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ETHNOGRAPHY, CULTURAL AND SOCIAL ANTHROPOLOGY

By ROBERT H. LOWIE

THE discussion by Professors Murdock and Firth, Professor Fortes's contribution to the debate, Professor Radcliffe-Brown's illuminating letter in a recent issue of this journal, and a number of other statements by American and British colleagues (Murdock 1951; Firth 1951; Radcliffe-Brown 1952; Fortes 1953; Evans-Pritchard 1951) stimulate reflections on cultural and social anthropology. In the present, wholly uncontroversial article I shall first define the aims of cultural anthropology as I understand them and shall then inquire into the relations of that discipline with social anthropology as defined by British scholars.

I

Whatever differences may divide cultural from social anthropologists, they are hardly greater than those which divide self-styled cultural anthropologists. Indeed, I should say that many of us feel incomparably closer to the English anthropologists referred to above than, say, to Goldenweiser in his later phases.

A concrete example will illustrate the issue. In one of his books (Goldenweiser 1922) this writer devotes a chapter to the Baganda, relying as he was bound to do on Roscoe's well-known work. He tells us that "maize is perhaps the principal staple food, but plantain trees are also cultivated on a large scale." Now the primary source (Roscoe 1911: 5, 432) states in unmistakable terms that *plantains* "furnish their staple food," whereas maize "was never grown in any quantity . . . ; no one called the two or three cobs which he ate a meal." Of course, anyone is liable to factual inaccuracies, but what is involved here is something deeper. Goldenweiser writes about these matters as might any layman—not as a student of culture. He does not explain to his reader that maize is a species introduced into Uganda in recent times. He shows no inkling of the problems connected with the occurrence and distribution of East African plantains, indigenous and otherwise. Yet in his several publications there is no end of references to culture—to "psychology and culture," to "culture and environment," to "the theoretical categories and cultural reality," and so forth.

It would be convenient to have an English term designating the kind of cultural anthropology of which some of us—say, Kroeber and myself—are votaries, a term corresponding to Klemm's "*allgemeine Culturwissenschaft*." For the purpose intended, "cultural history" will not do since it one-sidedly circumscribes the discipline; and notwithstanding the analogy of "sociology," many of us recoil from deliberately perpetrating a twin monstrosity such as "culturology." However, Professor Radcliffe-Brown informs us that since 1909 British colleagues have used "ethnography" to denote "descriptive accounts of

non-literate peoples." The accounts, I assume, relate to culture. I accept the definition with an extension of meaning. Greek "ethnos" assuredly did not apply solely to savages or barbarians, and non-literacy is obviously not a sharply defined concept. Ghegs are not literate in the sense that Scandinavians are; and the Maya present a borderline case. Further, it is not clear that in principle a report on the social life of ancient Egypt, modern China, or seventeenth-century England (witness the famous third chapter of Macaulay's *History*) differs from a report on the Kariera, the Yurok, or the Maori. Accordingly, I suggest that the term "ethnography" cover all cultures, past and present—thus becoming an equivalent for a "general science of culture." In accordance with my conceptions of science, which are those of Ernst Mach and G. R. Kirchhoff, I define the ideal aim of ethnography as the *complete* description of all cultural phenomena everywhere and at all periods.

Obviously one individual cannot thoroughly know more than a limited number of cultures; and with growing specialization he cannot even completely master all the aspects of a single culture. Nowadays the same person is not likely to describe social structure with the thoroughness of a Firth or Fortes and at the same time penetrate the intricacies of textile work with the devotion of an O'Neale or a Bühler. This has long been recognized—witness the division of labor among participants of the Cambridge Expedition to Torres Straits, Haddon taking over technology, Rivers social organization, and so forth. Yet, as a student I heard Haddon expound Rivers's genealogical technique for studying kinship terminology. There lies the crucial point: the true ethnographer may not be competent to advance knowledge along all lines within his science, but he takes an interest in what is done by fellow-ethnographers who happen to be drawn to other departments of culture. Characteristically he despises no item of culture as intrinsically negligible. However trivial it may appear at first blush, its correlates may be of the highest significance. The avoidance of parents-in-law doubtless struck many as merely "a quaint and somewhat comic custom" of primitive etiquette, but for Tylor it inaugurated a most stimulating line of research.

What distinguishes my ethnographer from the cultural anthropologist à la Goldenweiser is that the ethnographer uses terms not as counters in a metaphysical game, but to circumscribe an empirical content which he hopes to enlarge. If he deals with "plantains," he determines their place in tribal economy, the method of cultivation, the genetic relationship of the form planted with indigenous or alien wild forms of *Musa*, and so forth. Without such information he does not feel that he understands this particular facet of Baganda culture; with all the information available about the global distribution of *Musa*, the historical connections of its varieties, the uses to which they are put, etc., he understands what *can* be understood at the present time.

As the illustration indicates, the ethnographer cannot always obtain the

information he craves unaided by other disciplines; but such recourse to outside aid does not involve surrender of his autonomy. It is he who frames the problems, and there is never a question of merging the distinctively cultural phenomena in botanical or other terms.

It may be well to exemplify by a series of specific cases.

In Heizer's recent paper on piscicides (1952) there is, first of all, a definition of how the phenomenon under discussion is globally distributed. The author eliminates sham problems by supplementing his findings with a statement concerning the distribution of narcotic species. Obviously where such are lacking they cannot be utilized. Heizer further inquires whether natives always avail themselves of the opportunity to drug fish where nature does provide it, to what extent they deliberately cultivate narcotic plants for piscicidal objectives. He realizes that the data collectively impinge on no less a problem than the logical or prelogical mentality of man. Certainly there still remains a great deal to be learned about piscicides; but Heizer has appreciably added to our comprehension of the problem and worked towards that complete determination of relevant facts which constitutes the aim of a student interested in the realities of culture, not in an abstraction divorced from these realities.

Corresponding reflections hold for animal domestication. To take the much-mooted problem of the reindeer, any new zoological or geographical data may help towards a better view of the cultural problems. It is important to know that the species flourishes in the taiga as well as in the tundra; important to note the physical varieties that permit or preclude mounting the animal. Highly significant, too, is the discovery that—contrary to oft-repeated statements—reindeer, if kept in small numbers, are "thoroughly domesticated and tame as are the horses kept in stables." It alters the conception of the species as inherently migratory (Jochelson 1926: 361–367).

As the complete ethnographic picture may require the help of botany or zoology or metallurgy, so it may require that of psychology. The phobia of that science on the part of some cultural anthropologists and sociologists is historically intelligible, but logically indefensible. It is a survival of the period, long past, when students of society or culture had to insist that their subject matter demanded a special branch of learning. But what is at stake nowadays is something quite different. As some cultural phenomena cannot be grasped without the geographer's, botanist's, or metallurgist's help, so others require the light of psychology. Child behavior, e.g., as traditionally determined, is unquestionably a part of a given society's culture, hence Margaret Mead (1933) is wholly right in demanding that the old ethnographer's random observation of this field be superseded by systematic observation through approved psychological techniques. Such procedure in no way involves a subordination of ethnography to another discipline of supposedly higher hierarchical status.

To renounce on principle whatever illumination psychology may offer is to

renounce complete comprehension of many cultural phenomena. If "Arctic hysteria" is characteristic of groups widely separated, its occurrence evokes an urge for a fuller grasp of what Tylor would call its "adhesions." Psychology may not yet offer anything like a definitive interpretation, but even the tentative suggestion that the phenomenon goes with "a subservient or submerged social position," that it is more commonly associated with women, is not without value (Aberle 1952). As to shamanism, the term is a meaningless counter unless we ascertain to the fullest extent possible the mental state of the shaman and the responses of his group. Surely it is not a matter of indifference to the ethnographer whether, as Jochelson and Bogoras suggest, the shaman is a neurotic, if not a madman and invert, or whether according to investigators elsewhere he may be quite normal (Jochelson 1924: 199 f.; Lindgren 1935: 222 et seq.). Correspondingly, it is part of the ethnographic phenomenon whether the shamanizing practitioner is a humbug, whether he wholly loses consciousness, or approaches that state (Seligmann 1911: 133 f.).

Of course, the "complex whole" defined by Tylor as constituting culture embraces many phenomena not dealt with by any natural science. With reference to them, the ethnographer may either utilize techniques perfected by the humanities or devise new avenues of approach. As noted, one criterion of the ethnographer is his willingness to attack the apparently trivial, recognizing that like Tennyson's "flower in the crannied wall" it potentially sheds light on the cultural universe. Tylor (1896) did not disdain to survey lot-games, thereby arriving at significant conclusions (whether right or wrong) on Asiatic and American connections and at equally significant methodological considerations.

In order to preclude misunderstanding of my position, I turn from Tylor to Culin (1907). Probably there is not for any part of the globe a comparable mass of information on aboriginal amusements. However, it is raw material with a minimum of interpretation and even of serviceable arrangement. That is to say, though the description of the several games is often conspicuously full, the relationships of the facts to one another remain unilluminated. The monograph aptly illustrates the truism that a mere accumulation of data yields no significant conclusion. What is, however, often ignored in similar contexts is that, though men like Culin fall far short of that *complete* description which involves insight into correlations, their faithful gathering of facts makes it possible for others to extract meaning that they themselves failed to gain.

To take one other department of culture, ethnographers concerned with aboriginal literature have not merely collected tales and songs, but have applied to them the methods current among literary critics in higher civilizations. Boas (1916; 1927: 299 et seq.) investigates what aspects of a tribal culture are reflected in tales, the rhythmic repetition in aboriginal narratives, the metaphorical expressions in their poetry. Miss Beckwith (1919: 296-331;

1951) besides shedding light on the correlations of Polynesian literature with belief and social organization, has enlarged on the specifically poetic attributes of Hawaiian poetry. Radin (1915; 1948; 1949) has for forty years discussed problems of literary style and the relations of the author-raconteur to the traditional body of literature. Reichard (1947) compares the styles of neighboring tribes in the northwestern United States; Shimkin (1947) points out the comparative literary richness of an originally Basin people with a meager technology and the dependence of style on individual personality. Comparable researches have been credited to Russian folklorists, but the only European study I can recall having a similar orientation is Thurnwald's discussion of Papuan songs (1936).

Let me repeat that at present no one scholar can be expected to master to an equal degree all the subdivisions of culture. Even before the present degree of specialization it was impossible for one man to add with uniform success to the several departments of the total field. Over half of Tylor's *Primitive Culture* was devoted to religion and myth; Boas probably displayed most enthusiasm for decorative art and folk-literature; Haddon is known above all for his technological studies and his discussions of art. But these men were true ethnographers on two counts: they did concern themselves with the whole or at least the major part of the total range of cultural phenomena; and where they specialized they delved deep. *They were interested in the realities of culture.*

II

From the point of view here assumed there is no difficulty in fitting contemporary British social anthropologists into the picture. I accept them as true ethnographers, as scholars who at their best have dealt admirably with an important subdivision of culture. I recognize that some of them moreover show a commendable concern with other aspects of culture as well. I admit that it is profitable to view the culture of a people with social structure as a starting-point. What I must unequivocally reject, however, is Dr. Fortes's contention that social structure "is not an aspect of culture but the entire culture of a given people handled in a special frame of theory." In my opinion the social structure of a people by Tylor's definition is one aspect of their culture. For example, matriliney is not determined by the biological heredity of a group nor by the biological idiosyncrasies of given members of a group, but is part of the social heritage of the constituent individuals. Further, pending proof to the contrary, I cannot conceive the *entire* culture of a given people being subsumed under the head of social structure. Certainly it is possible to link crafts with social groups or to connect forms of religious faith, say ancestor worship, with segments of the society, and to do so is eminently worth while. But to do so in no way eliminates the need for studying the crafts—men's technological processes or the content of the beliefs. The cultural residue that

remains after approaching "the entire culture" from Dr. Fortes's point of view strikes me as appallingly large.

III

Finally I turn to the familiar dichotomy: historians versus generalizers. Undeniably it is vital in some contexts, but I cannot admit that it serves to distinguish my "cultural anthropologist," i.e. ethnographer, from the social anthropologists. The ethnographer, attempting to fathom the infinitely varied reality of culture, must cope with many problems that in themselves neither lead to historical reconstruction nor to the formulation of laws.

In his discussion of a Mohave epic Kroeber (1951) has to broach the question whether the incidents narrated as historical by the native informant had actually occurred; but this inquiry has nothing to do with tracing the antecedents of the tale, its possible diffusion, for instance, from an outside center. Of course, the historicity of the episodes has a bearing on the past of the Mohave people. But it bears no less on a generalization explicitly formulated by Kroeber: The memory of a group in the status of the Mohave cannot be trusted beyond a century or two. In the same paper Kroeber indulges in many comparisons of his Indian tradition—with the Homeric and Hindu epics, with Biblical stories, with the Book of Mormon. His intention is not to derive the Mohave narrative from any of these nor to demonstrate a law. He is trying to define the individual phenomenon with which he is dealing. When he aligns Mohave with Homeric use of proper names (1951: 132), the comparison merely tries to bring home the nature of the aesthetic effect achieved by the Indian. What Kroeber envisages here, as in most parts of his anthropological work is, to use his own phrase, "descriptive integration"; he has called this aim "historical," but it is historical in a novel sense and one quite distinct from that given to the term by social anthropologists (1935: 545 f.).

What Kroeber calls "descriptive integration" evidently comes close to my "complete description." Such complete description implies cognizance of correlated phenomena and thereby precludes erroneous inferences due to the currency of the same labels. The Bronze Age of Peru is not the equivalent of the Bronze Age in the Old World. As Nordenskiöld remarks, it "does not mark the adoption of a new civilization but only an amplification of the copper culture in Peru" (1931: 40).

Complete description involves a *global* survey of correlations because only such a global survey guarantees accurate definition of the cultural phenomenon under discussion in relation to its real or apparent equivalents elsewhere. When, however, these correlations are lifted from their respective contexts and found to coincide, when diffusion is eliminated or rendered improbable in the cases investigated, the ethnographer achieves precisely the kind of

generalizations—functional relationships between descriptively separable elements—that a sound social anthropology aspires to (Eggan 1950: 323). The ethnographer would fall short of his own objectives if he failed to arrive at such generalizations or renounced them on principle.

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