AN ANTHROPOLOGICAL ODYSSEY

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Dedicated to Isaac Schapera with respect and affection

My anthropological experience may be described as a long and inspiring journey across the seas of the human condition. I have always been struck by what I call the paradox of human reality: Born to live, we are destined to die; ready to enjoy life, we must struggle to avoid discomfort and disease; conscious of individuality, we are still unavoidably bound from birth to a social group; claiming to be equal, we nevertheless differ both as individuals and groups.

Whenever it seemed I had a comprehensive view of human nature, I was led to a reorienting perspective. I regret none of the various phases of my long journey. Each has contributed to the evolution of my anthropological approach to reality. The reverse is also true: My anthropological perspective has often brought changes in my life's route. Indeed, the study of anthropology, which revealed the essence of the human condition, decisively led me to change my social and professional stations. I left the Catholic priesthood and its missionary activity, dedicating myself fully to the study of anthropology.

BASIC TRAINING

It was during the first stage of my long journey, my early missionary vocation, that my interest in anthropology was aroused. Latin literature, Greek literature, and history had been the main subjects in my secondary education. At the end of my teens I took the philosophical and theological training offered by a seminary for missionaries. Certain highlights from this experience may be of interest, especially to my fellow anthropologists concerned with the cultural background of Christian missionaries.

1

As prospective missionaries, we were not trained as ordinary priests—as mere ritual specialists versed in theological and biblical knowledge. We studied all that, but we were also instructed in how to approach peoples of various cultures. We were trained to meet others not as Christians, nor necessarily as religious men, but rather as fellow human beings. The emphasis in our training was practical, and manual labor was required of us every day. I owed much to that kind of education, especially during my field work. Even now, at an old age, I feel all its advantages.

My seminary training involved also the study of moral theology, the theology of human acts. I was thus encouraged early to ponder personal autonomy and responsibility, which prepared me to evaluate later, as an anthropologist, the impact of cultural context upon individual actions. I am convinced that study of the nature of the human act (a term implying a degree of consciousness and freedom that confers responsibility) should be a required element of anthropological training. It helped me to deepen both my inspection of human behavior and my understanding of the social institutions within which that behavior is conceived and realized.

For almost four years (1944–1947) I was attached to a special office of the Secretariat of State of the Vatican designed to conduct the Vatican's relations with the Allied Military Authority in Rome. During this extraordinary period I had the opportunity to meet people from all walks of life. As a reaction to a world of formalities and officialdom, of ambitions and jealousies, I felt strengthened in my determination to respect all persons equally, whatever their qualifications.

The Secretariat of State was then directed by the high officers of its two main sections (to the silent resentment and embarrassment of the ambassadors, who were forced to deal with officials not of their own rank). These officers were Domenico Tardini and Giovanni Battista Montini. Both bore a family name ending in the diminutive "*ini,*" so they were ironically described as "the two diminutives of the Secretariat of State." Both, on the contrary, were giants: Tardini became Cardinal Secretary of State under Pope John XXIII, and Montini succeeded John as Pope Paul VI.

HISTORY AND ANTHROPOLOGY: THE PROBLEM OF ORIGINS

The theoretical issue between history and anthropology engaged my intellectual interest from the very beginning of my anthropological training in the early 1940s. My classical education had been imbued with history. During my undergraduate years, the historicism of Benedetto Croce represented the liberal reaction against the rude authoritarianism of the collapsing fascist regime. We were convinced that there is no escape from history whenever you

deal with the human condition, whatever your disciplinary orientation. As a consequence, I found congenial the historical approach to "primitive peoples." I also relished the debate between the early evolutionist school and the diffusionist school over the history of culture and society. I took the side of the diffusionistic school, attracted by the rigor of its methods. Both schools, in my view, sought the *historic* origin of human culture and society. During the 100 years when the two schools flourished—from 1859, the year of the first edition of Charles Darwin's *The Origin of Species,* to 1955, when the 12th volume of Wilhelm Schmidt's *Der Ursprung der Gottesidee* was posthumously published—an overwhelming literature arose on the theme of human origins. The debate's outcome, however, was disappointing; the origins of human culture remain uncertain.

When I entered the university, few students there showed interest in ethnology. (In Italy, ethnology is the equivalent of social and cultural anthropology, as distinct from physical anthropology.) The few who were interested, however, were extremely taken by methodological disputes. Personally, I had formed a high opinion of F. Gräbner and his attempt to define criteria of ethnological research as severe as those required by classical history. That rigor strengthened my sympathy for the German school of ethnology, including its Viennese version. I read, and even translated from German into Italian for my personal use, Wilhelm Schmidt's handbook of ethnological method. The construction of cultural cycles seemed to me a much more reliable way to trace the historical development of human culture and society than all the evolutionistic schemes. I considered such cycles to be broad generalizations about the styles of human culture, much the way the styles of art serve to periodize art history.

SCHMIDT AND THE ITALIAN CATHOLIC CONTEXT

The influence of Wilhelm Schmidt on the Italian Catholic intelligentsia of the early 1930s needs a special note.

Following the unification of Italy and the fall of the papal state (1870), Catholics were forbidden by the Church from participating in the political life of the new state. Their presence in the academic institutions of the state was also minimal. In addition, the positivism, idealism, and historicism dominant during the first decades of this century were not only anti-Catholic but also hostile toward such empirical disciplines as sociology, anthropology, and psychology. Against that trend, there came, in the early 1920s, a revival of Thomistic philosophy—"neo-scholasticism," with its Italian center at the recently established Catholic University of Milan. It was there that Schmidt found his main introduction to academic circles in Italy.

During the same period, Schmidt had been called to Rome in connection

with the missionary exhibition of the holy year, 1925. Following its success, he was entrusted by Pope Pius XI with the establishment of an ethnographic museum, *Pontificio Museo Missionario Etnologico,* at the Lateran Palace in Rome, using material from the exhibition. Schmidt impressed his theories upon the museum, especially in an exhibit showing the succession of cultural cycles from the primeval through the secondary and tertiary cycles. A specialized library was added to the museum, which soon became one of the two best libraries of ethnology in Rome, the other being the library of the Pigorini Museum.

At these centers I did my reading, and in 1946 I wrote my first doctoral thesis in anthropology at the University of Rome "La Sapienza." Among the activities of the new museum, Schmidt inaugurated the production of an annual review, *Annali del Pontificio Museo Lateranense,* to provide space for missionary ethnographic reports as well as for essays by professional anthropologists. On the same lines he had founded his first journal, *Anthropos,* in 1904.

Schmidt, who had never been in the field, was succeeded by one of his best students, Michael Schulien, a German native of the Saar with some field experience in Mozambique. Schmidt returned to Austria, St. Gabriel, Mödling, wherefrom, on the eve of the *Anschluss,* he escaped to Switzerland in order to save himself and the *Anthropos* library from Nazi requisition. Both Schmidt and Schulien were fierce antagonists of Nazi racism, and they continued to be regarded by some Italian antifascists as a source of inspiration. Schulien, in fact, wrote a series of articles in *L'Osservatore Romano,* the Vatican newspaper, on the unity of mankind (29). These were widely read as a response to the fascist propaganda of the day. During that period the Vatican paper reached its largest circulation, for it was the only antifascist, or nonfascist, paper available to the Italians of those days. Anthropology had thus been brought into the heart of the political scene, and we young anthropologists felt more than a moral responsibility to reject the appalling false claim of a scientific basis for fascist racial laws. The situation reached its climax during the Nazi occupation of Rome. When they were assaulted by the Nazis, some Roman Jews—and most Italian politicians—found refuge in the numberless religious facilities in Rome, including the Lateran Palace where the ethnographic museum was lodged.

RAFFAELE PETTAZZONI

Schmidt's personal success and his school of thought were not so unanimously accepted outside Italian Catholic spheres. The strongest ideological opposition came from Raffaele Pettazzoni, the renowned historian of

religions. Pettazzoni was the incumbent of the first chair of the history of religions at the University of Rome "La Sapienza." As a student, I greatly profited from his courses on the religion of the ancient Germans, and I recall some occasions when, because we were both from Bologna, I enjoyed special signs of his friendly kindness. He envisaged ethnology as an auxiliary discipline of the history of religions. Indeed, we owe to him the introduction of an academic course in ethnology at Rome University and the establishment of an ethnological institute initially named the *Istituto delle Civiltá Primitive* (Institute of Primitive Civilisations). Its name soon changed to *Istituto di Etnologia* (Institute of Ethnology) by Vinigi L. Grottanelli, this entity has recently been absorbed by the newly established *Dipartimento di Studi Glottoantropologici* (Department of Anthropology and Linguistics).

In my day there were two official courses of ethnology in Rome, one at Rome University "La Sapienza" taught by Alberto Carlo Blanc, a paleoethnologist; the other at the Urbaniana University of the Vatican by Renato Boccassino, who had attended Malinowski's famous seminars at the London School of Economics and had completed intensive fieldwork among the Acholi of Uganda. I followed both, with no great profit. My real teacher was Schulien. He did not hold a chair, refusing to stand publicly as a teacher while the Nazis were ravaging his own country and the world. We developed the habit of frequent encounters at the Lateran Museum, during which, patiently answering the queries of a curious young apprentice, he sought to elucidate the aims and problems of the *Kulturkreise* method.

The polemics between Schmidt and Pettazzoni had been constant and even bitter, from their early reviews of each other's works in the second decade of the century. Each in his own way was a scholar of exceptional value and consequence. When I first met Pettazzoni, the two had been battling for three decades. Our student loyalties were divided. Schmidt was acute in spotting the ethnological weaknesses of his opponent. He criticized particularly Pettazzoni's attribution of the idea of God among "primitives" to a mythical kind of thought *(mythische Denken),* rather than to causal thought *(kausalen Denken).* Pettazzoni was strengthened by his insistence upon a method of historical research and reconstruction that he claimed was not suggested by the idea of Schmidt's *Urmonotheismus.* He lived long enough (he died in 1959) to see the end of Schmidt's ethnological *Ur*-construction and *Kulturkreiselehre* (28; on Pettazzoni, see 16).

Strong criticism of Schmidt's historical approach came from another young Italian scholar, Ernesto De Martino, a brilliant student of Benedetto Croce. His work, though practically unknown outside of Italy, has had a tremendous impact on young Italian anthropologists. His first criticism of the cultural cycles of the German school placed him in the forefront (13), and his later research on tarantism, ritual mourning, and magic in Southern Italy gained

him enduring consideration as the true innovator of Italian cultural anthropology (13, 26, 27).

When Malinowski's functionalism was first made known in Italy as a new method of research it excited considerable interest but no real acceptance. Pettazzoni recognized its ideological foundation and methodological utility, though—as he told me—he would stay a "historian." Indeed we students followed suit. Having a classical background, we held history to be the only correct approach to the analysis of human culture. As for the new functionalist propositions, we remained skeptical, regarding them as good only for the current phenomena of colonial encounter.

AFRICAN STUDIES AT CAPE TOWN

When, in the first days of February 1948, I landed in Cape Town, after a sea voyage of more than 20 days from Venice to Durban and three days on the railway from Durban to Cape Town, the chair of social anthropology at the University of Cape Town was held by Isaac Schapera. My missionary sponsors had expected that I would eventually teach in one of the secondary schools of Kenya. I was to have received my professional training at either Oxford or Cambridge, where I would earn a PhD in anthropology. However, because in 1948 the Oxbridge institutions were filled with ex-servicemen, I was diverted to Cape Town. Michael Schulien provided me with a letter of introduction to Isaac Schapera.

The climate and the surroundings of Cape Town and Cape Peninsula were congenial. Indeed, from that point of view, I thoroughly enjoyed my three years in South Africa. My arrival coincided with the high tide of the political campaign that led to the fall of Jan Smuts and the first victory of the Afrikaaner nationalists under Daniel Malan. I could feel the mounting tension as the existing practice of color-bar was intensified by the new policy of apartheid, at that moment a hazy concept filled with gloomy implications. The University of Cape Town was liberal, and there I met and befriended African students. (One of them, the Hon. Peter Okondo, is now a Minister of State in the Government of Kenya.) While discontent simmered and the causes for grievance increased day by day, we students were able to do little. We endured the situation created by the selfishness of most European South Africans.

Those three years in South Africa were really a determinant stage of my odyssey. For the first time I came into contact with what I think of as the true Africa. I had the good fortune to meet Isaac Schapera—a renowned Africanist and a great teacher. Never verbose, he was rather matter-of-fact. I always found his suggestions poignant and stimulating. Even today, whenever I reach London either alone or with my wife, I visit him as an old friend.

But Schapera, and all that was behind him ideologically and methodologically, meant a deep change in my approach to anthropology. Though an avowed follower and admirer of his two great teachers, Radcliffe-Brown and Malinowski, he did not unduly emphasize their rejection of history. It was as if he did not entirely share their opinion on that issue. In fact, in his comprehensive research on the Tswana, he succeeded in synthesizing the functionalist and historical perspectives, and in this sense his oeuvre has been exceptionally consistent—really, paradigmatic (on Schapera, see 14).

I was also fortunate to establish a lasting friendship with Meyer Fortes. When in 1949 he came for a semester to Cape Town to replace Schapera, who was visiting in the United States, I had already become familiar with all the tenets of functionalism. My former prejudices had been removed as I saw the possibility of a synthesis between the functionalist perspective and the historical approach, on the lines that Schapera had been pursuing without any formal or public ideological declaration. However, I recollect having occasional, deep discussions with Meyer on the value of history in anthropology.

Passage from my early stance as an anthropologist of cultural-historical extraction to a structural-functionalist of new conviction came with my first fieldwork, the result of shrewd guidance by Schapera. During that first semester in Cape Town, I had been studying Zezuru, a dialect of the Shona language, under the tutorship of a fellow PhD candidate, George Fortune, a distinguished Bantuist. Since I had read the scarce literature available on the Zezuru, Schapera suggested that I spend my term vacation among this people to obtain direct, even if brief, experience in the field. He helped me discard some ill-conceived proposals. Finally, I spent four weeks visiting, family by family, the five villages around the Catholic Mission of Musami, where I was lodging. My report was published in the *Communications of the School of African Studies* (1). My ethnographic contribution to the history of the Zezuru was fortified by three replications over the next 30 years (1, 11, 18 regrettably, I have not seen the results of Chavunduka's last survey, that of 1980).

After that first field work, my approach to anthropology became more practical and direct. I developed a capacity, if I may say so, to observe a social structure at work within a living community, be it the kinship system, the age system or any other, tempering my theoretical approach with the experience of sharing life with "others" and matching my interpretation of reality with theirs and with their behavior.

At the end of 1950 my dissertation on the age system of the Nilo-Hamitic peoples was finally approved, and I was awarded the PhD in African Studies by the University of Cape Town. My work had been intended as a preliminary evaluation of the existing literature, a task made difficult by the tremendously confusing ethnographic reports. Each author had used his own jargon or attributed different meanings to an apparently common language. I attempted

to build a semantic concordance: *concordia discordantium*. The basic pattern of the age system seemed to emerge and make sense. I was convinced of its political significance in the structural context of the Nilo-Hamitic societies (3), a line of thought I was to pursue in my future, more general work on the same subject (7). I am happy to have witnessed an efflorescence of further important research on the same theme, the latest being that of Müller (23) and Tornay (34).

FIELD WORK IN KENYA

At the end of 1950, I went directly to Kenya from South Africa, having been summoned, as a teacher, to Kevote Secondary School among the Embu, where I stayed only for one term. During even so short a stay one could sense the general discontent mounting in the Central Province of Kenya, a simmering prelude to the Mau Mau rebellion.

At the end of the term I left for London, where I was to spend a full year at the Institute of Education, London University, for a Postgraduate Certificate in Education. I returned to Kenya at the end of 1953 when the State of Emergency had just been declared.

For the next six years I lived in Kenya. The East African experience served as a further stage of my odyssey. I lived through the Mau Mau resistance, sharing the heavy suffering of the Kikuyu and the Embu, whom I regularly visited, and of the Meru. To the latter I became particularly associated by my work, first as a teacher and afterwards, in the field, as an anthropologist. This connection I never discontinued, even when, in 1956, I was assigned to pastoral work among the Italian community in Nairobi. For a short period, at the beginning of 1954, I was the principal of the Nkubu secondary school among the Meru Imenti. I was soon relieved of my educational responsibility when, in mid-1954, having been awarded a fellowship from the Ford Foundation through the International African Institute, I became entirely engaged in my field research among the Tharaka.

Like the Imenti, the Tharaka were one of the nine Meru ethnic groups; but unlike the Imenti, they had until then been left to themselves, considered the most backward of all the Meru. I was to discover how unjust were the opinions that the first missionaries and colonial administrators had formed of the Tharaka. Secluded from the main trends of colonial presence, they were only marginally involved in the oppressive burdens of the Emergency, which rested heavily on all other Meru groups, especially the Imenti and the Chuka.

The Mugwe, a traditional institution of the Meru, was the main object of my research. He was a highly respected religious dignitary. His blessing was required to assure the welfare of the people and, in a special way, the success of raids by young warriors. He was to be protected from all intruders as "the

queen of the bees." The existence of the Mugwe's office, as I have reported (3), had been kept secret from almost all previous Europeans, a fact that brought me both apprehension and excitement. When I realized that the Mugwe had been a living factor of Meru history and sensed that this social institution was doomed soon to end, I became motivated by a sense of service to the future generations of the Meru.

I confess that on disclosing the Mugwe's existence, I felt a certain intimate embarrassment, as of one encroaching on private grounds. The significance of secrets had always been strongly felt among the East African peoples. A Kikuyu proverb, which also helps us to understand the secret character of the Mau Mau organisation, goes as follows: *andu matari ndundu mahuragwo na njuguna imwe,* "a people without a secret is destroyed by a single stroke." Deeply conscious of this attitude, I feared at first that the Meru were critical and suspicious of my activities, but it soon became apparent that I enjoyed the general confidence of those among whom I worked.

I aimed for the most rigorous objectivity in reporting on my observation of the Meru social life. I avoided, as much as possible, any personal impinge-ment on the ethnographic evidence. That is why my text is so frequently interspersed with the words of my informants. Following Monica Wilson's suggestion, I reported fully some of the most important texts recorded, providing direct evidence for the reader's evaluation. My Meru friends faulted only the generic implication of my study's subtitle, "The *Failing* Prophet." I have accepted their criticism, and in the edition now in press in Nairobi I have changed that subtitle to "A *Blessing* Prophet" (8).

Since my book appeared in 1959, the ethnography of the Mugwe has been the subject of several critical analyses, each inspired by a different theoretical approach. When the late Daryll Forde, the director of the International African Institute and the editor of the journal *Africa,* asked my opinion about Needham's symbolic and structural interpretation of "the left hand of the Mugwe," I said recognized its validity, as best expressed by George Dumé-zil's statement: *le système est dans les faits.* I have had no reason to change my opinion in the face of other polemic opinions (20, 21, 24, 33). The left hand of the Mugwe should, however, be analyzed together with all other aspects of his personality: The Mugwe also has a tail like an animal's, for example, and is compared to the queen of the bees. A global analysis would better respond to the conceptual categorization used by the Meru, who consider the Mugwe as one of their "men of wonder" *(antu ba kurigaria)* (3: 62–67, 72–76).

The Agwe I visited were the last of them all. No Mugwe is now at work in Meruland—none, in any case, relying on the traditional structure of Meru society, which has been markedly altered. Some have tried to adjust the conception of the Mugwe to the present, either for social prestige or as an

adaptation to Christian ideals (see 6: 180–81); but mine was practically the last chance to record the Mugwe institution from living reality. The Mugwe is now a mere historical figure. I was greatly rewarded to note, after Independence on the 12th of December 1963, that in various parts of the District country clinics, coffee houses, roads, and other public facilities had been named after the Mugwe: a clear example of how popular exaltation creates myth out of history.

THE ITALIAN SITUATION

From 1959 to 1969, having been appointed Director of Education at the headquarters of the Consolata missionary Society in Turin, Northern Italy, I traveled extensively through Africa, America, and Europe, though I had no time for any professional anthropological activity.

In 1970, I was called to the chair of Cultural Anthropology in the Faculty of Political Sciences at the University of Bologna, thus beginning the last stage of my anthropological journey. Entering the Italian universities almost as an outsider, I was surprised by the weak position of anthropology and the jingoistic divisions among Italian anthropologists.

Two schools of thought had been formed, one upholding ethnology as the traditional study of exotic and "primitive" societies, the other promoting a new approach in terms of cultural anthropology, exclusively interested in the study of "complex" societies (primarily the study of peasant Italy). This new approach was largely supported by sociologists and by most folklorists. The two schools had a common interest in obtaining more space in the universities. In such a context my appointment to the Institute of Sociology at Bologna University had a special significance, my chair being the first one in Italy professing Cultural Anthropology. As its first incumbent, I proposed, of course, to work for a clarification of all the issues involved.

Conventionally I was considered an ethnologist, but my anthropological identity had grown more complex, because I had been deeply connected with the first Malinowskian generation of social anthropologists—Schapera, Evans-Pritchard, and Fortes. My appointment implicitly disavowed the nominalistic dichotomy setting cultural anthropology against ethnology, and thus my predicament reminded me of the early British situation described by Stocking (32) in his paper, "What's in a name?"

I am convinced that the terms ethnology and anthropology may legitimately be used to refer to the same field of study. Both are derived from essential attributes of the human condition—in one case from our being born as individuals *(anthropos),* in the other from our belonging to a social community *(ethnos).* Unfortunately, the development of so many anthropological schools of thought has caused an undiscriminating use of the two terms, now as if they were synonymous, now as if mutually exclusive.

On becoming more deeply involved with this situation I realized that our task was seriously to rethink anthropology to meet the needs of the Italian universities. With this in mind, in 1972 I convened a conference on the theme "Etnologia e Antropologia Culturale." I made a point of inviting Jack Goody to give an introductory paper on the state of social anthropology in Britain. My aim was to set the Italian situation against an international background and to show how anthropology, as a discipline, was on the move everywhere in the world. I sought to demonstrate that what matters is not where research is carried out, whether in an exotic or a domestic field, but rather whether it employs objective and reliable methods to produce evidence that furthers knowledge and theory (see 4).

At that time, Professor V. L. Grottanelli held the chair of Ethnology at the University of Rome "La Sapienza," whom I was to succeed in 1982. His fieldwork, first in East Africa and later in West Africa, made him an outstanding figure in Italian anthropology. Interested readers may refer to his paper (15) for more information on the Italian situation. Always refractory to pretentious novelty, and almost by right of aristocratic birth attracted by pedigrees (he loves to trace the source of Italian anthropology to Roman classics, like Julius Caesar, etc), Grottanelli has been reproached for neglecting the new trends of cultural anthropology. In 1984, a further paper on the Italian situation was published by G. R. Saunders, who apparently attempted to fill Grottanelli's gaps (27). Although he gathered a long bibliography of Italian works, Saunders was strongly biased towards the new Italian cultural anthropology; he missed the lively polemic that, from the antithesis between ethnology and cultural anthropology, had expanded on Marxist anthropology (see 25). Perhaps neither paper is wholly satisfactory, but from both one can sense how animated the debate among the Italian anthropologists had been. For readers of the Italian language, a recent book on the last one hundred years of Italian anthropology is also suggested (12).

The folklorist tradition, ethnological research, an innovative cultural anthropology, and finally the debate on the relationship between Marxism and anthropology have been the main trends of Italian anthropology over this period. Their outcomes include the introduction of a variety of academic disciplines, besides ethnology, forming a broad ethnoanthropological group (it may interest American readers that "ethnoanthropology" has a long currency in Italian anthropology: cf. 26:423); an emphasis on the mental and ideological implications of the concept of culture; an expanding research on cultural values; and a semiotic approach to cultural phenomena.

On entering the Italian arena I was impressed not so much by the development of a Marxist anthropology as by the emphasis on Gramsci, and if he had been the inspiring source of cultural anthropology. On reading his works, which I did from their first publication, I was impressed by his perceptive analysis of Italian events, but I had not noted any direct contribution to

anthropology (4:232–33). Only through the writings of Alberto M. Cirese did such concepts as culture, hegemony, structure, and so on come to be conceived and discussed with direct reference to Gramsci (see 9, 10: 18–33).

Although I would not say that the Marxist approach has run its course, its fervor everywhere, and certainly in Italian anthropology, has subsided. Rarely has it engaged in either the discussion or radical criticism of ethnographic texts, and it has not led to any deconstructing revisionism. It failed, in any case, to inspire Italian anthropologists to undertake more intense fieldwork. Indeed, I think the weakest aspect of Italian cultural anthropology has been its neglect of direct, sustained research in the field—a weakness, I must hasten to state, that is beginning to be redressed by promising anthropologists of the youngest generation.

Anthropological disciplines are now (at the end of 1989) taught in many an Italian university by 29 professors and 66 associate professors. These figures refer to the whole system of Italian universities. One should include also a large number of junior and freelance professionals; these latter have formed a new society of freelance anthropologists to meet the apparently increasing demand for their profession. Since last year a reform has affected the Italian university system through the creation of a special *Ministero dell' Universitá e della Ricerca Scientifica e Tecnologica*. A law is currently under discussion in Parliament that aims to restore full academic autonomy to each university institution. If such a law passes, one may dream of a new kind of anthropology department in Italy, assembling (on the model of the best French *laboratoires*) Africanists, Americanists, Asiatists, Oceanists, and Europeanists who, sharing their field experience, would together contribute to anthropological theory.

CONTINUITY IN DIVERSITY

At the end of my anthropological journey (22) I am convinced that the essential aim of anthropology is a deeper understanding of the human condition, expressed by the social formations constantly changing under the pressure of each individual's urge to form a personal and ethnic identity. I believe fieldwork to be the only way to acquire reliable evidence about the determinant elements of the human condition. If fieldwork has grown to be the traditional form of anthropological initiation, it is because it affords the only means of observing the human condition as a concrete reality. Only in people's behavior are the patterns of social life revealed in the making and interconnection.

With these premises, I am always puzzled by the claim that only the exotic can provide significant research in anthropology. Anthropologists are too frequently and perhaps unconsciously inclined to attach, as Ioan Lewis puts

it, a "deep metaphysical significance to field work in *exotic* settings" (19:1; italics added). We owe to Lewis's ingenuity a refined and witty analysis of fieldwork, "the anthropologist's Muse"; but I do not share his belief that to-day "The ultimate test of the professional anthropologist's standing remains the successful completion of a major piece of field research in *exotic* surroundings" (19:1). Emphasis on the exotic was indeed correct in the past, when world communications were poor and many peoples, unknown to the Western world and thus considered "out of history," were assigned to the invented category of the so-called "primitives." Anthropologists set themselves the task of exploring the real situation of these societies, and their works contributed to a changed perspective. The image of the world is now deeply altered, having developed into that of a great village wherein mankind shares more and more a common way of life and a common destiny.

The essence of the human condition can never be exotic, being the same (despite its surprising variety of expressions) wherever a human being is found. I am thus impelled by my classical bias to cite the old saying of Terence: *Homo sum; humani nihil a me alienum puto* ("being a man, nothing human is exotic to me").

To Heraclitus is attributed the observation *ethos anthropo daimon:* The demon in our individuality drives us where we would not; our own unavoidable ethnocentrism can cause us to despise and even commit violence upon other peoples. In such a predicament, a thoughtful anthropological approach may be useful, if not decisive, in restoring our judgment and recovering the human solidarity of Terence's aphorism.

Such contradictory situations result from the structure of human personality. The fact that the essential components of that structure are at the same time individual and social constitutes one of the paradoxes of the human condition, affecting us all. Though we share some patterns of behavior with other individuals, we still differ, both as individuals and as social groups. Our individual differences and changing social interests alter our ideologies, our knowledge, and our social norms, however firmly these may have been established by tradition. Contrary to old theories, even myths do change. Change is the basic propeller of the development of human culture and society.

The century now nearing its end has been characterized by such great technological achievements as nuclear fission and the exploration of the cosmos. However, I personally assign greater historical consequence to a different accomplishment: We opened up our concept of history to include those peoples who, less than 50 years ago, were thought to be "out of history." They are now a part of the network of human nations, conscious of their own cultural inheritance, and respectfully accepted by others. This

achievement, like the so-called discovery of the New World and its peoples, has altered our perception of reality, modifying our "common knowledge" and lofty ideologies alike.

It is the business of the anthropologist to explore the human condition, with its variety of ideologies and social institutions. Such a task requires alertness, sensitivity, and the ability to decode observed facts in order to further knowledge without letting preestablished or ethnocentric theories overshadow reality. May these qualities of discernment and perception always be used for the benefit of humanity.

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