

PRIMITIVE SOCIETY

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PREFACE

ANTHROPOLOGISTS are hard put to it when asked to recommend a book that shall give the layman a brief summary of what is now known regarding their science as a whole or any one of its branches. They are usually obliged to confess that such an up-to-date synthesis as is likely to satisfy the questioner does not exist. In no department of anthropology has the want of a modern summary made itself more painfully felt than in that of social organization. Sociologists, historians, and students of comparative jurisprudence all require the data the anthropologist might supply, but for lack of a general guide they have been content to find inspiration in Morgan's *Ancient Society*, a book written when scientific ethnography was in its infancy. Since 1877 anthropologists have not merely amassed a wealth of concrete material but have developed new methods and points of view that render Morgan hopelessly antiquated. His work remains an important pioneer effort by a man of estimable intelligence and exemplary industry, but to get one's knowledge of primitive society therefrom nowadays is like getting one's biology from some pre-Darwinian naturalist. It is emphatically a book for the historian of anthropology and not for the general reader.

As I discovered during a year's lecturing at the University of California, the college student who takes anthropological courses suffers as grievously from the want of an introductory statement on primitive social organization as the interested layman or the investigator of neighboring

branches of knowledge. It is the requirements of these three classes of readers that I have had in mind in the preparation of the present volume, which purports to present the position of modern American workers.

I am naturally under obligations to more of my colleagues than can conveniently be named here. Above all I must acknowledge my indebtedness to my preceptor, Professor Franz Boas, the champion of scientific method in all anthropological research. To Dr. Clark Wissler of the American Museum of Natural History I owe abundant and varied field experience among North American Indians and a great deal of stimulation in our common field of specialist investigation, the Plains area. To Professor A. L. Kroeber I am indebted for the opportunity to lecture at the University of California during the academic year of 1917-18, which led to a systematization of my views on primitive society and thus indirectly to the present volume. Among many of my Berkeley associates from other departments I found quite unexpectedly a most encouraging interest in anthropological theory. More particularly, I was stimulated by my friend Professor Francis S. Philbrick, now of Northwestern University, whose broad knowledge of comparative jurisprudence helped greatly to enlarge my own vision of primitive law. Finally, I must express my obligations to my friend Mr. Leslie Spier of the American Museum of Natural History for reading and acutely criticising the typescript of chapters II to VIII before setting out on a field trip; and to Miss Bella Weitzner for the competent preparation of the index.

ROBERT H. LOWIE.

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CHAPTER I

INTRODUCTION

PRIMITIVE society is in a sense coextensive with primitive civilization. For civilization or culture, to substitute the ethnological term, is according to Tylor's famous definition "that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society"; whence it follows that a complete consideration of society involves a study of all the phases of civilization. No such stupendous task is here attempted. I will limit myself to those aspects of culture known as *social organization*, i.e., I will deal with the groups into which society is divided, the functions of these groups, their mutual relations, and the factors determining their growth.

Yet so closely are the several departments of civilization knitted together that concentration on any one of them to the exclusion of all others is an impracticable undertaking. Recent events have familiarized us with the mutual dependence of apparently disparate branches of culture. Military operations cannot be successfully conducted without the activities of the laboratory scientist and of the husbandman. In stages of lesser advancement the same principle holds. If we wish to study social organization it is impossible to ignore industrial factors because often society is organized precisely along industrial lines, into guilds of blacksmiths and architects, shipwrights and tattooers. Our concern, however, will not be with the technical processes employed

by these artisans, even though they are characteristic of the society to which they belong; we shall rather deal with the position of each body in the community, its comparative status of superiority or inferiority, its prerogatives and duties as one of a number of parallel or intercrossing aggregates. Similarly, if we have occasion to take notice of religious corporations, interest will not center in beliefs or observances, but in the position which the several groups occupy in the general polity. If we were to view Christianity from this angle, differences as to auricular confession or the theory of transubstantiation would figure mainly as group labels, while the rise to ascendancy in the state of one body of believers, the degradation of another, the disabilities of a third, would primarily engage our attention. Nevertheless it is impossible to anticipate how much knowledge of religion proper would prove necessary to illuminate the main problem, and unawares we might find ourselves plunged head over heels into the subtleties of scholastic disputation. It is not otherwise with savage peoples, and in order to gauge with accuracy the character of a social organization it is sometimes essential to take note of data representing all other phases of aboriginal activity.

Scientifically the study of primitive societies does not require justification. They *exist* and as part of reality Science is bound to take note of them. But the manner and spirit in which they have been regarded in the past have differed widely, and it will not be amiss to consider some of the ideals pursued in their investigation.

For one thing, it is possible to assume a predominantly monographic attitude. Some students fix their gaze upon a single people at a single epoch of its existence, and endeavor to describe this one culture with the utmost fidelity. In the higher reaches of this type of work the ethnographer becomes an artist who sympathetically penetrates into the latent spirit of his culture and creates a picture after the fashion of Gobineau's Renaissance. That is the ideal of

humanistic research acclaimed by the philosopher Windelband and his school. To them each manifestation of human history represents a unique phenomenon, an absolutely indefinable set of values that can merely be experienced through the visionary's intuition and then transmitted in fainter tints to his public. Ethnographic effort conducted in this spirit would result in a gallery of cultural portraits, each complete in itself and not related with the rest.

Such an attitude toward the data of civilization is by no means inconsistent with scientific aims, and inasmuch as it reveals the subtler phases of culture it may even contribute indispensable elements to a complete description of reality. But it is equally true that Science cannot rest content with this aesthetic immersion in distinct manifestations of human society. Indeed, a student passing successively from one of these reproductions to another would imperceptibly yield to a mental exercise quite different from the impulse that fired the painter in plumbing the individuality of his subject or from his own initial attempt at re-creation. Spontaneously comparison of later and earlier pictures would blend with merely absorptive processes. Against the martial cast of one culture would stand out the devotional twist of another or the blot of money-madness in a third. Resemblances would be noted as well as differences, and the question would imperatively obtrude itself how both are to be explained. In other words, phenomena would be not merely apperceived by themselves but viewed in their relations.

In part it would be a problem of causal relations. It is natural to suppose that like phenomena must have like causes and accordingly it would become the ethnologist's duty to determine these: a priori they might be supposed to lie in racial affinity, or the similarity of geographical environment, or some other fundamental condition shared by the cultures compared. Practically, however, as will appear later, it is not so easy to isolate such determinants

amidst the tremendous complexities of cultural data and to demonstrate that they are the significant factors. Indeed, some ethnologists have abandoned all hope of ever unraveling them. But whether the quest of causal relations be a hopeless one or not, one kind of relation can never be ignored by the scientific student of culture—the chronological one. Assume that our cultural picture gallery contains delineations of all distinguishable cultures. It would then embrace separate pictures of the successive cultures of the same people. Aesthetic contemplation might rest content with apperceiving the picture of Japan in 1000 A.D. and the picture of Japan in 1900 as representing two disparate embodiments of cultural ideals as independent of each other as either is of the Italian Renaissance. But that could not possibly be the attitude of the scientific student. To him the fact that one culture has grown out of another, that the same culture has varied with time, is an all-important fact; without a knowledge of the time relations of cultures that are merely links of one chain he would feel that he had missed the most essential part of reality. To put it tersely, whatever else the investigator of civilization may do, he *must* be an historian.

But what kind of an historian shall the ethnologist be? Some eminent savants whose thinking has been moulded in experimental laboratories have prescribed with much emphasis what kind of history is worth while. Accustomed to seeing physical phenomena described in the stenographic equations of the calculus, they cannot conceive of any branch of knowledge as worth a candle unless it conforms to the pattern of celestial mechanics. Says Professor Pearson in *The Grammar of Science*: "History can never become science, can never be anything but a catalogue of facts rehearsed in more or less pleasing language until these facts are seen to fall into sequences which can be briefly resumed in scientific formulae." Applying his tenet specifically to civilization, this author contends that in

broad outline the development of man has followed the same course in Europe, in Africa, in Australasia; that it can be briefly resumed in terms of certain basic principles; and that except in so far as the historian undertakes to ascertain these, his efforts are hardly worthy of serious consideration. Similar opinions have been voiced by Professor Ostwald, the chemist, and Dr. Driesch, the zoologist.

The attitude just defined displays a surprising naïveté. No doubt ethnologists and other historians would be greatly at fault if they failed to discover the laws underlying civilization, thus giving to their data the highest degree of coördination to which they are amenable. But the first question is whether any such laws exist and what measure of coördination is feasible. The existence of uniformity in culture history cannot be assumed simply because it would be convenient. Even in physics the investigator is not always fortunate enough to reduce his phenomena to a Newtonian formula. He must theoretically accept the fact that water has its point of maximum density at four degrees Centigrade, as men at large have had to reckon with it practically, without waiting until water shall assume the properties of other liquids. So the ethnologist cannot permit his task to be pre-determined for him. If there are laws of social evolution, he must assuredly discover them, but *whether* there are any remains to be seen, and his scholarly position remains unaffected by their non-existence. His duty is to ascertain the course civilization actually has followed; and the kind of synthesis he gives must depend on the nature of his facts. To strive for the ideals of another branch of knowledge may be positively pernicious, for it can easily lead to that factitious simplification which means falsification. It would be equivalent to insisting that water *must* condense in freezing. If every people of the globe had a culture history wholly different from that of every other, the historian's task would still be to record these singularities and make the best of them; and in contributing his

share to the sum total of knowledge he would suffer no loss in scientific dignity from the unmalleability of his material. Without, therefore, at the outset renouncing the search for laws of social evolution, we will emphatically declare our independence of that pseudo-scientific dogmatism which insists on formulating all phenomena after the fashion that has proved serviceable in a diminutive corner of the field of human knowledge. Uninfluenced by any bias for or against historical regularities, we shall attempt to determine what are the facts and what has been their actual sequence.

Here, however, the ethnologist encounters an obstacle from which the historian of the higher civilizations is exempt. The succession of events in primitive communities is rarely a matter of recorded knowledge except for the most recent period, and when positive information extends back to several centuries ago the student considers himself unusually fortunate. This presents a real difficulty but not an insurmountable one. For in addition to the sparse documentary sources the ethnologist possesses a stock of established ethnographic and linguistic fact, and when this is combined with the data of geographical distribution it is often possible to reconstruct history with practical certainty. With regard to phenomena of social organization instances will be supplied in later chapters; I will therefore elucidate the method by a technological illustration. In smelting iron the natives of Madagascar employ the piston-bellows, a type quite different from the bellows of the Negro blacksmiths of the neighboring African continent. In a splendid example of historical reconstruction Tylor pointed out that the piston-bellows occurs also in Sumatra, in other parts of the Malay Archipelago, and the adjacent portion of the Asiatic mainland; and that anthropologically and linguistically the Malagasy of Madagascar are members of the Malay family. Hence the piston-bellows is undoubtedly a Malay invention, which was carried by the Malays to various regions in the

course of their migrations. By thus combining general anthropological knowledge with knowledge of the distribution of a trait Tylor succeeded in establishing the history of a mechanical contrivance beyond any reasonable doubt.

In the historical reconstruction of culture the phenomena of distribution play, indeed, an extraordinary part. If a trait occurs everywhere, it might veritably be the product of some universally operative social law. If it is found in a restricted number of cases, it may still have evolved through some such instrumentality acting under specific conditions that would then remain to be determined by analysis of the cultures in which the feature is embedded. On the other hand, as in the instance of the Malagasy bellows, there may be no law involved but a question of genetic relationship. Finally, the sharers of a cultural trait may be of distinct lineage but through contact and borrowing have come to hold in common a portion of their cultures.

Thus the data as to distribution demand an interpretation, whether in terms of some causal factor, or of tribal affinity or international intercourse; and the answer elicited with the aid of extraneous ethnological information is necessarily cast in historical form. If we were tracing the history of ironwork, we should assign to the Malay bellows a relatively late date because it is a specialized form evolved in a region of Asia remote from the ancient centers of metallurgy; and we should regard the Malagasy bellows as a relatively recent importation because Madagascar represents the farthest outpost of Malay civilization.

Since, as a matter of fact, cultural resemblances abound between peoples of diverse stock, their interpretation commonly narrows to a choice between two alternatives. Either they are due to like causes, whether these can be determined or not; or they are the result of borrowing. A predilection for one or the other explanation has lain at the bottom of much ethnological discussion in the past; and at present

influential schools both in England and in continental Europe clamorously insist that all cultural parallels are due to diffusion from a single center. It is inevitable to envisage this moot-problem at the start, since uncompromising championship of either alternative has far-reaching practical consequences. For if every parallel is due to borrowing, then sociological laws, which can be inferred only from independently developing likenesses, are barred. Then the history of religion or social life or technology consists exclusively in a statement of the place of origin of beliefs, customs and implements, and a recital of their travels to different parts of the globe. On the other hand, if borrowing covers only part of the observed parallels, an explanation from like causes becomes at least the ideal goal in an investigation of the remainder. It is therefore proper to justify in the beginning whatever position one is inclined to take in the conflict between the rival theories of diffusion and independent evolution.

The great strength of the diffusionist theory lies in the abundance of evidence that transmission has played an enormous part in the growth of cultures. This is often not merely a matter of inference but of recorded observation, as in the influence of Egyptian on Greek, or of Arabian on mediaeval European civilization. To this vast body of testimony must be added numerous examples of borrowing established by inference but in a manner that admits of no doubt. Whenever a well-defined trait is distributed over a continuous area, the conclusion can hardly be avoided that it has developed in one spot within that area and has thence traveled to its confines. Often that conclusion is corroborated by a quantitative test: the feature in question is found in a state of high elaboration about the center of origin and dwindles towards the periphery. Thus, Professor Boas has demonstrated with great elegance that the Raven cycle of Canadian mythology originated about the northern part of British Columbia and thence traveled southwards. The

farther one proceeds from the point of origin the smaller is the degree of elaboration of the cycle until it finally tapers away. This combination of legendary adventures could not be confined to a narrow coastal strip if it were the product of some law of myth-making; and there would not be noticeable that progressive diminution of complexity if we were not dealing with a case of successive transmission to districts farther and farther removed from the fountain-head.

Diffusion must accordingly be hailed as a *vera causa*. But is it the only one? What shall we say when like traits crop up in widely severed regions of which the populations are neither racially related nor have ever been in contact so far as is known? In that contingency the diffusionist must have recourse to the auxiliary hypothesis that contact at one time existed; and he does so because of his conviction that every element of culture is ultimately due to so extraordinary a confluence of circumstances that the conditions for a second invention can never recur. This is the basic tenet of the diffusionist creed that we must face.

It may at once be admitted that some of the arguments leveled at this position in the past have not been especially fortunate. Thus, the duplication of scientific discoveries has been cited to prove that the same feature may develop independently. Yet in general this argument lacks cogency. A careful historical examination usually shows that the co-discoverers both borrowed largely from the same cultural stock, as did Newton and Leibnitz in the discovery of the calculus. Such a case, then, cannot be likened to the independent creation of cultural elements in completely separated areas. Further, when modern scientists duplicate each other's results they are not merely building on the same foundation but are trained workers who consciously seek to add definite stones to the structure. This deliberate striving on the basis of special training is a motive that must be wholly banished in considering the ruder civilizations,