

CHAPTER I

Introduction: History and Anthropology

MORE THAN A HALF-CENTURY has elapsed since Hauser and Simiand formulated and contrasted the principles and methods which seemed to them to distinguish history from sociology. These differences stemmed primarily from the comparative nature of the sociological method, on the one hand, and the documentary and functional character of the historical method, on the other.¹ While the two authors agreed on the contrasting nature of these disciplines, they diverged in evaluating the respective merits of each method.

What has happened since then? We must acknowledge that history has confined itself to its original modest and lucid program and that it has prospered by adhering to it closely. From the vantage point of history, problems of principle and method appear to have been definitely resolved. What has happened to sociology, however, is another matter. Those branches of sociology with which we shall be particularly concerned here, ethnography and ethnology, have, during the last thirty years, produced a great



number of theoretical and descriptive studies. This productivity, however, has been achieved at the price of conflicts, cleavages, and confusions which duplicate, within anthropology itself, the traditional and far more clear-cut dispute that set off ethnology as a discipline separate from history. Just as paradoxically, the historians' theories have been taken over literally by anthropologists, and particularly by those anthropologists who proclaim their opposition to the historical method. This situation will be more easily understood if we briefly trace its origins and, for the sake of clarity, sketch some preliminary definitions.

In this discussion we shall not use the term *sociology*, which has never come to stand, as Durkheim and Simiand hoped it would, for a general science of human behavior. If *sociology* is taken to mean examination of the principles of social life and the ideas which men either have entertained or now entertain with respect to it—and this interpretation is still current in several European countries—then *sociology* can be equated with social philosophy and thus it falls outside our scope. If, on the other hand, *sociology* is considered, as it is in the Anglo-Saxon countries, as the corpus of all the empirical research bearing on the structure and functioning of the more complex societies, it becomes a branch of ethnography. In the latter case, precisely because of the complexity of its subject matter, it cannot yet aspire to findings so concrete and varied as those of ethnography, which, at least from a methodological standpoint, have greater value.

We have yet to define ethnography itself and ethnology. Let us distinguish them briefly and tentatively—this being sufficient at the outset—by stating that ethnography consists of the observation and analysis of human groups considered as individual entities (the groups are often selected, for theoretical and practical reasons unrelated to the nature of the research involved, from those societies that differ most from our own). Ethnography thus aims at recording as accurately as possible the respective modes of life of various groups. Ethnology, on the other hand, utilizes for comparative purposes (the nature of which will be explained below) the data provided by the ethnographer. Thus, *ethnography* has the same meaning in all countries, and *ethnology* corresponds approximately to what is known in Anglo-Saxon countries—where the term *ethnology* has become obsolete—as social or cultural anthropology.

Social anthropology is devoted especially to the study of institutions considered as systems of representations,² cultural anthropology, to the study of techniques which implement social life (and, sometimes also, to the study of institutions considered as such techniques). Finally, it is obvious that if the data resulting from the objective study of both complex societies and so-called primitive societies should ever be successfully integrated to provide universally valid conclusions from a synchronic or diachronic point of view, then sociology, having attained its positivistic form, would automatically acquire the crowning position among the social sciences that its scholars have always coveted. But we have not yet reached that point.

After these preliminary remarks and definitions, we can formulate the problem of the relationship between the anthropological sciences and history as follows: Either anthropology is focused on the diachronic dimension of phenomena, that is, on their temporal order, and thus is unable to trace their history; or anthropologists attempt to apply the method of the historian, and the time dimension escapes them. The problem of reconstructing a past whose history we are incapable of grasping confronts ethnology more particularly; the problem of writing the history of a present without a past confronts ethnography. That is, at any rate, the dilemma which has too often halted the development of these sciences in the course of the last fifty years.

This contradiction has not been stated here in terms of the classical opposition between evolutionism and diffusionism, because from this point of view the two schools converge. The evolutionist interpretation in anthropology clearly derives from evolutionism in biology.³ Western civilization thus appears to be the most advanced expression of the evolution of societies, while primitive groups are "survivals" of earlier stages, whose logical classification reflects their order of appearance in time. But the task is not so simple. The Eskimo, while excellent technicians, are poor sociologists; the reverse is true of the natives of Australia. One could cite many such examples. With an unlimited choice of criteria an unlimited number of evolutionary sequences could be constructed, all of them different. Nor does Leslie White's neo-evolutionism⁴ seem able to overcome this difficulty. Although the criterion which he

suggests, namely, the amount of energy harnessed per capita in each society, corresponds to an ideal found in certain historical periods and valid for certain aspects of Western civilization, it does not apply to the great majority of human societies, for which the proposed standard would seem to entirely lack significance.

An alternative approach is to break down cultures into abstract elements and to establish, between elements of the same type in different cultures, rather than between cultures themselves, the same kind of relationships of historical descent and progressive differentiation which the paleontologist sees in the evolution of species. For the ethnologist, Tylor says:

The bow and arrow is a species, the habit of flattening children's skulls is a species, the practice of reckoning numbers by tens is a species. The geographical distribution of these things, and their transmission from region to region, have to be studied as the naturalist studies the geography of his botanical and zoological species.⁶

But nothing is more dangerous than this analogy. For even if the concept of species should be discarded once and for all in the development of genetics, what made—and still makes—the concept valid for the natural historian is the fact that a horse indeed begets a horse and that, in the course of a sufficient number of generations, *Equus caballus* is the true descendant of *Hipparrison*. The historical validity of the naturalist's reconstructions is guaranteed, in the final analysis, by the biological link of reproduction. An ax, on the contrary, does not generate another ax. There will always be a basic difference between two identical tools, or two tools which differ in function but are similar in form, because one does not stem from the other; rather, each of them is the product of a system of representations. Thus the European fork and the Polynesian fork (which is used in ritual meals) do not constitute a species, any more than do the straws through which one sips lemonade at a café, the "bombilla" to drink maté, and the drinking tubes used for ritual purposes by some American Indian tribes. The same is true of institutions. We cannot classify under the same rubric the custom of killing the old for economic reasons and that of hastening their entrance into the joys of the other world.

Therefore when Tylor writes, "When a general law can be inferred from a group of facts, the use of detailed history is very

much superseded. When we see a magnet attract a piece of iron, having come by experience to the general law that magnets attract iron, we do not take the trouble to go into the history of the particular magnet in question,"⁶ he is really forcing us into a blind alley. For, unlike the physicist, the anthropologist is still uncertain which of the objects of his study correspond to the magnet and which to the iron, and, furthermore, whether it is possible to identify objects which at first appear to be two magnets or two pieces of iron. Only a "detailed history" would enable the anthropologist to overcome his doubts in each case. The critical evaluation of the concept of totemism has long provided an excellent example of this difficulty. If one limits the application of the totemic concept to irrefutable cases where the institution appears with all its characteristics, these cases are too special to permit the formulation of a law of religious evolution. If, on the other hand, one extrapolates only from certain constituent elements, it becomes impossible to know, without a "detailed history" of the religious ideas of each group, whether animal or plant names and practices or beliefs referring to animal or plant species can be explained as vestiges of an earlier totemic system or in terms of entirely different causes, such as, for example, the logico-aesthetic tendency of the human mind to classify into categories the physical, biological, and social entities which constitute its universe. (A classic study by Durkheim and Mauss has demonstrated the universality of this mode of thinking.⁷)

Actually, the evolutionist and diffusionist interpretations have a great deal in common. Tylor, indeed, formulated and applied them side by side. Both approaches differ from the historian's methods. The historian always studies individuals, whether these be persons, events, or groups of phenomena individualized by their location in space and time. The diffusionist breaks down the species developed in the comparative method in order to reconstruct individuals with fragments borrowed from different categories. But he never succeeds in building more than a pseudo-individual, since the spatial and temporal coordinates are the result of the way the elements were chosen and assembled, instead of being the reflection of a true unity in the object. The "cycles" or cultural "complexes" of the diffusionist, like the "stages" of the evolutionist, are the product of an abstraction that will always lack the corroboration of empirical evidence. Their history remains conjectural and

ideological. This qualification applies even to the more modest and rigorous studies—such as those by Lowie, Spier, and Kroeber—of the distribution of certain cultural traits in limited areas of North America.⁸ This is true not so much because we can never conclude that events actually occurred in the way suggested by the proposed reconstruction—for it is always legitimate to formulate hypotheses, and, at least in some instances, the points of origin and the extent of diffusion which are postulated hold a high degree of probability; such studies deceive us because they do not teach us anything about the conscious and unconscious processes in concrete individual or collective experiences, by means of which men who did not possess a certain institution went about acquiring it, either by inventing it, by modifying previous institutions, or by borrowing from the outside. This kind of research seems to us to be one of the essential goals of the ethnographer as well as of the historian.

No one has contributed more than Boas toward exposing these contradictions. Thus, a brief analysis of his major tenets will enable us to find out to what extent he himself was able to escape such dilemmas and whether they are inherent in the nature of all ethnographic research.

Referring to history, Boas begins with a proclamation of humility: "As a matter of fact, all the history of primitive peoples that any ethnologist has ever developed is reconstruction and cannot be anything else."⁹ And to those who object that he has not studied the history of this or that aspect of a civilization to which he has, nevertheless, devoted the greater part of his life, he gives this heroic answer: "Unfortunately we have not found any data that throw light on its development."¹⁰ But once these limitations are recognized, it becomes possible to define a method whose application, though no doubt limited in scope by the exceptionally unfavorable conditions under which the anthropologist works, may still yield findings. The detailed study of customs and of their place within the total culture of the tribe which practices them, together with research bearing on the geographical distribution of those customs among neighboring tribes, enables us to determine, on the one hand, the historical factors which led to their development

and, on the other, the psychological processes which made them possible.¹¹

To be legitimate, such research should be restricted to a small region with clearly defined boundaries, and comparisons should not be extended beyond the area selected for study. Actually, the recurrence of similar customs or institutions cannot be held as a proof of contact unless there is a continuous chain of traits of the same type which allows us to relate the polar traits through a series of intermediaries.¹² We shall probably never achieve chronological certainty, but it is possible to obtain high probabilities with reference to phenomena, or groups of phenomena, of limited distribution in time and space. The evolution of secret societies among the Kwakiutl was traced over a period of a half-century. Hypotheses bearing on the relationship between the cultures of northern Siberia and the Northwest Coast have been formulated; and the itineraries followed by one or another mythical theme of North America have been reasonably reconstructed.

Nevertheless, such thorough inquiry rarely reaches the point of truly recreating history. In the entire work of Boas the result appears to be rather negative. Among the Pueblo Indians of the Southwest, as well as among the tribes of Alaska and British Columbia, it has been noted that social organization takes extreme and contrasting forms at the two ends of the territory under consideration and that the intermediary regions present a series of transitional forms. Thus, the western Pueblo have matrilineal clans without moieties and the eastern Pueblo patrilineal moieties without clans. The northern part of the Pacific Coast is characterized by few clans and a large number of local groups with clearly defined privileges, while the southern part has a bilateral organization and local groups without marked privileges.

Can we draw the conclusion that one type has evolved from the other? For such a hypothesis to be legitimate we should have to be able to prove that one type is more primitive than the other; that the more primitive type evolves necessarily toward the other form; and, finally, that this law operates more rigorously in the center of the region than at its periphery. Failing this threefold and impossible demonstration, any theory of survivals is futile, and in this particular case the facts support no reconstruction tending, for exam-

ple, to assert the historical priority of matrilineal over patrilineal institutions: "All that can be said is that fragments of earlier historical stages are bound to exist and are found."¹³ While it is possible and even likely that the instability inherent in matrilineal institutions often leads to their transformation into patrilineal or bilateral institutions, it can by no means be concluded that, always and everywhere, matrilineal descent represents the primitive form.¹⁴

Such a critical analysis is conclusive. Nevertheless, if it were carried to an extreme, it would lead to complete historical agnosticism. For Boas, however, this kind of analysis is directed against the alleged universal laws of human development and generalizations based on what he once called "a 40 per cent possibility,"¹⁵ not against modest and conscientious effort at historical reconstruction within the bounds of precise and limited objectives. What are, according to Boas, the conditions necessary for such effort? He recognizes that in ethnology ". . . evidence of change can be inferred only by indirect methods," that is, as in comparative linguistics, by analysis of static phenomena and study of their distribution.¹⁶ But we should not forget that Boas, a geographer by training and a disciple of Ratzel, became aware of his anthropological vocation during the course of his first field work, as a result of a flash of insight into the originality, uniqueness, and spontaneity of social life in each human group. These social experiences and these constant interactions between the group and the individual can never be inferred; they must be observed. As he once said, "In order to understand history it is necessary to know not only how things are, but how they have come to be."¹⁷

We are now in a position to explore the character of Boas' thought and to bring out its paradoxical quality. By university training a physicist as well as a geographer, he ascribes a scientific aim and universal scope to anthropological research. "He himself often said that the problem was the relation between the objective world and man's subjective world as it had taken form in different cultures."¹⁸

But while he aspired to apply to this subjective world the rigorous methodology that he had learned in the natural sciences, he recognized the infinite variety of historical processes which shapes it in each case. Knowledge of social facts must be based on

induction from individualized and concrete knowledge of social groups localized in time and space. Such specific knowledge, in turn, can be acquired only from the history of each group. Yet such is the nature of the subject-matter of ethnographic studies that in the vast majority of cases history lies beyond reach. Boas introduces the standards of the physicist in tracing the history of societies for which we possess only documents that would discourage the historian. When Boas is successful, his reconstructions amount to true history—but this is a history of the fleeting moment, the only kind of history that can be captured immediately—in other words, a *microhistory*, which can no more be related to the past than can the *macrohistory* of evolutionism and diffusionism.¹⁹

In this demanding enterprise to overcome contradictory requirements with rigor, toil, and genius, the work of Boas continues, and will long continue, to dominate from its monumental heights all subsequent developments. In any event, the developments of recent years can only be understood as efforts to escape the dilemma which he himself formulated without recognizing its inevitable character. Thus, Kroeber tried to loosen somewhat the rigid criteria of validity which Boas imposed for historical reconstructions, justifying his method with the observation that, after all, the historian, who with a wealth of documents to help him occupy a much more secure position than the anthropologist, is far from being so exacting.²⁰ Malinowski and his school, along with most of the contemporary American school, chose the opposite direction. Since Boas' work itself demonstrated the extent to which it was deceptive to seek knowledge about "how things have come to be," they renounced "understanding history" in order to convert the study of cultures into a synchronic analysis, in the present, of relationships between their constituent elements. The whole question is to know whether, as Boas so profoundly observed, even the most penetrating analysis of a unique culture—which includes description of its institutions and their functional interrelations, as well as study of the dynamic processes by which culture and the individual interact—can attain full significance without knowledge of the historical development underlying the present patterns.²¹ This essential point will become clearer from the discussion of a specific problem.

The term *dual organization* has been ascribed to a type of social structure frequently found in America, Asia, and Oceania and characterized by the division of the social group—whether tribe, clan, or village—into two moieties, whose respective members have relationships which may range from the most intimate cooperation to latent hostility, and which generally contain both types of behavior. Sometimes the purpose of moieties seems to be the control of marriage, in which case they are termed exogamous. Sometimes their role is confined to activities of a religious, political, economic, ceremonial, or merely recreational character, or even to one or another of these activities only. Membership in a moiety is transmitted in some instances by matrilineal descent, in others by patrilineal descent. The division into moieties may or may not coincide with clan organization. It may be simple or complex, in which case several pairs of moieties will cross-cut one another, each pair having different functions. In short, almost as many kinds of dual organization are known as peoples possessing it. Where then does it begin and where does it end?

Let us immediately rule out evolutionist and diffusionist interpretations. The evolutionist, who tends to consider dual organization as a necessary stage of social development, would first have to define a simple form whose actually observed forms would be concrete manifestations, survivals, or vestiges. He would then have to postulate the presence, at a remote time, of this form among peoples where nothing demonstrates that a moiety division ever existed. The diffusionist, in turn, would select one of the observed types, usually the most developed and complex, as representing the archaic form of the institution and would attribute its origin to that region of the world where it is best documented, all other forms being considered the product of migrations and borrowings from the common cradle. In both cases, one arbitrarily selects a type from all those provided by experience and makes of this type the model from which one attempts, through speculation, to derive all the others.

Are we then compelled to carry Boasian nominalism to its limit and study each of the cases observed as so many individual entities? We should be aware, first, that the *functions* assigned to dual organization do not coincide, and, second, that the *history* of

each social group demonstrates that the division into moieties stems from the most different origins.²² Thus, depending on the case, dual organization may be the result of the invasion of a population by an immigrant group; of fusion between two neighboring groups, for any of several reasons (economic, demographic, or ceremonial); of the crystallization, in institutional form, of empirical norms designed to insure marriage exchanges within a given group; of the distribution within the group—over two parts of the year, two types of activities, or two segments of the population—of two sets of antithetical behavior, each of which is considered equally indispensable for the maintenance of social equilibrium; and so forth. We are therefore forced to reject the concept of dual organization as a spurious category and, if we extend this line of reasoning to all other aspects of social life, to reject *institutions* exclusively, in favor of *societies*. Ethnology and ethnography—the former thus reduced to the latter—would be no more than history reduced to such a level, owing to the lack of written or graphic documents, that it would no longer be worthy of the name.

Malinowski and his followers have rightly protested against this abdication. But we might ask them whether, by banning all history on the premise that ethnologists' history is not worthy of consideration, they have not gone to too great extremes. For one of two things will occur: The functionalists may proclaim that all anthropological research must be based on painstaking study of concrete societies and their institutions—including the interrelations of those institutions, their relationships with custom, belief, and technology, and the interrelations between the individual and the group and among individuals within the group. In this case, they are merely doing what Boas recommended in these same terms in 1895 and what the French school, under Durkheim and Mauss, also advised in those days—in other words, sound ethnography. Malinowski at the beginning of his career did some admirable ethnography, especially in his *Argonauts of the Western Pacific*. But we fail to see in what way he transcended Boas' theoretical position.

Or the functionalists may find salvation in their asceticism and, by an unheard-of miracle, do what every good ethnographer must do and does; but they stubbornly refuse to consider any his-



torical information regarding the society under study or any comparative data borrowed from neighboring or remote societies. In this way they claim to achieve, through inner meditation, those general truths whose possibility Boas never denied, but which he placed at the end of an undertaking so vast that all primitive societies will no doubt have disappeared long before appreciable progress has been made. Such, indeed, is Malinowski's attitude. His belated cautiousness²³ cannot temper his many ambitious proclamations. This is also the attitude of many an anthropologist of the younger generation who disdains study of any source materials or regional bibliographies before going into the field. He does this in order not to spoil the wonderful intuition that will enable him to grasp eternal truths on the nature and function of social institutions through an abstract dialogue with his little tribe, over and beyond a context of highly differentiated norms and customs, each of which possesses, nevertheless, countless variants among neighboring or remote peoples. But did not Malinowski label "herodotage" the curiosity for "primitive eccentricities of man"? ²⁴

When one confines oneself to the study of a single society, one may do valuable work. Experience shows that the best monographs are generally produced by investigators who have lived and worked in one particular region. But they must forgo conclusions about other regions. When, in addition, one completely limits study to the present period in the life of a society, one becomes first of all the victim of an illusion. For everything is history: What was said yesterday is history, what was said a minute ago is history. But, above all, one is led to misjudge the present, because only the study of historical development permits the weighing and evaluation of the interrelationships among the components of the present-day society. And a little history—since such, unfortunately, is the lot of the anthropologist—is better than no history at all. How shall we correctly estimate the role, so surprising to foreigners, of the *apéritif* in French social life if we are ignorant of the traditional prestige value ascribed to cooked and spiced wines ever since the Middle Ages? How shall we analyze modern dress without recognizing in it vestiges of previous customs and tastes? To reason otherwise would make it impossible to establish what is an essential distinction between primary function, which corresponds to a present need of the social body, and secondary function, which



survives only because the group resists giving up a habit. For to say that a society functions is a truism; but to say that everything in a society functions is an absurdity.

The danger of the truism, which threatens functionalist interpretations, was aptly pointed out by Boas: ". . . the danger is ever present that the widest generalizations that may be obtained by the study of cultural integration are commonplaces."²⁵ Because such general characteristics *are* universal, they pertain to biology and psychology. The ethnographer's task is to describe and analyze the different ways in which they are manifested in various societies; the ethnologist's task is to explain them. But what have we learned about the "institution of gardening" when we are told that it is "universally found wherever the environment is favorable to the cultivation of the soil and the level of culture sufficiently high to allow it"?²⁶ Or about the outrigger canoe, its multiple forms, and its peculiar distribution, when it is defined as a canoe whose "arrangement gives the greatest stability, seaworthiness and manageability, considering the limitations in material and in technical handicraft of the Oceanic cultures"?²⁷ Or about the nature of society in general and the infinite variety of manners and customs, when we are confronted by this statement: "The organic needs of man [Malinowski lists nutrition, defense and comfort, mating and propagation] form the basic imperatives leading to the development of culture . . ."?²⁸ Yet these needs are common to both man and animals. It might also be submitted that one of the essential tasks of the ethnographer is to describe and analyze the complicated marriage rules and associated customs in various societies. Malinowski rejects this: "To put it bluntly, I should say that the symbolic, representative or ceremonial contents of marriage are of secondary importance to the anthropologist. . . . The real essence of the marriage act is that by means of an extremely simple or highly complicated ceremony it gives a public, tribally recognized, expression to the fact that two individuals enter the state of marriage."²⁹ Why then bother going to distant places? And, according to this point of view, would the 603 pages of *The Sexual Life of Savages in Northwestern Melanesia* be worth very much? In the same way, should we dismiss the fact that some tribes permit pre-marital sexual freedom while others require chastity, on the premise that these customs can be reduced to one function, that of insuring



permanent marriage? ³⁰ What interests the anthropologist is not the universality of the function—which is far from definitely established, and which cannot be asserted without a careful study of all the customs of this type *and their historical development*—but, rather, the fact that the customs are so varied. It is true that a discipline whose main, if not sole, aim is to analyze and interpret differences evades all problems when it takes into account only similarities. But at the same time it thus loses the means of distinguishing between the general truths to which it aspires and the trivialities with which it must be satisfied.

It may be said, perhaps, that these unfortunate incursions into the field of comparative sociology are exceptions in Malinowski's work. But the idea that empirical observation of a single society will make it possible to understand universal motivations appears continually in his writings, weakening the significance of data whose vividness and richness are well known.

The ideas held by the Trobriand Islanders concerning the value and respective function of each sex in the society are highly complex. They take pride in numbering in their clans more women than men, and are unhappy when there are fewer women. At the same time, they hold male superiority as an accepted fact: Men possess an aristocratic virtue which their wives lack. Why should such subtle observations be blunted by the brutal and contradicting introductory statement that "For the continuation and very existence of the family, woman as well as man is indispensable; therefore both sexes are regarded by the natives as being of equal value and importance"? ³¹ The first part of this statement is a truism, and the second is not consistent with the facts reported. Few topics have attracted Malinowski's attention as much as that of magic, and throughout his whole work one finds reiterated constantly the argument that all over the world,³² as well as in the Trobriand Islands, magic is used for "all important activities and enterprises in which man has not the issue firmly and safely in hand. . . ." ³³ Let us set aside the general thesis to consider its application to the specific case.

The men of the Trobriand Islands, we are told, employ magic for gardening, fishing, hunting, canoe-building, safety at sea, woodcarving, sorcery, and weather; women use magic for abor-



tion, toothache, and skirtmaking.³⁴ These activities represent only a small fraction of those "in which man has not the issue firmly and safely in hand"; but even from this point of view they are not comparable. Why the making of straw skirts and not the preparation of dried gourds or pottery, which has an equally uncertain outcome? Can one arbitrarily state that a better knowledge of the history of religious thought in Melanesia, or of data from other tribes (both of which reveal the role often attributed to plant fiber as the symbol of a change of status),³⁵ would not throw some light on the reasons why these specific activities are carried out with the aid of magic? Let us cite two other examples which illustrate the flaws in this intuitive method. In Malinowski's book on the sexual life of Melanesians, we learn that one of the principal reasons for marriage, here as elsewhere, is ". . . the natural inclination of a man past his first youth to have a house and a household of his own . . . and . . . a natural longing for [children]."³⁶ But in *Sex and Repression in Savage Society*, in which a theoretical commentary is added to the field-work account, we read the following: "In man this need for an affectionate and interested protector of pregnancy still remains. That the innate mechanism has disappeared we know from the fact that in most societies . . . the male refuses to take any responsibility for his offspring unless compelled to do so by society."³⁷ An odd natural longing, indeed!

The followers of Malinowski are unfortunately not exempt from the curious combination of dogmatism and empiricism that weakens his entire system. When Margaret Mead, for instance, characterizes three neighboring societies of New Guinea in terms of the different and complementary combinations of relationship between the sexes (passive man, passive woman; aggressive man, aggressive woman; aggressive woman, passive man), we admire the elegance of this construction.³⁸ But our suspicion of oversimplification and *a priori* thinking is confirmed by other observations, such as those that stress the existence of a specifically female piracy among the Arapesh.³⁹ And when the same author classifies North American tribes as competitive, cooperative, and individualistic,⁴⁰ she remains as far removed from a true taxonomy as a zoologist who would define species by grouping animals as solitary, gregarious, and social.

We really wonder whether all these hasty constructions,

which only result in making the peoples studied "reflections of our own society,"⁴¹ of our categories and our problems, do not proceed, as Boas profoundly perceived, from an overestimation of the historical method, rather than from the opposite attitude. For, after all, it was the historians who formulated the functionalist method. After enumerating the complex of traits that characterized a certain stage of Roman society, Hauser wrote in 1903: "All this constitutes an irreducible complex; all these traits are mutually explanatory to a much greater extent than the evolution of the Roman family can be explained in terms of the Semitic, or Chinese, or Aztec family."⁴² This statement could have been written by Malinowski, with the one exception that Hauser added events to institutions. And, undoubtedly, his statement requires a double qualification: For what is true of *process* is not so true of *structure*, and for the anthropologist comparative studies compensate to some extent for the absence of written documents. But the paradox remains, nevertheless: The criticism of evolutionist and diffusionist interpretations has showed us that when the anthropologist believes he is doing historical research, he is doing the opposite; it is when he thinks that he is not doing historical research that he operates like a good historian, who would be limited by the same lack of documents.

What are the differences then between historical method and ethnographic method, if we use these terms in the strict sense defined at the beginning of this essay? Both history and ethnography are concerned with societies *other* than the one in which we live. Whether this *otherness* is due to remoteness in time (however slight), or to remoteness in space, or even to cultural heterogeneity, is of secondary importance compared to the basic similarity of perspective. What constitutes the goal of the two disciplines? Is it the exact reconstruction of what has happened, or is happening, in the society under study? To assert this would be to forget that in both cases we are dealing with systems of representations which differ for each member of the group and which, on the whole, differ from the representations of the investigator. The best ethnographic study will never make the reader a native. The French Revolution of 1789 lived through by an aristocrat is not the same phenomenon as the Revolution of 1789 lived through by a *sans-*



culotte, and neither would correspond to the Revolution of 1789 as conceived by Michelet or Taine. All that the historian or ethnographer can do, and all that we can expect of either of them, is to enlarge a specific experience to the dimensions of a more general one, which thereby becomes accessible *as experience* to men of another country or another epoch. And in order to succeed, both historian and ethnographer must have the same qualities: skill, precision, a sympathetic approach, and objectivity.

How do they proceed? Here the difficulty begins. For history and ethnography have often been contrasted on the grounds that the former rests on the critical study of documents by numerous observers, which can therefore be compared and cross-checked, whereas the latter is reduced, by definition, to the observations of a single individual.

To this criticism we reply that the best way to overcome this obstacle in ethnography is to increase the number of ethnographers. Certainly we shall not reach this goal by discouraging prospective ethnographers with tendentious objections. Furthermore, this criticism has been rendered obsolete by the very development of ethnography. Today there are indeed few peoples who have not been studied by numerous investigators and observed from different points of view over a period of several generations, sometimes even several centuries. Moreover, what does the historian do when he studies documents if not to surround himself with the testimony of amateur ethnographers, who were often as far removed from the culture they described as is the modern investigator from the Polynesians or Pygmies? Would the historian of ancient Europe have made less progress if Herodotus, Diodorus, Plutarch, Saxo Grammaticus, and Nestorius had been professional ethnographers, familiar with the difficulties of field-work and trained in objective observation? Far from distrusting ethnographers, the historian concerned about the future of his discipline should heartily welcome them.

But the methodological parallels which are sought between ethnography and history, in order to contrast them, are deceptive. The ethnographer is someone who collects data and (if he is a good ethnographer) presents them in conformity with requirements that are the same as those of the historian. The historian's role is to utilize these studies when the observations extend over a sufficient



period of time. The ethnologist also draws upon the ethnographer's observations when they include a sufficient number of different regions. At any rate, the ethnographer furnishes documents which the historian can use. And if documents already exist and the ethnographer chooses to integrate their contents into his study, should not the historian—provided, naturally, that the ethnographer has a sound historical method—envy him the privilege of writing the history of a society which he has experienced as a living reality?

The issue can thus be reduced to the relationship between history and ethnology in the strict sense. We propose to show that the fundamental difference between the two disciplines is not one of subject, of goal, or of method. They share the same subject, which is social life; the same goal, which is a better understanding of man; and, in fact, the same method, in which only the proportion of research techniques varies. They differ, principally, in their choice of complementary perspectives: History organizes its data in relation to conscious expressions of social life, while anthropology proceeds by examining its unconscious foundations.

The principle that anthropology draws its originality from the unconscious nature of collective phenomena stems (though in a still obscure and ambiguous manner) from a statement made by Tylor. Having defined anthropology as the study of "Culture or Civilization," he described culture as "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."⁴³ We know that among most primitive peoples it is very difficult to obtain a moral justification or a rational explanation for any custom or institution. When he is questioned, the native merely answers that things have always been this way, that such was the command of the gods or the teaching of the ancestors. Even when interpretations are offered, they always have the character of rationalizations or secondary elaborations. There is rarely any doubt that the unconscious reasons for practicing a custom or sharing a belief are remote from the reasons given to justify them. Even in our own society, table manners, social etiquette, fashions of dress, and many of our moral, political, and religious attitudes are scrupulously observed by everyone, although their real origin and



function are not often critically examined. We act and think according to habit, and the extraordinary resistance offered to even minimal departures from custom is due more to inertia than to any conscious desire to maintain usages which have a clear function. There is no question that the development of modern thought has favored the critical examination of custom. But this phenomenon is not something extraneous to anthropological study. It is, rather, its direct result, inasmuch as its main origin lies in the tremendous ethnographic self-consciousness which the discovery of the New World aroused in Western thought. And even today, secondary elaborations tend to acquire the same unconscious quality as soon as they are formulated. With surprising rapidity—which shows that one is dealing with an intrinsic property of certain modes of thinking and action—collective thought assimilates what would seem the most daring concepts, such as the priority of mother-right, animism, or, more recently, psychoanalysis, in order to resolve automatically problems which by their nature seem forever to elude action as well as thought.

Boas must be given credit for defining the unconscious nature of cultural phenomena with admirable lucidity. By comparing cultural phenomena to language from this point of view, he anticipated both the subsequent development of linguistic theory and a future for anthropology whose rich promise we are just beginning to perceive. He showed that the structure of a language remains unknown to the speaker until the introduction of a scientific grammar. Even then the language continues to mold discourse beyond the consciousness of the individual, imposing on his thought conceptual schemes which are taken as objective categories. Boas added that "the essential difference between linguistic phenomena and other ethnological phenomena is, that the linguistic classifications never rise to consciousness, while in other ethnological phenomena, although the same unconscious origin prevails, these often rise into consciousness, and thus give rise to secondary reasoning and to reinterpretations."⁴⁴ But this difference, which is one of degree, does not lessen their basic identity or the high value of linguistic method when it is used in ethnological research. On the contrary:

The great advantage that linguistics offers in this respect is the fact that, on the whole, the categories which are formed always



remain unconscious, and that for this reason the processes which lead to their formation can be followed without misleading and disturbing factors of secondary explanations, which are so common in ethnology, so much so that they generally obscure the real history of the development of ideas entirely.⁴⁵

In the light of modern phonemics we can appreciate the immense scope of these propositions, which were formulated eight years before the publication of *Cours de linguistique générale* by Ferdinand de Saussure, which marked the advent of structural linguistics. But anthropologists have not yet applied these propositions to their field. Boas was to use them fully in laying down the foundations of American linguistics and they were to enable him to refute theories theretofore undisputed.⁴⁶ Yet with respect to anthropology he displayed a timidity that still restrains his followers.

Actually Boasian ethnographic analysis, which is incomparably more honest, solid, and methodical than that of Malinowski, remains, like Malinowski's, on the level of individual conscious thought. Boas of course refused to consider the secondary rationalizations and reinterpretations, which retained so much hold over Malinowski that he managed to discard those offered by the natives only by substituting his own. But Boas continued to utilize the categories of individual thought. His scientific scruples only deprived it of its human overtones. He restricted the scope of the categories that he compared, but he did not re-create them on a new level. When the work of fragmentation seemed to him impossible, he refrained from making comparisons. And yet linguistic comparison must be supported by something more than a mere fragmentation—namely, a real analysis. From words the linguist extracts the phonetic reality of the phoneme; and from the phoneme he extracts the logical reality of distinctive features.⁴⁷ And when he has found in several languages the same phonemes or the use of the same pairs of oppositions, he does not compare individually distinct entities. It is the same phoneme, the same element, which will show at this new level the basic identity of empirically different entities. We are not dealing with two similar phenomena, but with one and the same. The transition from con-

scious to unconscious is associated with progression from the specific toward the general.

In anthropology as in linguistics, therefore, it is not comparison that supports generalization, but the other way around. If, as we believe to be the case, the unconscious activity of the mind consists in imposing forms upon content, and if these forms are fundamentally the same for all minds—ancient and modern, primitive and civilized⁴⁸ (as the study of the symbolic function, expressed in language, so strikingly indicates)—it is necessary and sufficient to grasp the unconscious structure underlying each institution and each custom, in order to obtain a principle of interpretation valid for other institutions and other customs, provided of course that the analysis is carried far enough.

How are we to apprehend this unconscious structure? Here anthropological method and historical method converge. It is unnecessary to refer here to the problem of diachronic structures, for which historical knowledge is naturally indispensable. Certain developments of social life no doubt require a diachronic structure. But the example of phonemics teaches anthropologists that this study is more complex and presents other problems than the study of synchronic structures,⁴⁹ which they are only beginning to consider. Even the analysis of synchronic structures, however, requires constant recourse to history. By showing institutions in the process of transformation, history alone makes it possible to abstract the structure which underlies the many manifestations and remains permanent throughout a succession of events. Let us return to the problem of dual organization. If we do not wish to conceive it either as a universal stage in social development or as a system devised in a single place and at one particular time, and if, on the other hand, we are too well aware of what all dual institutions have in common to consider them as totally unrelated products of unique and dissimilar historical development, we must analyze each dual society in order to discover, behind the chaos of rules and customs, a single structural scheme existing and operating in different spatial and temporal contexts. This scheme will correspond neither to a particular model of the institution nor to the arbitrary grouping of characteristics common to several variants

of the institution. It may be reduced to certain relationships of correlation and opposition, which undoubtedly operate unconsciously among peoples who possess dual organization, but which, because they are unconscious, should also be found among peoples who have never known this institution.

Thus the Mekeo, Motu, and Koita of New Guinea, whose social evolution Seligman was able to reconstruct over a considerable period of time, have a highly complex organization constantly troubled by various factors. Warfare, migrations, religious schisms, demographic pressures, and quarrels over prestige bring about the destruction of whole clans and villages or the emergence of new groups. And yet these units, whose identity, number, and distribution are constantly varying, remain linked by relationships whose content is equally variable but whose formal character is maintained through the vicissitudes in their history. Whether economic, jural, matrimonial, religious, or ceremonial, the *ufapie* relationship links, two by two, at the level of the clan, subclan, or village, social units bound by reciprocal gift exchange. In some villages of Assam chronicled by Von Furer-Haimendorf, marriage exchanges are frequently threatened by quarrels between boys and girls of the same village or by antagonism between neighboring villages. These dissensions are expressed by the withdrawal, or sometimes the extermination, of one or another group; but the cycle is restored in each case, either through a reorganization of the exchange structure or through the admission of new partners. Finally, the Mono and Yokut of California, some of whose villages possess dual organization while others do not, enable us to study how an identical social structure can be expressed with or without a defined and concrete institutional form. In every case, something is preserved which may be gradually isolated through observation—by means of a kind of straining process which allows the “lexicographical” content of institutions and customs to filter through—in order to retain only the structural elements. In the case of dual organization, these elements appear to be three in number: the need for a rule; the concept of reciprocity, providing immediate resolution of the opposition between the self and the other; and the synthetic nature of the gift. These factors are found in all the societies considered; at the same time, they explain the

less differentiated usages and customs which fulfill the same functions among peoples who do not have dual organization.⁵⁰

Thus, anthropology cannot remain indifferent to historical processes and to the most highly conscious expressions of social phenomena. But if the anthropologist brings to them the same scrupulous attention as the historian, it is in order to eliminate, by a kind of backward course, all that they owe to the historical process and to conscious thought. His goal is to grasp, beyond the conscious and always shifting images which men hold, the complete range of unconscious possibilities. These are not unlimited, and the relationships of compatibility or incompatibility which each maintains with all the others provide a logical framework for historical developments, which, while perhaps unpredictable, are never arbitrary. In this sense, the famous statement by Marx, "Men make their own history, but they do not know that they are making it," justifies, first, history and, second, anthropology. At the same time, it shows that the two approaches are inseparable.

Although the anthropologist applies his analysis primarily to the unconscious elements of social life, it would be absurd to suppose that the historian remains unaware of them. The historian no doubt intends, first of all, to explain social phenomena in terms of the events in which they are embodied and the way in which individuals have thought about and lived them. But in his progress in grasping and explaining that which appears to men as the consequence of their representations and actions (or of the representations and actions of some of them), the historian knows quite well, and to an increasing degree, that he must call to his aid the whole apparatus of unconscious elaborations. We are no longer satisfied with political history which chronologically strings dynasties and wars on the thread of secondary rationalizations and reinterpretations. Economic history is, by and large, the history of unconscious processes. Thus any good history book (and we shall cite a great one) is saturated with anthropology. In his *Le Problème de l'incroyance au XVI^e siècle*, Lucien Febvre constantly refers to psychological attitudes and logical structures which can be grasped only indirectly because they have always eluded the consciousness of those who spoke and wrote—for example, the lack



of terminology and standards of measurement, vague representation of time, traits common to several different techniques, and so forth.⁵¹ All these pertain to anthropology as well as to history, for they transcend documents and informants' accounts, none of which deals with this level, and rightly so.

It would be inaccurate, therefore, to say that on the road toward the understanding of man, which goes from the study of conscious content to that of unconscious forms, the historian and the anthropologist travel in opposite directions. On the contrary, they both go the same way. The fact that their journey together appears to each of them in a different light—to the historian, transition from the explicit to the implicit; to the anthropologist, transition from the particular to the universal—does not in the least alter the identical character of their fundamental approach. They have undertaken the same journey on the same road in the same direction; only their orientation is different. The anthropologist goes forward, seeking to attain, through the conscious, of which he is always aware, more and more of the unconscious; whereas the historian advances, so to speak, backward, keeping his eyes fixed on concrete and specific activities from which he withdraws only to consider them from a more complete and richer perspective. A true two-faced Janus, it is the solidarity of the two disciplines that makes it possible to keep the whole road in sight.

One final remark will clarify our thinking. Traditionally we distinguish history from anthropology by the presence or absence of written documents in the societies studied. This distinction is not incorrect; but we do not think it essential, since it stems from, rather than explains, those fundamental characteristics which we have attempted to define. Beyond question, the absence of written documents in most so-called primitive societies forced the anthropologist to develop methods and techniques appropriate to the study of activities which remain, for that reason, imperfectly conscious on all the levels where they are expressed. But, apart from the fact that this limitation is often overcome by oral tradition (so rich among certain African and Oceanian peoples), we should not regard it as a rigid barrier. Anthropology is equally concerned with populations which possess writing, such as those of ancient Mexico, the Arab world, and the Far East. And it is also possible to reconstruct the history of peoples who have never known writing,



as, for example, the Zulu. Here again, the question is one of a difference in orientation, not in subject matter, and of two ways of organizing data which are more alike than they first appear. The anthropologist is above all interested in unwritten data, not so much because the peoples he studies are incapable of writing, but because that with which he is principally concerned differs from everything men ordinarily think of recording on stone or on paper.

Until now, a division of labor, justified by ancient tradition and the needs of the moment, has contributed to the confusion of the theoretical and practical aspects of the distinction, and thus to an undue separation of anthropology from history. If anthropology and history once begin to collaborate in the study of contemporary societies, it will become apparent that here, as elsewhere, the one science can achieve nothing without the help of the other.

NOTES

1. H. Hauser, *L'Enseignement des sciences sociales* (Paris: 1903); F. Simiand, "Méthode historique et science sociale," *Revue de Synthèse* (1903).
2. *Translator's note:* In the Durkheimian sense of beliefs, sentiments, and norms common to the members of a society.]
3. This became true at the end of the nineteenth century. But it should not to be forgotten that, historically, sociological evolutionism preceded biological evolutionism.
4. Leslie A. White, "Energy and the Evolution of Culture," *American Anthropologist*, n.s., XLV (1943); "History, Evolutionism, and Functionalism: Three Types of Interpretation of Culture," *Southwestern Journal of Anthropology*, I (1945); "Evolutionary Stages, Progress, and the Evaluation of Cultures," *Southwestern Journal of Anthropology*, III (1947).
5. E. B. Tylor, *Primitive Culture* (London: 1871), I, 7.
6. E. B. Tylor, *Researches into the Early History of Mankind and the Development of Civilization* (London: 1865), p. 3.
7. Émile Durkheim and Marcel Mauss, "De quelques formes primitives de classification," *L'Année sociologique*, VI (1901-2).
8. R. H. Lowie, *Societies of the Hidatsa and Mandan Indians*, Anthropological Papers of the American Museum of Natural History, XI (1913); L. Spier, *The Sun-Dance of the Plains Indians*, Anthropological Papers of the American Museum of Natural History, XVI (1921), A. L. Kroeber, "Salt, Dogs, Tobacco," *Anthropological Records*, VI (1941).
9. F. Boas, "History and Science in Anthropology: a Reply," *American Anthropologist*, n.s., XXXVIII (1936), p. 139.
10. *Ibid.*, p. 140.
11. F. Boas, "The Limitations of the Comparative Method of Anthropology" (1896) in *Race, Language, and Culture* (New York: 1940), p. 276.

12. *Ibid.*, p. 277.
13. F. Boas, "Evolution or Diffusion?" *American Anthropologist*, n.s., XXVI (1924), p. 342.
14. *Loc. cit.*
15. F. Boas, "History and Science in Anthropology . . .," *op. cit.*, p. 139.
16. F. Boas, "The Methods of Ethnology," *American Anthropologist*, n.s., XXII (1920), pp. 314-15.
17. *Ibid.*, p. 314.
18. Ruth Benedict, "Franz Boas as an Ethnologist," in *Franz Boas: 1858-1942*, Memoirs of the American Anthropological Association, n.s., No. 61 (1943), p. 27.
19. We are not criticizing here Boas' archaeological work, which belongs to archaeology and not ethnology, or his research on the diffusion of mythological themes, which constitutes historical research based on ethnographic data. Similarly, Paul Rivet, when he formulates his hypotheses on the primitive peopling of America, uses archaeological, linguistic, and ethnographic data for an investigation which is properly historical. These undertakings must, therefore, be examined from a historical viewpoint. The same may be said about some of Rivers' studies.
20. A. L. Kroeber, "History and Science in Anthropology," *American Anthropologist*, n.s., XXXVII (1935), pp. 539-69.
21. F. Boas, "History and Science in Anthropology . . ., *op. cit.*
22. R. H. Lowie, "American Culture History," *American Anthropologist*, n.s., XLII (1940).
23. B. Malinowski, "The Present State of Studies in Culture Contact," *Africa*, XII (1939), p. 43.
24. B. Malinowski, "Culture as a Determinant of Behavior," in *Factors Determining Human Behavior*, Harvard Tercentenary Publications (Cambridge: 1937), p. 155. On the following page he also speaks of "these queer and sordid customs," which have, however, "a core of rational and practical principle." This constitutes a return to the eighteenth century, but to its worst aspect.
25. F. Boas, "Some Problems of Methodology in the Social Sciences," in Leonard D. White (ed.), *The New Social Science* (Chicago: 1930), pp. 84-98.
26. B. Malinowski, "Culture," in *Encyclopaedia of the Social Sciences* (New York: 1935), IV, 625.
27. *Ibid.*, p. 627.
28. *Loc. cit.* For Malinowski, furthermore, it seems that no distinction is necessary when a transition is made from the general to the particular: "Thus culture, as we find it among the Masai, is an apparatus for the satisfaction of the elementary needs of the human organism." And, referring to the Eskimo, ". . . towards sex they have the same attitude as the Masai. They have also a somewhat similar type of political system. . . ." ("Culture as a Determinant of Behavior," *op. cit.*, pp. 136 and 140.)
29. B. Malinowski, "Introduction" to H. I. Hogbin, *Law and Order in Polynesia* (London: 1934), pp. 48-9.
30. B. Malinowski, "Culture," *op. cit.*, p. 630.
31. B. Malinowski, *The Sexual Life of Savages in Northwestern Melanesia* (London-New York: 1929), I, 29.



32. B. Malinowski, "Culture," *op. cit.*, p. 634 ff.
33. B. Malinowski, *The Sexual Life . . . , op. cit.*, p. 40.
34. *Ibid.*, pp. 43-5.
35. F. Boas, *The Social Organization and the Secret Societies of the Kwakiutl Indians* (Washington: 1895); M. Griaule, *Masques Dogons* (Paris: 1938), and "Mythe de l'organisation du monde chez les Dogons," *Psyché*, II (1947).
36. B. Malinowski, *The Sexual Life . . . , op. cit.*, p. 81.
37. B. Malinowski, *Sex and Repression in Savage Society* (London-New York: 1927), p. 204.
38. Margaret Mead, *Sex and Temperament in Three Primitive Societies* (New York: 1935), p. 279.
39. R. F. Fortune, "Arapesh Warfare," *American Anthropologist*, n.s., XLI (1939), pp. 22-41.
40. Margaret Mead (ed.), *Competition and Cooperation among Primitive Peoples* (London-New York: 1937), p. 461.
41. F. Boas, "History and Science . . . , *op. cit.*
42. H. Hauser, *op. cit.*, p. 414.
43. E. B. Tylor, *Primitive Culture*, I, 1.
44. F. Boas (ed.), *Handbook of American Indian Languages*, Bureau of American Ethnology, Bulletin 40, 1911 (1908), Part I, p. 67.
45. *Ibid.*, pp. 70-1.
46. At a time when students of Indo-European linguistics still firmly believed in the theory of the "proto-language," Boats demonstrated that certain traits common to several American Indian languages may be the result of the secondary formation of areas of affinity just as well as of a common origin. The application of the same hypothesis to Indo-European data had to await Troubetzkoy.
47. R. Jakobson, "Observations sur le classement phonologique des consonnes," in *Proceedings of the Third International Congress of Phonetic Sciences* (Ghent: 1938).
48. See "The Effectiveness of Symbols," Chapter X of this volume.
49. R. Jakobson, "Principien der historischen Phonologie," in *Travaux du Cercle Linguistique de Prague*, IV.
50. C. Lévi-Strauss, *Les Structures élémentaires de la parenté* (Paris: 1949), Chapters VI and VII.
51. Lucien Febvre, *Le Problème de l'incroyance au XVI^e siècle*, Second Edition (Paris: 1946). (First edition: 1942.)

