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Source: Current Anthropology, Vol. 7, No. 2 (Apr., 1966), pp. 112-123
Published by: The University of Chicago Press on behalf of Wenner-Gren Foundation for Anthropological Research
Stable URL: http://www.jstor.org/stable/2740021
Accessed: 18/01/2010 03:28

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The Scope of Anthropology

by Claude Lévi-Strauss

It was a little more than a year ago, in 1958, that the College of France decided to create in its midst a chair of social anthropology. This science is too attentive to those forms of thought which, when we encounter them among ourselves, we call superstition, for me not to be allowed to render to superstition a preliminary homage; is it not the characteristic of myths, which have such an important place in our research, to evoke a suppressed past and to apply it, like a grid, upon the present in the hope of discovering a sense in which the 2 faces in which man is confronted with his own reality—the historic and the structural—coincide? It would seem to me also permissible on this occasion, on which all the patterns or features of myth are for me reunited, to proceed on their example, seeking to discern in past events the meaning and the lesson of the honor which has been done me, to which, my dear colleagues, the very date of your deliberation bears witness: by the strange recurrence of the number 8, already well-known from the arithmetic of Pythagoras, the periodic table of chemical elements, and the law of symmetry of the medusa-jellyfish, the proposal in 1958 to create a chair of social anthropology revives a tradition which even if I had wished to I would not have been able to escape.

Fifty years prior to your initial decision, Sir James George Frazer delivered the inaugural lecture of the 1st chair of social anthropology in the world, at the University of Liverpool. Fifty years earlier, in 1858, 2 men were born—Franz Boas and Emile Durkheim—whom posterity will regard as, if not the founders, at least the chief engineers, 1 in America and the other in France, of anthropology as we know it today.

It is appropriate that these 3 anniversaries, these 3 names, have been evoked here. Those of Frazer and Boas give me occasion to express my gratitude, if only briefly, for all that social anthropology owes to Anglo-American thought, and for what I owe it personally, since it was in close conjunction with it that my 1st works were conceived and developed. But it will not surprise you that Durkheim occupies a larger place in this lecture. He incarnates the essence of France's contribution to social anthropology, even though his centennial, celebrated with enthusiasm in many foreign countries, passed almost unnoticed here and has not yet been marked by any official ceremony.²

How are we to explain this injustice to him, and to ourselves, if not as a minor consequence of that desperate eagerness which drives us to forget our own history, to hold it “in horror,” in the words of Charles de Remusat? This sentiment today opens social anthropology to the possibility of losing Durkheim as it has already lost Gobineau and Dumezil.

And yet, my dear colleagues, those among you who share these distant memories will not contradict me if I recall that, around 1935, when our Brazilian friends wanted to explain to us the reasons which led them to choose French missions to organize their 1st universities, they always cited 2 names: 1st, of course, Pasteur, and after that Durkheim.

But in reserving these thoughts for Durkheim, I am carrying out another duty. No one would have appreciated more than Marcel Mauss an homage addressed to him at the same time as to the master of whom he was pupil and then successor. From 1931 to 1942, Marcel Mauss held the chair at the College of France consecrated to the study of society, and so brief was the passage in these halls of the unfortunate Maurice Halbwachs that it seems that one can, without being untruthful, consider that in creating a chair of social anthropology, it is Mauss's chair which you wanted to restore. In any case, I owe too much to Mauss's thought not to take pleasure in this notion.

To be sure, his chair was called “Sociology,” for Mauss, who did so much (together with Paul Rivet) to make ethnology a science in its own right, had not completely succeeded by the 1930's. But to attest to the bond between our fields, it will suffice to recall that in Mauss's field ethnology took an ever growing place; that beginning in 1924, he proclaimed that the “place of sociology” was “in anthropology” (Mauss 1950c: 285); and that, if I am not mistaken Mauss was the 1st (in 1938) to introduce the term “social anthropology” into French terminology (Mauss 1950d:362). He would not disavow the term today.

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1 “Inaugural Lecture” delivered on 5 I 60 at the Collège de France. The article was translated by Sherry Artner Paul and Robert Paul.

2 A commemoration took place at the Sorbonne on 30 I 60.
Even in his boldest advances, Mauss never felt that he departed from the Durkheimian line. Better than he, perhaps, we perceive today how, without betraying the fidelity so often affirmed, he knew how to simplify and soften the doctrine of his great precursor. This doctrine has never ceased to astonish us by its imposing proportions and its powerful logical framework, and by the perspectives which it opened onto horizons where so much remains to be explored. Mauss’s mission was to finish and furnish the prodigious edifice sprung from the earth at the passage of the demiurge. He had to exorcise some metaphysical phantoms that were still trailing their chains in it, and shelter it once and for all from the icy winds of dialectic, the thunder of syllogisms, and the lightning flashes of antinomies. But Mauss secured the Durkheimian school against yet other dangers.

Durkheim was probably the 1st to introduce the requirement of specificity into the sciences of man, thereby making possible a renovation from which most of these sciences, and especially linguistics, benefited at the beginning of the 20th century. In all forms of human thought and activity, one cannot ask questions of nature or of origin before having identified and analyzed the phenomena and having discovered to what extent the relations which unite them suffice to explain them. It is impossible to discuss an object, to reconstruct the history which gave it its being, without knowing 1st what it is; in other words, without having exhausted the inventory of its internal determinations.

Yet when one rereads *The Rules of Sociological Method* today, one cannot help thinking that Durkheim has applied these principles with a certain partiality: he appeals to them in order to constitute the social as an independent category, but without recognizing that this new category, in its turn, entails all sorts of specificities corresponding to the different aspects in which we apprehend it. Before affirming that logic, language, law, art, and religion are projections of the social, would it not have been reasonable to wait until the particular sciences had thoroughly explored the mode of organization and the differential function of each of these codes, thus permitting the understanding of the nature of the relations among them?

At the risk of being accused of paradox, it seems to me that in the theory of the “total social fact” (so often praised and so poorly understood), the notion of totality is less important than the very special way in which Mauss conceived of it: foliated, one might say, and made up of a multitude of distinct yet connected planes. Instead of appearing as a postulate, the totality of the social is manifested in experience; these privileged instances which one can apprehend on the level of observation, in well-defined situations, when “the totality of society and its institutions . . . is set in motion.” Now, this totality does not suppress the specific character of phenomena, which remain “at once juridical, economic, religious, and even aesthetic, morphological”; and so it consists finally in the network of functional interrelations among all these planes (Mauss 1950b:274).

This empirical attitude of Mauss’s accounts for his so quickly overcoming the repugnance which Durkheim had felt from the beginning with respect to ethnographic investigation. “What counts,” said Mauss, “is the Melanesian of such-and-such island . . .” (Mauss, 1950b:276). Against the theoretician, the observer should always have last word; and against the observer, the native. Finally, behind the rationalized interpretations of the native—who often makes himself observer and even theoretician of his own society—one will look for the “unconscious categories” which, Mauss wrote in 1 of his 1st works, are determinants “in magic, as in religion, in linguistics” (Mauss 1950a:111). Now, this analysis in depth was to permit Mauss, without contradicting Durkheim (since it was to be on a new plane), to re-establish bridges—which at times had been imprudently destroyed—to the other sciences of man: to history, since the ethnographer deals in the particular; and also to biology and psychology, since he recognized that social phenomena are “first social, but also, and simultaneously, physiological and psychological.” (Mauss 1950c:299). It will suffice to take the analysis far enough to attain a level where, again as Mauss said, “body, soul, society—everything merges” (Mauss 1950c:302).

This healthy sociology considers men as they are depicted by travelers and ethnographers who have partaken of their existence in a fleeting or in a lasting way. It shows them engaged in their own historical development, settled in a concrete, geographic space. It has, says Mauss, “as principle and as end . . . to perceive the entire group and the entire range of its behavior” (Mauss 1950b:276).

If disembodiment was 1 of the perils which lay in wait for Durkheimian sociology, Mauss protected it with equal success against another danger: “automatic explanation.” Too often since Durkheim—and even among some of those who believe themselves to be liberated from his doctrinal grip—sociology has seemed like the product of a raid hastily made at the expense of history, psychology, linguistics, economics, law, and ethnography. To the booty of this pillage, sociology was content to add its label: whatever problem was posed to it, one could be assured of receiving a prefabricated “sociological” solution.

If we have not yet arrived at that state, we owe it in large part to Mauss and to Malinowski. At the same time, and no doubt aided by one another, they showed—Mauss as theoretician, Malinowski as experimenter—what could constitute proof in the ethnological sciences. They were the 1st to understand clearly that it was not enough to break down and dissect. Social facts do not reduce to scattered fragments. They are lived by men, and that subjective consciousness is as much a form of their reality as their objective characteristics.

While Malinowski was instituting uncompromising participation of the ethnographer in the life and thought of the natives, Mauss was affirming that what is essential “is the movement of all, the living aspect, the fleeting instant in which society becomes, or in which men become, sentimentally conscious of themselves and of their situation vis-à-vis others” (Mauss 1950b:275). This empirical and subjective synthesis offers the only guarantee that the preliminary analysis,
carried as far as the unconscious categories, has let nothing escape.

Without a doubt, the proof will remain largely illusory: we will never know if the other, with whom we cannot, after all, identify, makes from the elements of his social existence a synthesis exactly superposable on that which we have worked out. But it is not necessary to go so far; all that is needed—and for this, inner feeling is sufficient—is that the synthesis, however approximate, arises from human experience. We must be sure of this, since we study men; and as we are ourselves men, we have that possibility. The way in which Mauss (1950:285) poses and resolves the problem in the Essay on the Gift brings to view, in the intersection of 2 subjectivities, the nearest order of truth to which the sciences of man can aspire when they confront the wholeness of their object.

Let us make no mistake: all this which seemed so new was implicit in Durkheim. He has often been reproached for having formulated, in the 2nd part of The Elementary Forms of the Religious Life, a theory of religion so vast and so general that it seemed to render superfluos the minute analysis of Australian religions which preceded it and—one hoped—paved the way for it.

The problem is to know if Durkheim the man could have arrived at this theory without being forced, at the outset, to superimpose upon the religious representations received from his own society those of men who historical and geographical evidence guarantees were entirely “others,” not accomplices or unsuspected acolytes. Such is certainly the approach of the ethnographer when he goes into the field, because—however scrupulous and objective he may want to be—it is never either himself or the other whom he encounters at the end of his investigation. At most he can claim to extricate, by the superposition of himself on the other, what Mauss called the facts of general functioning, which he showed were more universal and had even more reality.

In thus completing the intention of Durkheim, Mauss liberated anthropology from the false opposition (introduced by thinkers such as Dilthey and Spengler) between explanation in the physical sciences and explanation in the human sciences. The search for causes ends with the assimilation of an experience, but this is at once external and internal. The famous rule to “consider social facts as things” corresponds to the 1st step, the search for causes, which are left to the 2nd to validate. We already discern the originality of social anthropology: it consists not in opposing causal explanation and understanding, but in bringing to light an object which may be at the same time objectively very remote and subjectively very concrete, and whose causal explanation may be based on that understanding which is, for us, but a supplementary form of proof. A notion like that of empathy inspires great mistrust in me, because it connotes irrationalism and mysticism. In his demand for additional proof, I prefer to imagine the anthropologist modeled after the engineer, who conceives and constructs a machine by a series of rational operations: it has to work; logical certainty is not enough. The possibility of trying the intimate experiences of another upon oneself is but 1 of the means at one’s disposal for obtaining that final empirical satisfaction for which the physical sciences and the human sciences equally feel the necessity: it is less a proof, perhaps, than a guarantee.

* * *

What, then, is social anthropology? No one, it seems to me, was closer to defining it—if only by omission—than Ferdinand de Saussure, when, presenting linguistics as 1 part of a science yet to be born, he reserved for this science the name semiology and attributed to it as its object of study the life of signs at the heart of social life. Did he not, furthermore, anticipate our adherence when he compared language to “writing, to the alphabet of deaf-mutes, to symbolic rites, to forms of politeness, to military signals, etc.?” (de Saussure 1960:33). No one would deny that anthropology counts within its own field at least some of these systems of signs, along with many others: mythic language, the oral and gestural signs of which ritual is composed, marriage rules, kinship systems, customary laws, and certain terms and conditions of economic exchange.

I conceive, then, of anthropology as the occupant in good faith of that domain of semiology which linguistics has not already claimed for its own, pending the time when for at least certain sections of this domain, special sciences are set up within anthropology.

It is necessary, however, to make this definition more precise in 2 ways.

First of all, I hasten to recognize that certain items which have just been cited are already within the scope of particular sciences: economics, law, political science. However, these disciplines examine the very facts which are closest to us and thus of particular interest. Let us say that social anthropology apprehends these facts, either in their most distant manifestations, or from the angle of their most general expression. From this latter point of view, anthropology can do nothing useful without collaborating closely with the particular social sciences; but these, for their part, would not know how to aspire to generality were it not for the cooperation of anthropology, which alone is capable of bringing them the accounts and the inventories which it seeks to render complete.

The 2nd difficulty is more serious, because one can ask oneself whether all the phenomena in which social anthropology is interested indeed manifest the character of symbols. This is sufficiently clear for the problems we study most frequently. When we consider some system of belief (let us say totemism), some form of social organization (unilineal clans, bilateral cross-cousin marriage), the question which we ask ourselves is, "What does all this mean?", and to answer it, we force ourselves to translate into our language rules originally stated in a different language.

But is this true of other aspects of social reality, such as tool-making, techniques, and modes of production and of consumption? It would seem that we are concerned here with objects, not with signs—the sign being, according to Peirce’s celebrated definition, “that which replaces something for someone.” What, then, does a stone axe replace, and for whom?

The objection is valid up to a certain point, and it
explains the repugnance which some people feel toward admitting phenomena which come from other sciences, such as geography and technology, into the field of social anthropology. The term "cultural anthropology" will be appropriate, then, to distinguish this part of our studies.

However, it is well known—and it is 1 of Mauss's claims to fame that he established this, along with Malinowski—that in the societies with which we are concerned above all, but also in others, these domains are pregnant with meaning. From this point of view, they still concern us.

Finally, the intention of being exhaustive which inspires our researches broadly transforms their object. Techniques taken in isolation may appear as raw fact, historical heritage, or the result of compromise between the needs of man and the constraints of environment. But when one puts them into that general inventory of societies which anthropology is trying to construct, they re-emerge in a new light, for we imagine them as the equivalents of choices each society seems to make (I here use convenient language, which must be stripped of its anthropomorphism) among the possible ones which will constitute the complete list. In this sense, a certain type of stone axe can be a sign: in a given context, for the observer capable of understanding it, it takes the place of the different implement which another society employs for the same purpose.

Consequently, then, even the simplest techniques of any primitive society have hidden in them the character of a system, analyzable in terms of a more general system. The manner in which some elements of this system have been retained and others excluded permits of conceiving of the local system as a totality of significant choices, compatible or incompatible with other choices, which each society, or each period within its development, has been led to make.

* * *

In admitting the symbolic nature of its object, social anthropology does not thus intend to cut itself off from realia. How could it do this, when art, in which all is sign, utilizes material media? One cannot study the gods without knowing their images; rites, without analyzing the objects and the substances which the officiant makes or manipulates; social rules independently of the things which correspond to them.

Social anthropology does not confine itself to a part of the domain of ethnology; it does not separate material and spiritual culture. In the perspective which is its own, and in which we must find a place, it brings to each of them the same interest. Men communicate by means of symbols and signs; for anthropology, which is a conversation of man with man, everything is symbol and sign, when it acts as intermediary between 2 subjects.

By this deference toward objects and techniques, as well as by the conviction that we must work on meanings, social anthropology becomes appreciably different from Radcliffe-Brown's conception who—right up to his untimely death in 1955—did so much to give autonomy to our science.

According to the always marvelously clear views of the English master, social anthropology is to be an inductive science which, like other sciences of this type, observes facts, formulates hypotheses, and submits these to the control of the experiment, in order to discover general laws of nature and society. It thus sets itself apart from ethnology, which tries to reconstruct the past of primitive societies, but with means and methods so precarious that it can teach social anthropology nothing.

When it was formulated, around 1920, this conception—inspired by the Durkheimian distinction between circumfusa and praeitra—marked a salutary reaction to the abuses of the diffusionist school. But, since then, "conjunctural history," as Radcliffe-Brown called it, not without contempt, has perfected and refined its methods, thanks especially to stratigraphic excavations, the introduction of statistics into archaeology, the analysis of pollens, and the use of carbon-14, and above all the closer and closer collaboration between ethnologists and sociologists, on the one hand, and archaeologists and prehistorians, on the other. One may well ask oneself, then, if Radcliffe-Brown's mistrust of historical reconstructions did not correspond to a stage of scientific development which will soon have passed.

On the other hand, several of us hold more modest views on the future of social anthropology than those encouraged by the great ambitions of Radcliffe-Brown. These views picture social anthropology not on the model of the inductive sciences as they were conceived in the 19th century, but rather as a taxonomy, whose purpose is to identify and to classify types, to analyze their constituent parts, and to establish correlations between them. Without this preliminary work—and let us not deceive ourselves; it has barely been begun—the comparative method recommended by Radcliffe-Brown in fact risks being kept at a standstill: either the facts which one proposes to compare are so close to each other geographically or historically that one is never certain one is dealing with distinct phenomena, or they are too heterogeneous, and the comparison is illegitimate because it brings together things which one cannot compare.

Up until a few years ago, we assumed that the aristocratic institutions of Polynesia were phenomena of recent introduction, the result of the arrival from elsewhere of small groups of conquerors scarcely a few centuries ago. But now the measurement of the residual radioactivity of organic remains from Melanesia and Polynesia reveals that the difference between the dates of occupation of the 2 regions is less than was supposed. All at once, the conceptions about the nature and unity of the feudal system must be modified; for at least in this part of the world, it can no longer be denied, after the fine work of Guiart, that some kind of feudalism existed prior to the arrival of the conquerors, and that certain forms of feudalism can arise in humble gardening societies (Guiart, 1963a, b).

The discovery in Africa of the art of Ifé, as refined and masterful as that of the European Renaissance, but perhaps earlier by 3 or 4 centuries, and much preceded in Africa itself by the art of the so-called Nok civilization, influences our conceptions of the recent
arts of black Africa and the corresponding cultures. We are now tempted to see them as impoverished, rustic replicas of high art forms and high civilizations. The shortening of the prehistory of the Old World and the lengthening of that of the New which carbon-14 has permitted us to recognize will perhaps lead us to judge that the civilizations which developed on the 2 sides of the Pacific were even more akin than it appears and to understand them differently, each in its own terms.

We must lean toward facts of this order before tackling any classification or comparison. For if we hasten to postulate the homogeneity of the social field, and if we cherish the illusion that it is immediately comparable in all its aspects and on all its levels, we will let the essential fact escape. We will fail to appreciate that the coordinates required for defining 2 apparently very similar phenomena are not always the same, or of the same number; and we will believe we are formulating laws of social nature when in fact we will only be describing superficial properties or enunciating tautologies.

To scorn the historical dimension on the pretext that the means are insufficient to evaluate it except approximately will result in our being satisfied with an impoverished sociology, in which the phenomena are disengaged, as it were, from their foundations. Rules and institutions, states and processes seem to float in a void in which one strains to spread a tenuous net of functional relations. One becomes wholly absorbed in this task, and one forgets the men in whose thought these relationships are established, one neglects their concrete culture, one no longer knows whence they came and what they are.

It is not sufficient, indeed, that phenomena can be called social in order for anthropology to be eager to claim them as its own. Espinas, another of the masters we allow ourselves the luxury of forgetting, was certainly right from the point of view of social anthropology when he refused to accept the notion that institutions shorn of their biological roots have the same coefficient of reality as other things: “The administration of a great railroad company,” he wrote in 1901, “is not at all a social reality . . . nor is an army” (Espinas, 1901:470).

The statement is excessive, since administrations are the object of thorough studies in sociology, in social psychology, and in other particular sciences; but it helps us to specify the difference between anthropology and the preceding disciplines: the social facts which we study are manifested in societies each of which is a total entity, concrete and cohesive. We never lose sight of the fact that existing societies are the result of great transformations occurring in mankind at certain moments in prehistory and at certain places on the earth, and that an uninterrupted chain of real events relates these facts to those which we can observe.

The chronological and spatial continuity between the natural order and the cultural order upon which Espinas insisted strongly (in a language which is not our own and which, for that reason, we have sometimes poorly understood), is also the basis of Boas’ historicism. It explains why anthropology, even social anthropology, proclaims itself to be joined in a common cause with physical anthropology, whose discoveries it watches for eagerly. For, even if social phenomena ought to be provisionally isolated from the rest and treated as if they arose from a specific level, we know well that in fact, and even rightfully so, the emergence of culture will remain a mystery to man: he will not succeed in determining, on the biological level, the modifications of the structure and functioning of the brain of which culture was both the natural result and the social mode of apprehension, and which at the same time created the intersubjective milieu indispensable for further transformations. These transformations, although certainly anatomical and physiological, can be neither defined nor studied with reference only to the individual.

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This historian’s profession of faith will come as a surprise, because I have at times been reproached for being closed to history and for giving it a negligible place in my works. I do not practice it much, but I am determined to reserve it its rights. I simply believe that in this formative period of social anthropology, nothing would be more dangerous than an unmethodical eclecticism which seeks to give the illusion of a finished science by confusing its tasks and mixing its programs.

Now it happens that in anthropology, experimenta tion precedes both observation and hypothesis. One of the peculiarities of the small societies which we study is that each constitutes a complete experiment, because of its relative simplicity and the limited number of variables required to explain its functioning. But on the other hand, these societies are alive, and we have neither the time nor the means to manipulate them. By comparison with the natural sciences, we benefit from an advantage and suffer an inconvenience: we find our experiments already prepared, but they are ungovernable. We thus are forced to substitute for them models, systems of symbols which preserve the characteristic properties of the experiment, but which we can manipulate.

The boldness of such an approach is, however, compensated for by the humility—one might almost say the servility—of observation as it is practiced by the anthropologist. Leaving his country, his hearth, for long periods; exposing himself to hunger, sickness, and sometimes danger; surrendering his habits, his beliefs, his convictions to a profanation to which he becomes an accomplice when, without mental reservation or ulterior motive, he assumes the forms of life of a strange society, the anthropologist practices total observation, beyond which there is nothing except—and it is a risk—the complete absorption of the observer by the object of his observations.

This rhythmic alternation between 2 methods—the deductive and the empirical—and the strictness with which we practice each in its extreme and purified form give social anthropology its distinctive character: of all the sciences, it is without a doubt unique in making the most intimate subjectivity into a means of objective demonstration. The same mind which has abandoned itself to the experience and allowed itself to be modeled by it becomes the theater of mental operations which, without suppressing the experience, nevertheless transform it into a model which makes possible further mental operations. In the last analysis,
the logical coherence of these mental operations is based on the sincerity and honesty of him who can say, like the explorer bird of the fable, "I was there; such-and-such happened to me; you will believe it to be there yourself," and who in fact succeeds in communicating that conviction.

But this constant oscillation between theory and observation requires that the 2 planes always be distinguished. To return to history, it seems to me that the same holds true, whether one devotes oneself to the static or to the dynamic, to the order of structure or to the order of the event. The history of the historians does not need to be defended, but it is not attacking it to say (as Braudel admits) that next to a short time there exists a long time; that certain facts arise from a statistical and irreversible time, others from a mechanical and reversible time; and that the idea of a structural history contains nothing which could shock historians (Braudel 1954). The 2 come together, and it is not contradictory that a history of symbols and signs engenders unforeseeable developments, even though it brings into play a limited number of structural combinations. In a kaleidoscope, each recomposition of identical elements yields new results; but it is because the history of the historians is present—in the succession of flicks of the finger, as it were, which bring about the reorganization of the structure—and because the chances are practically nil that the same arrangement will appear twice.

I do not intend by this to take up again, in its original form, the distinction introduced in the Course in General Linguistics between the synchronic and the diachronic orders. From this aspect of the Saussurian doctrine, modern structuralism, along with Troubetzkoy and Jakobson, has most resolutely diverged; and recent documents show that the editors of the Course ... may at times have forced and schematized the master's thought (Godel 1957).

For the editors of the Course in General Linguistics, there exists an absolute opposition between 2 categories of fact: on the 1 hand, that of grammar, the synchronic, the conscious; on the other hand, that of the phonetic, the diachronic, the unconscious. Only the conscious system is coherent; the unconscious infra-system is dynamic and out of balance, composed at once of the legacy of the past and the tendencies of the future not yet realized.

In fact, Saussure had not yet discovered the presence of differential elements behind the phoneme. His position indirectly foreshadowed, on another plane, that of Radcliffe-Brown, who was convinced that structure is of the order of empirical observation, when in fact it is beyond it. This ignorance of hidden realities leads the 2 men to opposite conclusions. Saussure appears to deny the existence of a structure where it is not immediately given; Radcliffe-Brown affirms it but, seeing it where it is not, he deprives the notion of structure of its full force and significance.

In anthropology, as in linguistics, we know today that the synchronic can be as unconscious as the diachronic. In this sense the divergence between the 2 is already reduced.

On the other hand, the Course in General Linguistics sets forth relations of equivalence between the phonetic, the diachronic, and the individual, which form the domain of the parole; and the grammatical, the synchronic, and the collective, which are the domain of the langue. But we have learned from Marx that the diachronic can also exist in the collective, and from Freud that the grammatical can be achieved entirely within the individual.

Neither the editors of the Course nor Radcliffe-Brown sufficiently realized that the history of systems of symbols includes logical evolutions which relate to different levels of structure and which it is necessary first to isolate. If a conscious system exists, it can only result from a sort of "dialectical average" among a multiplicity of unconscious systems, each of which concerns 1 aspect or 1 level of social reality. Now, these systems do not coincide either in their logical structures or in their historical affiliations. They are as if diffracted upon a temporal dimension, whose thickness gives the synchronism its consistency, and lacking which the synchronism would dissolve into a tenuous and impalpable essence, a phantom of reality.

It would thus not be going too far to suggest that in its oral expression, the teaching of Saussure must not have been very far from these profound remarks of Durkheim, which, published in 1900, seem to have been written today:

Without a doubt, the phenomena which concern structure are somewhat more stable than functional phenomena, but between the 2 orders of facts there is only a difference of degree. Structure itself occurs in the process of becoming ... it is ceaselessly breaking down and being reconstituted, it is life arrived at a certain degree of consolidation; and to distinguish the life whence it derives from the life which it determines would be to dissociate inseparable things (Durkheim 1953:190).

In truth, it is the nature of the facts we study which leads us to distinguish within them that which belongs to structure and that which belongs to the process. As important as the historical perspective may be, we can only attain it at the end: after long researches which—as radiocarbon dating and palynology demonstrate—are not even always within our competence. On the other hand, the diversity of human societies and their number—several thousand still at the end of the 19th century—make it seem to us as if they were displayed in the present. It is not so astonishing if, responding to this solicitation of our object, we adopt a method of transformations rather than of fluxions.

As a matter of fact, there is a very close relationship between the idea of transformation and the idea of structure which is so important in my works. Radcliffe-Brown, inspired by the ideas of Montesquieu and Spencer, introduced the latter into social anthropology, to designate the durable manner in which individuals and groups are connected within the social body. For him, consequently, structure is of the order of a fact; it is given in the observation of each particular society. This view proceeds, no doubt, from a certain conception of the natural sciences, but one which would have already been unacceptable for a Cuvier.

No science can today consider the structures within its jurisdiction as reducing to just any arrangement of
just any parts. Only that arrangement is structured which meets 2 conditions: that it be a system, ruled by an internal cohesiveness; and that this cohesiveness, inaccessible to observation in an isolated system, is revealed in the study of transformations, through which the similar properties in apparently different systems are discovered. As Goethe wrote:

All forms are similar, and none are the same,
So that their chorus points the way to a hidden law.

This convergence of scientific perspective is very comforting for the semiological science of which social anthropology is a part, since signs and symbols can only play their roles insofar as they belong to systems, regulated by internal laws of implication and exclusion, and since the property of a system of signs is to be transformable, in others words, *translatable*, into the language of another system with the aid of permutations. That such a conception could be born in paleontology leads social anthropology to nourish a secret dream: it belongs to the human sciences, as its name adequately proclaims; but while it resigns itself to making its purgatory beside the social sciences, it surely does not despair of awakening among the natural sciences at the hour of the last judgment.

I shall attempt to show by 2 examples how social anthropology now endeavors to justify its program.

We know what function the incest prohibition fulfills in primitive societies. By casting the sisters and daughters out of the consanguineal group, so to speak, and by assigning them to husbands who belong to other groups, it creates bonds of alliance between these natural groups, the 1st such bonds which one can call social. The incest prohibition is thus the basis of human society, and in a sense it is the society.

To justify this interpretation, we did not proceed inductively. How could we have done so, with phenomena which are universally correlated, but among which different societies have invented all sorts of strange connections? Moreover, this is not a matter of facts but of meanings. The question we asked ourselves was that of the *meaning* of the incest prohibition (what in the 18th century was called its "spirit") and not its *results*, real or imaginary. It was necessary, then, to establish the systematic nature of each kinship terminology and its corresponding marriage rules. And this was possible only by elaborating the system of these systems and putting them into a relationship of transformations among themselves. From this point on, what had been nothing but an immense disorder was organized in the form of a grammar: terms constraining in all conceivable ways to set up and maintain a system of reciprocity.

This is where we are now. And now, how should we proceed to answer the next question, that of the universality of these rules in the totality of human societies, including contemporary ones? Even if we do not define the incest prohibition in terms of the form it takes among the Australians or the Amerindians, does the form it takes among us still have the same function? It could be that we are attached to it for very different reasons, such as the recent discovery of the harmful consequences of consanguineal unions. It could also be—as Durkheim thought—that the institution no longer plays a positive role among us and that it survives only as a vestige of obsolete beliefs, anchored in collective thought. Or, is it not rather the case that our society, a particular instance in a much vaster family, depends, like all others, for its cohesiveness and for its very existence on a network—grown infinitely unstable and complicated among us—of ties between consanguineal families? If so, is it necessary that the network be homogeneous in all its parts, or must we recognize therein types of structures differing according to environments or regions and variable as a function of local historical traditions?

These problems are essential for anthropology, since the response to them will determine the intimate nature of the social fact and its degree of plasticity. Now, it is impossible to settle this once and for all using methods borrowed from the logic of John Stuart Mill. We cannot vary the complex relationships—on the technical, economic, professional, political, religious, and *biological* planes—which a contemporary society presupposes. We cannot interrupt and re-establish them at will in the hope of discovering which ones are indispensable to the existence of the society as such, and which ones, if it had to, it could do without.

But we could choose the most complex and least stable of those matrimonial systems whose function of reciprocity is best established; we could then construct models of them in the laboratory to determine how they would function if they involved increasing numbers of individuals; we could also distort our models in the hope of obtaining models of the same type but even more complex and unstable; and we could compare the cycles of reciprocity thus obtained with the simplest cycles it is possible to observe in the field, e.g., in regions characterized by small isolated groups. By means of successive trips from laboratory to field, from field to laboratory, we would try gradually to fill in the void between the 2 series—one known, the other unknown—by intercalating a series of intermediary forms. In the end, we would have done nothing but elaborate a language whose sole merits would be that it would be coherent, like all language, and that it would render an account of phenomena until then thought to be very different by the application of a small number of rules. In the absence of an inaccessible truth of fact, we would have arrived at a truth of reason.

** The 2nd example relates to problems of the same type carried to another level: it will still be concerned with the incest prohibition, but no longer in the form of a system of rules—rather, in the form of a theme of mythic reflection.

The Iroquois and Algonquin Indians tell the story of a young girl exposed to the amorous enterprises of a nocturnal visitor whom she believes to be her brother. Everything seems to point to the guilty one: physical appearance, clothing, and the scratched cheek which bears witness to the virtue of the heroine. Formally accused by her, the brother reveals, that he has a counterpart, or more exactly, a double, for the tie between them is so strong that any accident befalling the 1 is automatically transmitted to the other. To convince his incredulous sister, the young man kills his double before her, but with the same
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This is, apparently, a formula completely devoid of sense. And yet, it springs to mind that there are myths, or fragments of myths, which derive their dramatic force from this structure—a symmetrical inversion of the other. Time is too limited for me to recount the American examples. I will therefore restrict myself to reminding you of the death of the Buddha, rendered inevitable because a disciple fails to ask the expected question. Closer to home, there are the old myths adapted in the Holy Grail cycle, in which the action hangs on the timidity of the hero in the presence of the magic vessel, of which he does not dare to ask, “What good is it?”

Are these myths independent, or must they be considered in turn as a species of a vaster genus, of which Oedipal myths constitute only another species? Repeating the preceding step, we will look to see if, and to what extent, the characteristic elements of 1 group can be reduced to permutations (which will here be inversions) of the characteristic elements of the other group. And that indeed is what is produced: from a hero who misuses sexual intercourse (since he carries it as far as incest), we pass to a chaste man who abstains from it; a shrewd person who knows all the answers is replaced by an innocent who does not even know that he should ask questions. In the American variants of this 2nd type, and in the Holy Grail cycle, the problem to be resolved is that of the “gaste pays,” that is to say, the lost summer. Now, all the American myths of the 1st or “Oedipal,” type refer to an eternal winter which the hero dispels when he solves the puzzles, thereby bringing on the summer. Simplifying a great deal, Perceval then appears as an inverted Oedipus—a hypothesis we would not have dared to consider had it been necessary to compare a Greek with a Celtic source, but which is forced upon us in a North American context, where the 2 types are present in the same populations.

However, we are not at the end of our demonstration. We have verified that, at the heart of the semantic system, chastity and “the answer without a question” are in a relationship to that of incestuous intercourse and “the question without an answer”; therefore we must also admit that the 2 stated in socio-biological terms are themselves in a homologous relationship with the 2 stated in grammatical terms. Between the puzzle solution and incest there exists a relationship, not external and of fact, but internal and of reason, and that indeed is why civilizations as different as those of classical antiquity and indigenous America can independently associate the 2. Like the solved puzzle, incest brings together elements sworn to remain separate; the son marries the mother, the brother marries the sister, in the same way in which the answer succeeds, contrary to all expectation, in rejoining its question.

In the legend of Oedipus, then, the marriage with Jocasta does not arbitrarily follow the victory over the sphinx. Besides the fact that myths of the Oedipal type by definition always assimilate the discovery of incest to the solution of a living puzzle personified by the hero, their various episodes are repeated on different planes and in different languages and provide the same demonstration which one finds in an invert-
ed form in the old myths of the Holy Grail: the audacious union of masked words or of consanguines concealed from themselves engenders decay and fermentation, the unchaining of natural forces—one thinks of the Theban plague—just as impotence in sexual substance dries up animal and vegetable fertility (as well as preventing a proposed dialogue).

In the face of the 2 perspectives which might capture the imagination—an equally eternal summer or winter, the former licentious to the point of corruption, the latter pure to the point of sterility—man must resign himself to preferring the equilibrium and the periodicity of the seasonal rhythm. In the natural order, the latter fulfills the same function which is fulfilled on the social plane by the exchange of women in marriage and the exchange of words in conversation, when these are practiced with the frank intention of communicating, that is to say, without ruse or perversity, and above all, without hidden motive.

* * *

I have been satisfied simply to sketch in the broad outlines of a demonstration—which will be taken up again in detail at some future time—to illustrate this problem of invariance which social anthropology seeks to resolve. The other sciences are concerned with this problem too, but for anthropology it seems like the modern form of a question which it has always asked itself—that of the universality of human nature. Do we not turn our back on this human nature when, in order to sift out our invariants, we replace the givens of experience with models upon which we are free to perform abstract operations as the algebraist does with his equations? I have sometimes been reproached for this, but, apart from the fact that the objection carries little weight with the expert—who knows with what fastidious fidelity to concrete reality he pays for the liberty of skimming for a few brief moments—I would like to recall that in proceeding as it does, social anthropology is only reassuming responsibility for a forgotten part of the program which Durkheim and Mauss mapped out.

In the preface to the 2nd edition of The Rules of Sociological Method, Durkheim defends himself against the charge of having unjustifiably separated the collective from the individual. This separation, he says, is necessary, but it does not preclude the possibility that in the future we will come to conceive of the possibility of a completely formal psychology which would be a sort of common ground of individual psychology and sociology.... what would be necessary would be to seek, by the comparison of mythic themes, legends, popular traditions, and languages, in what way social representations call for each other or are mutually exclusive, merge with one another or remain distinct.... (Durkheim, 1960:xxi-xxi).

This research, he noted in closing, is on the whole under the jurisdiction of abstract logic. It is curious to note how close Lévy-Bruhl could have come to this program if he had not chosen at the outset to relegate mythic representations to the antechamber of logic, and if he had not rendered the separation irremediable when he later renounced the notion of prelogical thought. In so doing, he was simply throwing out, as the English say, the baby with the bath: he denied to the "primitive mentality" the cognitive character which he had initially conceded to it, and cast it back entirely into the realm of emotion.

More faithful to the Durkheimian conception of an "obscure psychology" underlying social reality, Mauss orients anthropology "toward the study of what is common to men.... Men communicate by symbols... but they can only have these symbols and communicate by them because they have the same instincts" (Mauss 1950:296).

Does not such a conception, which is also my own, expose one's flank to another criticism? If your final goal, someone will say, is to arrive at certain universal forms of thought and morality (for the Essay on the Gift ends with conclusions on morals), why give a privileged status to the societies which you call primitive? Shouldn't one in theory arrive at the same results starting from any society? This is the last problem which I would like to consider here.

This is all the more vital since some ethnologists and sociologists who study societies in rapid transformation will perhaps dispute the conception which I seem implicitly to hold of primitive societies. Their putative distinctive character, they may believe, verges on an illusion which is the effect of our present ignorance of what is actually going on; objectively, they do not correspond to reality.

Without a doubt, the character of ethnographic investigations is changing as the little savage tribes we used to study disappear; these investigations are coming to rely on vaster wholes in which the problems tend to resemble our own. But if it is true, as Mauss taught us, that ethnology is an original mode of knowledge rather than a source of particular bits of knowledge, we can only conclude that today ethnology is conducted in 2 ways: in the pure state and in the diluted state. To seek to develop it where its method is mixed with other methods, where its object is confused with other objects, is not the course of action resulting from a sound scientific attitude. This chair will therefore be consecrated to pure ethnology, which does not mean that its teaching cannot be applied to other ends, nor that it is not interested in contemporary societies, which, at certain levels and under certain aspects, are immediately relevant for ethnological method.

What, then, are the reasons for our predilection for those societies which, in the absence of a better term, we call primitive, although they certainly are not that?

The 1st reason, let us frankly acknowledge, is of a philosophic order. As Merleau-Ponty has written, each time the sociologist [but it is the anthropologist be means] returns from the living sources of his knowledge to that which operates in him as a means of understanding the cultural formations most remote from himself, he spontaneously makes philosophy (Merleau-Ponty 1960:138).

In fact, the field research with which every ethnologic career begins is mother and nurse of doubt, the philosophic attitude par excellence. This "anthropological doubt" does not only consist of knowing that one knows nothing, but of resolutely exposing that which one thought one knew, and indeed one's very ignorance, to the insults and contradictions which are
directed at one’s most cherished ideas and habits by those who can contradict them to the highest degree. Contrary to appearances, I think it is by its more strictly philosophic method that ethnology is distinguished from sociology. The sociologist objectivizes for fear of being duped. The ethnologist does not experience this fear, since he is not immediately concerned by the distant society which he studies and since he is not compelled in advance to extract all its nuances, all its details, and even its values—in a word, all that in which the observer of his own society risks being implicated.

However, in choosing a subject and an object radically distant from one another, anthropology runs a risk: that the knowledge obtained from the object does not reach its intrinsic properties but is limited to expressing the relative and always changing position of the subject in relation to that object. It is highly possible, indeed, that so-called ethnological knowledge is condemned to remain as bizarre and inadequate as that which an exotic visitor would have of our own society. The Kwakiutl Indian whom Boas sometimes invited to New York to serve him as an informant was indifferent to the spectacle of skyscrapers and streets lined with automobiles. He respected all his intellectual curiosity for the dwarfs, giants, and bearded ladies which were at that time exhibited in Times Square, for automats, and for the brass balls decorating staircase bannisters. For reasons which I cannot go into here, all these things challenged his own culture, and it was that culture alone which he was seeking to recognize in certain aspects of ours.

In their own way, do not ethnologists succumb to the same temptation when they permit themselves, as they so often do, to interpret indigenous customs and institutions in the light of new charges, with the unacknowledged goal of making them fall into line better with the theories of the day? The problem of totemism, which some of us hold to be transparent and insubstantial, has weighed upon ethnographic thought for years, and we understand now that its importance proceeds from a certain taste for the obscene and the grotesque which is for the science of religion like a childhood disease: a negative projection of an uncontrollable fear of the sacred from which the observer has not been able to disengage himself. Thus the theory of totemism is constructed “for us,” not “in itself,” and nothing guarantees that in its current forms it does not still proceed from a similar illusion.

The ethnologists of my generation are disconcerted by the repulsion inspired in Frazer by the research to which he had dedicated his life: “tragic chronicles,” he wrote of the errors of man, “foolish, vain efforts, lost time, frustrated hopes” (Frazer 1936:vi). We are hardly less surprised to learn from the Notebooks how a Lévy-Bruhl considered myths, which according to him “no longer have any effect on us . . . strange narratives, not to say absurd and incomprehensible . . . it costs us an effort to take an interest in them . . . .” Of course, we have acquired direct knowledge of exotic forms of life and thought which our precursors lacked; but is it not also the case that surrealism—an internal development of our own society—has transformed our sensitivity, and that we are indebted to it for having discovered or rediscovered at the heart of our studies a certain lyricism and integrity?

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Let us then resist the seductions of a naive objectivism, but without failing to recognize that, by its very precariousness, our position as observer brings us unhoped-for assurances of objectivity. It is in the degree to which the so-called primitive societies are distant from our own that we can discover in them those “facts of general functioning” of which Mauss spoke, which have the chance of being “more universal” and of having “more of reality” (Lévy-Bruhl 1949:200). In these societies—and I am still quoting Mauss—“one grasps men, groups, and behavior . . . one sees them driven as in piece of machinery . . . one sees masses and systems” (Mauss 1950b:276). This observation, privileged by distance, no doubt implies certain differences in nature between these societies and our own: astronomy demands not only that the celestial bodies be distant, but also that the passage of time there have a different rhythm; otherwise, the earth would have ceased to exist long before astronomy was born.

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Of course the so-called primitive societies exist in history; their past is as old as ours, since it goes back to the origin of the species. In the span of thousands of years they have undergone all sorts of transformations; they have gone through periods of crisis and of prosperity; they have known wars, migrations, adventure. But they have specialized in ways different from those which we have chosen. Perhaps they have, in certain respects, remained closer to very ancient conditions of life, but this does not preclude the possibility that in other respects they are farther from those conditions than we are.

Although they exist in history, these societies seem to have elaborated or retained a particular wisdom which incites them to resist desperately any modifications of their structure which would permit history to invade their midst. Those which have best protected their distinctive character appear to be societies which inspire in their members a predominant concern for persevering in their existence. The way in which they exploit the environment guarantees both a modest standard of living and the conservation of natural resources. Their marriage rules, though diverse, manifest to the eye of the demographer a common function, namely, to set the upper limit on the fertility rate and to keep it constant. Finally, a political life based on consent and admitting of no decisions other than those unanimously arrived at seems conceived to preclude the possibility of employing that driving force of collective life which takes advantage of the contrast between power and opposition, majority and minority, exploiters and exploited.

In a word, these societies, which are “cold” in that their internal environment is near the zero of historical temperature, are, by their limited total manpower and their mechanical mode of functioning, distinguished from the “hot” societies which appeared in different parts of the world following the Neolithic revolution and in which differentiations between castes and between classes are urged unceasingly in order to extract energy from them.
The value of this distinction is mainly theoretical, because probably no concrete society exists which corresponds exactly to 1 or the other type. And in another sense also the distinction remains relative, if it is true, as I believe, that social anthropology responds to a double motivation: retrospective, since the various types of primitive life are on the point of disappearing and we must hasten to cum our lessons from them; and protractive, to the extent that, being conscious of an evolution whose tempo is ever increasing, we feel ourselves already the "primitives" of our great-grandchildren, and to the extent that we seek to validate ourselves by drawing closer to those who were—and still are, for a brief moment—like a part of us which continues to exist.

On the other hand, neither do those societies which I have called "hot" have this character in the absolute. When, on the morrow of the Neolithic revolution, the great city-states of the Mediterranean Basin and of the Far East imposed slavery, they constructed a type of society in which the differential statuses of men—some dominant, others dominated—could be used to produce culture at a rate until then inconceivable and unthought of. By the same logic, the mechanistic revolution of the 19th century represents less an evolution oriented in the same direction, than a rough sketch of a different solution: though for a long time it remained based on the same abuses and injustices, yet it made possible the transfer to culture of that dynamic function which the protohistoric revolution had assigned to society.

If—Heaven forbid!—it were expected of the anthropologist that he predict the future of humanity, no doubt he would not conceive of it as a continuation or a projection of present forms, but rather on the model of an integration, progressively unifying the appropriate characteristics of the "cold" societies and the "hot" ones. His thought would retie the thread with the old Cartesian dream of putting machines, like automats, in the service of man; it would follow a trail through the social philosophy of the 18th century and up to Saint-Simon. The latter, in announcing the passage "from the government of men to the administration of things," anticipated at the same time the anthropological distinction between culture and society, and that conversion of which the advances of information theory and electronics gives us at least a glimpse: the conversion of a type of civilization which inaugurated historical development at the price of the transformation of men into machines into an ideal civilization which could succeed in turning machines into men. Then, culture having entirely taken on the burden of manufacturing progress, society would be freed from the millenial curse which has compelled it to enslave man in order that there be progress. Henceforth, history would make itself by itself, and society, placed outside and above history, would be able to assume once again that regular and, as it were, crystalline structure which the best-preerved of primitive societies teach us is not contradictory to humanity. In this perspective, utopian as it is, social anthropology would find its highest justification, since the forms of life and thought which it studies would no longer have a purely historical or comparative interest: they would correspond to a permanent hope for mankind, which social anthropo-

logy, particularly at the darkest hours, would have a mission to keep watch over.

Our science would not have been able to mount this watchful guard—and would not even have conceived of the importance and the necessity of it—if, in the remote regions of the earth, men had not obstinately resisted history, and if they had not remained as living examples of that which we want to preserve.

In conclusion, I would very much like to evoke in a few words the very exceptional emotion which the anthropologist feels when he enters a house in which tradition, uninterrupted for 4 centuries, goes back to the reign of Francis I. Especially if he is an Americanist, many bonds attach him to that era, in which Europe received the revelation of the New World and was opened to ethnographic knowledge. He would have wanted to live then—indeed, he lives there every day in thought. And because, remarkably, the Indians of Brazil (where I went through my 1st field campaign) could have adopted as a motto, "I will stay put," it happens that the study of them takes on a double quality: that of a trip to a distant land, and—even more mysterious—that of an exploration of the past.

But for this reason also—and remembering that the mission of the College of France has always been to teach science in the making—I am touched by the hint of a regret: why was this chair created so late? How does it happen that ethnography did not receive its place when it was still young, and when the facts still retained their richness and freshness? For it is in 1558 that one likes to imagine this chair established, when Jean de Léry, returning from Brazil, drafted his 1st work, and when André Thevet's The Singularities of French Antartica appeared.

Certainly social anthropology would be more respectable and more self-assured if official recognition had come at the moment when it was beginning to outline its projects. However, supposed that all had happened thus, anthropology would not be what it is today: a restless and fervent study which plagues the investigator with moral as well as scientific questions. It was perhaps in the nature of our science that it appeared as an effort to make up for lost time and at the same time as a reflection on a backlog to which certain of its fundamental traits should be attributed.

If society is in anthropology, anthropology is itself in society: it has been able to enlarge progressively the object of its study to the point of including therein the totality of human societies; it has, however, appeared at a late period in their history and in a small sector of the inhabited world. More than that, the circumstances of its appearance are comprehensible only in the context of a particular social and economic development: one suspects then that they are accompanied by a seizure of conscience—almost of remorse—that humanity could have remained alienated from itself for such a long time, and above all, that that fraction of humanity which produced anthropology should be the same fraction of humanity which has made so many other men the objects of execution and contempt. "Sequels to colonialism," it is sometimes
said of our investigations. The 2 are certainly linked, but nothing would be more false than to hold anthropo-
logy to be a throwback to the colonial frame of
mind, a shameful ideology which would offer colonialism a chance of survival.

What we call the Renaissance was a veritable birth
for colonialism and for anthropology. Between the 2,
confronting each other from the time of their common
origin, an equivocal dialogue has been maintained for 4
centuries. If colonialism had not existed, the rise of
anthropology would have been less belated; but per-
haps also anthropology would not have been led to
implicate all mankind in each of its particular ex-
amples. Our science arrived at maturity the day that
Western man began to see that he would never un-
derstand himself as long as there was a single race or
people on the surface of the earth that he treated as
an object. Only then could anthropology affirm itself
as what it is: an enterprise renewing and atoning for
the Renaissance, in order to spread humanism to all
humanity.

Having rendered homage to the masters of social
anthropology at the beginning of this lecture, let me
reserve my last words for those savages whose obscure
tenacity still offers us a means of assigning to human
facts their true dimensions. Men and women who, as
I speak, thousands of miles from here on some savan-
nah ravaged by brush fire, or in some forest dripping
with rain, are returning to camp to share a meager
pittance and to invoke their gods together; those
Indians of the tropics and their counterparts through-
out the world who have taught me their poor knowl-
edge (in which resides, nevertheless, the essence of
the knowledge which you have charged me to transmit to
others); soon, alas, they are all destined for extinction
under the impact of illnesses and—for them even more
horrible—modes of life which we have brought them.
To them I have incurred a debt which I can never
repay, even if in the place in which you have put me
I were able to give proof of the tenderness which they
inspire in me and the gratitude which I feel toward
them by continuing to be as I was among them, and
as, among you, I would hope never to cease being:
their pupil and their witness.

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