. Harrington, Bureau of American Ethnology, "study of the sociology of the Mission Indians of Southern California;" Paul Kosok, Long Island University, "work on the history of Indian caste systems;" Margaret Mead, American Museum of Natural History, "study of character formation in a homogeneous culture."

Applications for grants for 1937–1938 must be filed before January 15, 1937 on forms to be secured from the Secretary for Grants-in-Aid (230 Park Avenue, New York).

Post-doctoral fellowships were awarded to Ralph A. Bunche, Howard University "for study in Europe and Africa of advanced cultural anthropology, and field work in colonial policy and culture contacts in an East African tribe;" Lewis U. Hanke, Harvard University, "for study in United States and Latin America of the human geography and cultural anthropology of Latin America;" Thomas W. Wallbank, Santa Monica Junior College, "for study in England and Africa of the culture of native peoples in relation to current problems of colonial administration." Pre-doctoral field fellows are Horace M. Minor, University of Chicago, "for study in Canada of French Canadian communities;" Bernard Mishkin, Columbia University, "for study in New Guinea of the adjustment of youth to the social structure in a primitive society."

Applications for Post-Doctoral Research Training Fellowships and Post-Doctoral Field Fellowships should be made to the Fellowship Secretary before February 1, 1937, and for Pre-Doctoral Fellowships for Graduate Study before March 15, 1937.

RECENT DEATHS

We announce with regret the death of Dr Waldemar Bogoras, Institute of Anthropology and Ethnology, Academy of Sciences, Leningrad, at Kharkov, Ukraine, on May 12th, aged 71. Dr Bogoras is best known to American anthropologists for his monumental account of the Chuckchee of Siberia, published among the results of the Jesup North Pacific Expedition.

The regrettable death of Mr Samuel J. Guernsey, of the Peabody Museum of Harvard University, occurred May 22nd at Arlington, Massachusetts, at the age of 68. Mr Guernsey was best known for his contributions to the archaeology of the Basket Makers of the Southwest.
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