comments and reflections

an interview with Claude Lévi-Strauss, 30 June 1982

BERNADETTE BUCHER—*Fordham University*

Bernadette Bucher: Originally, my intention was to ask you about your work as you may see it now, with some distance, to judge it, so to speak, both retrospectively and in terms of its prospects and heritage. But I know—you told me so again a few days ago—it does not interest you, it never did, to go back to what you already wrote. However, there is in you a sort of paradox from this point of view and this is where, if you agree, I would like to start. There is on the one hand a will to always go forward, never to turn back; but on the other, each one of your books tries to show a continuity with what precedes, between *The Savage Mind* and *Totemism Today,* or between the different volumes of the *Mythologiques* for instance, one senses a deep need for continuing with a past which never seems to disappear, is never abolished, a will to build up a unified work, as an architect, careful that each piece he separately built at different periods of time become only one whole complex. This entails a constant presence of the past, an intense attention to it. It has a whole Proustian aspect . . .[1]

Claude Lévi-Strauss: That is what I was going to say. It is a *Recherche Du Temps Perdu,* but it is because time is lost. This return to the past is more obvious in *Naked Man,* by the way. I think there is a character trait here and an effort to rationalize it. The fact is that I do obliterate my past. The older members of my family are always amazed that I should not remember anything of my childhood or adolescence. Likewise, when I was a student I was actively involved in a political movement called "constructive revolution," within the socialist party, then named SFIO. It just happened that young Ph.D. candidates got interested in it for their dissertations. So they came to see me, as I am one of the rare survivors of this movement. I do my best to answer their questions. But they constantly tell me, "No, this is not possible. It is not right. Here are the dates. This is the way things happened." So then: a certain amount of personal deficiency. But, moreover, I have an acute feeling that the books I write, the lectures I give are events passing through me and, once out, they leave me stranded, if I may say so. I am only the passageway, the transit-road of a certain number of things which, I admit, have been elaborated within me, but once gone, leave me empty.

BB: Maybe, then, we are touching a break between the unconscious, the affective part and the intellectual aspect in you, because what is striking in your work is this link with the past.

CLS: Probably because the past escapes me, hence this obsession to recuperate it.

BB: Since we are on this problem of forgetfulness, obliteration of the self, when in your work, on a different level, from an epistemological point of view, you evoke the *effacement du sujet,* the "illusions of subjectivity," is it that . . .

0094-0496/85/020360-09$1.40/1

CLS: Because this is the way I experience myself. I have absolutely no feeling of my personal identity.

BB: This is strange. It might explain your relationship with psychoanalysis. I recently became interested in the relationship one may see—or not see—between the unconscious as you describe it through myths, and the individual, the Freudian unconscious. I know that you discussed that problem several times. But many points still remain unanswered. For instance, are the laws which, according to Freud, govern the individual unconscious completely heterogeneous, incompatible with those you discover in the myths of certain societies or in other cultural phenomena? This is certainly a big problem.

CLS: Indeed. First of all, I certainly do not eliminate Freud. The discovery of Freud's work has been essential to me from the time I was in my teens. I happened to have a schoolfriend whose father was a psychiatrist and very close to Marie Bonaparte. So, this small group introduced me to Freud in its own indirect way, rather earlier than most people at the time. Freud always appeared essential to me in that he discovered (this is what will survive for a long time, if not forever) that thinking processes which may appear totally irrational hide a form of rationality and are understandable. This is a fundamental rule in the human sciences. We are confronted with a wealth of thinking modes. Our role is to discover what may lie behind as rationality. This seems to me a definite asset. Now, my reservations toward psychoanalysis are twofold: first of all, I am not at all convinced that this discovery, pertaining to Freud's theoretical contribution, can really have any practical application. In other words, I am not convinced psychoanalysis ever cured anyone.

BB: Did you ever try to be analyzed?

CLS: Never. Never, but in the society we live in, we are constantly surrounded, girded with people who have been or are being analyzed and that's a field of observation. This is a first point. So, in Freud's work, in what remains of it, I would clearly distinguish the theoretical aspect from the applications. I am skeptical that there are any. On the other hand, from a methodological point of view, I have felt the need to react strongly against a rather common tendency in the human sciences whereby whenever a discipline stumbles on a problem, it turns to the next to fill in the gaps. Anthropologists do it each time they turn to psychoanalysis and vice versa. This holds true also for other disciplines. So, I am willing to admit a third response to what you said. I am ready to concede that what psychoanalysts and anthropologists do is, to a certain extent, complementary; that psychoanalysts work on individual psychology and anthropologists on collective phenomena; that in certain respects, they work in a parallel manner; but it would be disastrous for both of them to try to fill the gaps in each discipline by borrowing from each other and that their relations can be all the more fruitful if they always remain clearly separated.

BB: There is still something left unanswered in my question, if I may . . .

CLS: Surely.

BB: Let's take, for instance, the modes of thinking expressed in myth. Of course, myths are collective. They belong to specific cultural groups, to a society; but these groups or societies are made of individuals. In your opinion, can the mechanisms, for instance, the systems of transformations whose laws you define for myths, be entirely different from those which operate otherwise, within the individuals for whom these myths are meaningful?

CLS: Take dreams, for example. When I read *The Interpretation of Dreams* for the first time, it was a revelation to me for the reasons mentioned before. What strikes me now is that Freud examined only a very special case of dreams, not the general case. What is most general in

dreams is that the mind tries as well as it can to make a more or less well organized whole out of a mass of stimulations which invade the unconscious during sleep. These stimulations may be of the kind Freud exclusively recognizes, that is, unconscious desires. But I think there may also be many of a totally different sort. They may come from the vague perception of physiological disorders or more simply from the external world. There may be many other sources. At any rate, the problem is for the mind to make a coherent whole out of it. So, to go back to your question, I would say yes, I think that in this case the mind works in the same way as—I do not like the expression "collective thinking," I will tell you why in a minute—let's say in relation to the mass of stimulations it receives from outside, from society: all the questions raised by the universe, by cosmology, economics, social rules, and so on.

I don't like the term "collective thinking" because there is no doubt that originally myths were created by individuals. One must suppose that as far back as we can go, there must have been one individual who told a story for the first time. But this story is not yet a myth. It will become so only when it has been accepted by a certain collective group, indeed made of individual minds, but they will converge to repeat and eliminate certain aspects of the story only by obeying the same constraints. One of them stems from the fact that the brain structure and functioning are fundamentally the same among all men. That's a common denominator. On the other hand, men live in a particular society, have certain kinds of relationships with the natural world, and all these constraints will transform a myth from what originally was a dream, or something close to a dream, into a tool subservient to social life.

BB: Is this not where anthropology relates to biology? At some point is the anthropologist not forced to yield to the biologist?

CLS: This, I would say, is wishful thinking. At the moment, there is no way it can be so. There is a chasm between the two fields. We are too far off the mark and the biologists and neurophysiologists will tell you they have not advanced far enough to claim having a theory of intellectual activity. All we can say is that the great advances made in neurophysiology of the brain can let us hope that in the long run, in decades or centuries, a convergence will occur.

BB: Let's leave biology and psychoanalysis and turn toward history. Here again I know you have written a lot on the relationship between anthropology, especially structural anthropology, and history. All I want to do is to try filling certain gaps.
CLS: Fine.

BB: Right now we see anthropological research proliferate in areas which in the past were mostly the sociologist's territory: rural European societies, urban anthropology in regions which possess important historical archives, often going very far back in time. In this particular case, how do you envisage the relation between fieldwork and historical data? In other words, is it possible in your view to apply a form of structural anthropology to this type of fieldwork situation where important historical documentation exists?

CLS: It seems to me that one of the most important developments which occurred in the last few years is the rapprochement between anthropologists and historians. For a long time, we were living in a situation where historians took care of important things and anthropologists took care of trivia, too insignificant to be of interest to historians. I will always remember how during a discussion at the great meeting "Anthropology Today," held in New York in 1952, I said that anthropologists were like the "ragpickers of history" and were collecting what historians would put in their garbage can, in their dumps. This totally poisoned the rest of the discussion. Later on Margaret Mead told me: "You know, there are words that are too emotionally loaded to be ever pronounced: 'garbage' for one." She was quite right. But I think the idea itself

was correct. In the last 20 or 30 years historians have learned that all these small details they ignored, scorned, totally overlooked, were very important and they now include them in their own historical research. This is an important step. The other aspect, which one may consider inversely symmetrical, is that if historians are willing to become, so to speak, the ethnographers of past societies, anthropologists themselves can profit considerably from it. Before, what they examined, the data they could gather, dealt with societies coexisting in space. Now, thanks to the historians' new perspectives, they can add to it another dimension, that is to say, different moments of societies coming in a temporal sequence. This increases our research possibilities in almost geometrical proportion. As a result, we realize that between so-called "primitive societies" and the former times of large-scale societies, there are many more similarities than one may have supposed and for that reason research in each field can fertilize the other. For instance, in the last few years in the course of my work on cognatic societies, I was constantly aware that many gaps and puzzles in ethnographic documents can be bridged and solved, if not easily, at least to a certain extent, by looking into what was going on in Medieval European, Eastern, or Far Eastern societies. That is a great rapprochement and a preamble to your main question: What can the anthropologist, especially a structural anthropologist, do in contemporary societies? First of all, he or she cannot afford to bracket away history. We should never forget that when we study so-called primitive societies in the present, it is not because there is an inherent virtue to synchrony; it is simply because the diachronic dimension is missing, at least for the most part. Each time we can avail ourselves of this diachronic dimension, each time we have a chance to use history, we must look at it in the first place. There is, in my view, nothing more pernicious than the attitude that prevailed (not any more), with Anglo-Saxon functionalism, according to which one had to tackle the study of a society, a village, or a group in a state of intellectual virginity and was not supposed to introduce any information that could have altered the observer's candor and spontaneity. Start with history, this is the first point. The second is to keep constantly in mind that there is no single fundamental structure in a society, that everything in it is not structural. There are structural or structurable islands and these islands or islets bathe in an ocean of random phenomena not amenable to structural analysis. They belong to the historian's domain, because his role is to tell us how things happened. However, there is in my view no ground to say that because things happened in a certain way, they had to happen this way by some internal, hidden necessity. The historian's domain is essentially contingent. But in this ocean of contingency, of dispersed structures, as Prigogine would say, "there are islands which spontaneously structure themselves." The role of the structural anthropologist is to spot them and study them, but in no way can he claim to eliminate the historian's specific role. He must on the contrary be constantly aware of the limits and necessary modesty of his procedure in relation to the ocean of uncertainty, where only historical method can operate.

BB: In what you just said, it seems to me you implicitly accept the idea of some human freedom. If there is no internal necessity that things happened the way they did, by means of some determinism, does it not imply that there are areas . . .

CLS: Listen, to talk about unpredictability or uncertainty does not mean freedom at all. When I say, "In any historical process we cannot foresee how things will happen," it does not mean at all that things happen this way by a conscious human decision. Take an example I often give about the origin of agriculture in the New World. We know that one of the oldest cultivated plants to be found there is a species of squash called *Lagenaria*. They are not American but come from Africa. It means the beginnings of agriculture, or if not the beginnings (I oversimplify), at least some of the earliest forms of agriculture in the New World, which to a certain extent shaped things to come, depend upon the fact that a certain plant went with its seeds from

Africa to America. How did it get there? We have no idea. Maybe pirogues carrying provisions from Africa got lost on the American shores or, more simply, squash floated across the Atlantic, drifting on the current. We do not know. At any rate, it is a random phenomenon. It happened; may not have happened. Men spotted these plants and cultivated them. They could have ignored them.

BB: I still keep in mind the criticism so often reiterated against structuralism, at least your form of structuralism, namely, that it reduces man and eventually negates him. In what you just said about history, I sense a sort of ambiguity. One could see, as I did first, and you contradicted me, a certain denial, but . . .

CLS: The critique is absurd. But I do not see any valorization of man in saying: in human societies, things happen, like, let's say, Brownian movements, that is to say particles (represented by individuals or societies), constantly move in various directions which nothing in their previous positions can allow us to predict.

BB: So, does the idea of a "human freedom" in the sense used by Sartre, for instance, or even in its current meaning, seem to you, in this case too, an illusion, of the same type as the illusions of "subjectivity"? Or, on the contrary, even if there are forms of cultural determinism . . .

CLS: I would say: neither one nor the other. It is a concept which is operational at a certain level of observation and which ceases to be so at another level. I would repeat a comparison I often used. If you look at a drop of water in a microscope with different levels of magnification, at the lowest level you will see a multitude of little creatures busy eating each other, making love or what not. At the next level, you will no longer see them but the cells their bodies are made of. One step further and you will see molecules and if you still increase the magnification, you will eventually see atoms. At a certain level of observation, where you see individuals, organisms that have various relations toward each other, I am willing to accept that the notion of freedom may be operational, for, if we govern these little creatures, we would have the highest interest in persuading them they are responsible for their actions and therefore free. It will be the best way to control their activities. But it loses any sort of meaning when we are no longer dealing with individuals but only with the cells their bodies are made of or with the atoms inside the cells' molecules. In brief, there is no privileged level. The fact that this type of discussion should ever take place within the human sciences demonstrates, in my view, that they are not really sciences. It is inconceivable in what is called in English "hard sciences." A molecular biologist will never question that what a zoologist does is interesting and vice versa.

BB: Then, let us remain within the classification of sciences to which you devoted several articles. This is a problem which becomes more and more important, especially in the States where multi- and interdisciplinary programs proliferate (I teach in one of them) and reshufflings of departmental organization are more frequent. At any rate, there is a renewed interest in this issue. Can you, at the moment, in spite of what you just said, see a central role for anthropology?

CLS: If I were to say a central role, I would contradict myself. Let's put it this way: frames of reference become more profitable than others at certain periods. They change with time. For instance, there is no doubt that in the past few years, molecular biology has been a very profitable frame of reference in life sciences. In the past half century, anthropology has been, I think, a particularly profitable one, but it does not mean it will remain that way forever.

BB: In *Structural Anthropology* you said that in order to establish a consistent classification of sciences, one should first make an epistemological critique of each science. To begin with anthropology, more specifically the history of anthropology, how do you envisage a critical history of anthropology that would not fall into the traps of evolutionism, for instance?

CLS: I have not thought of it. You catch me unaware.

BB: I was mainly thinking of your paper on Rousseau, which is splendid, but insofar as you present him as the founder of the Human Sciences and the 18th century as a period of fundamental change, one could somehow interpret this as an evolutionist view of the history of anthropology. It is paradoxical. This is what Marvin Harris claims, among others. Rereading Montaigne, particularly the "Apology for Raymond de Sebonde," which in my view is crucial in the history of anthropological thought, it seems to me that this fundamental break had already occurred in the 16th century, with Montaigne and certainly others, like Bodin for instance . . .

CLS: Or Rabelais.

BB: Yes. I wonder why this aspect of Montaigne has been overlooked for so long. Everyone has read Montaigne. Rousseau read him; everyone in the 18th century read him and plundered him. In spite of this, historians of anthropology continue to place in the 18th century a break which, in my view, was already accomplished in the 16th century by certain individuals like Montaigne and maybe others.

CLS: Or Rabelais, too.

BB: Yes, Rabelais. So I am trying to understand the historians' mistake, and I was wondering whether you would have an opinion on this.

CLS: I don't. I don't believe I have an evolutionist attitude on this point. I rather see abrupt scintillations which occurred at a certain time and whose glow faded away until another scintillation appeared. You are quite right to say that with Montaigne, and more generally the 16th century (it is not very difficult to find out why: it is because of the discovery of America), there has been a sort of explosion, the first firework of ethnological thought. Then it dwindled and, in the 18th, another one took place. I don't want to recant what I said. After all, this text on Rousseau is a speech I delivered in Geneva for the 250th, if I am not mistaken, anniversary of his birth. So what I had to show, was Rousseau's importance. I did not have to show Montaigne's importance. If I had talked about Montaigne, I would have shown Montaigne's role. What remains to be said, is that anthropology has two aspects: fieldwork data (you know it better than anyone else as a specialist in Jean de Lery and others in the 16th century) and critical reflection. This is quite essential in Montaigne's and Rabelais's works. In the 18th century, these two aspects are always present. Voyages become more frequent, and with them, ethnographic data collecting, while moral and critical reflection come to the forefront with Rousseau, Diderot, and others. But there is something more in Montesquieu's and Rousseau's works, namely, hypotheses which I would call scientific or prescientific and which, it seems to me, are more directly at the base of our present thinking. Take an example. Throughout the 19th century, the invention of agriculture and animal husbandry was believed to have created a food surplus which in turn allowed population increase. Nowadays, all good specialists take an opposite stand. It is population increase which, they say, where occurring, made it impossible to survive on hunting and gathering and thus imposed agriculture and animal husbandry. This has been shown in a very precise way. In New Guinea, for instance, one can establish correlations between different types of agriculture (from the most extensive to the most intensive) and demographic density. It has also been shown in other parts of the world. The authors who do this in a very rigorous and empirical way do not realize that it is one of Rousseau's intuitions to have said in the clearest way that population increase imposed the invention of agriculture. Thus, with Rousseau we have a number of quite dazzling intuitions (I do not say that it had any scientific or objective foundation) with which we agree.

BB: May I ask you now what you are interested in at the present?

CLS: I don't know. Stage-setting. Once I had a chance to make stage sets, and I never had such fun in my life as when I was working on the stage, not only with carpenters and painters but technicians as well, setting the lightings and that sort of thing. I have a kind of repressed vocation for manual work and if I could . . . Anyway, I am too old now and there is no chance anything of the kind will come up, so I will remain an anthropologist to the end of my life.

BB: Apart from this, you briefly spoke to me about your new interest in Japan. What do you find or are you looking for there?

CLS: I find a society as different from ours as those anthropologists study in South America or Melanesia, but at the same time it provides us with all the means of investigation and reflection we may have in our own and even more so, since 11th- and 12th-century Japanese literature offers a wealth of precise information. One has to wait until Saint-Simon's *Memoires* to find anything comparable at home.

BB: How does your interest materialize? through trips? readings? studies?

CLS: I went twice to Japan, once to Korea. I am going back to Japan next year. So, first through travels. I always set as a condition for going there, in exchange for whatever I am requested to do (usually give one or several lectures), to have the possibility of traveling inland and staying in villages. So I am able to see more closely how people live, at least as far as not knowing the language does not completely block any ethnological perspective. Although I learned some Japanese a few years ago, without any success . . .

BB: Did you start again?

CLS: Yes, I did.

BB: Can you speak it at all?

CLS: I can: the length and breadth of my knowledge is about ten sentences. Judge for yourself. I can read the syllabaria, but since I don't understand what I am reading, I am no better off for it. As for ideograms, I learn them very assiduously, but since I am too old, I forget them as soon as I learn them. No, I am starting too late, and as, fortunately, the ancient texts of Japanese literature become more and more accessible in excellent English or French translations, there is an incredible mass of ethnographic and sociological documentation. Japanese scientists have already mined it a lot, but it may not have been sufficiently looked at in a comparative perspective in the light of the last 20 or 30 years' advances made in anthropology. I get a lot from it.

BB: These last few years you have devoted your course at the Collège de France to complex kinship structures. This is no doubt a return to your first research work. I heard (since I am not in France during the year) that you focused on the notion of "house" *(maison).* As the course has not yet been published,[2] may I ask you what you mean by that term?

CLS: I would say that it is a form of institution that had no place in anthropological taxonomy. We are used to reducing everything to preexisting categories. Thus, we know of clans, *Gens,* lineages, families, and so on. But I have been struck by the converging testimonies of eminent anthropologists writing about America as well as Africa, stating that they can't find any term in ethnographic literature for describing what they observe. Boas said it long ago about the Kwakiutl and Evans-Pritchard in almost the same terms about the Nuer, and so on. So I asked myself: what are these institutional forms that cannot fit in any traditional ethnological categories? So here we are back to the rapprochement between anthropology and history. What strikes me is that this unknown which ethnologists stumbled upon—historians have known well for years and have a noun for it—the "house," in the sense of a noble house. There is no need to go as

far as ancient history. Reflections on noble houses were going on among those concerned way up into the mid-19th century. If you look into the available memoirs by nobles, you will find this notion, once needed to fill in a gap in ethnographic literature (the famous Kwakiutl *Numay Ma,* for instance, which Boas could not fit in the traditional typology), is precisely the "house" as it was understood in Medieval Europe and up to the 19th century.

BB: But what made you come back to kinship and social organization and complete a full circle, as it were, from elementary to more complex kinship systems? Are you resuming your initial project stated in the *Elementary Structures of Kinship* where you say that, in order to really have a genuine kinship theory, one should also discover and include complex structures?

CLS: Yes.

BB: Is it what you are aiming at, in going back to it? Are you hoping . . .

CLS: No, I don't, for the good reason that already in the *Elementary Structures* and in later works, such as the preface to the second edition, I was fully aware of the difficulty involved and of the methods to be used to complete the task. I could foresee that one could not manage it without the help of computer science and this is a program which is now admirably well handled by Francoise Heritier.[3] She has precisely the competence in this field I completely lack and have no better chance to acquire at my age than Japanese. So, this is a field I have completely handed over to my successors, who alone are well equipped to tackle the problem. What took me back to it is much simpler and down to earth. When someone spends 22 years at the Collège de France as I did, with the marvelous freedom which reigns there, but the terrible servitude of having to teach a course on a completely new topic every year, I would say that for the first 10 or 12 years, it is no problem. One is full of new topics and ideas. But towards the end, all this is exhausted and one is forced then to broach subjects one has no idea or notion about. As I had completely left aside the semi-complex (I would not say complex) kinship structures and an enormous literature had piled up on the subject (while I was writing *The Structures,* no one realized the considerable proportion of cognatic societies in the world. Everybody was a bit obsessed with unilineal systems), so I told myself: there is all this I don't know and I should get to know it. I made up these last courses not at all to bring something to my readers, but to fill in a gap in my knowledge.

BB: Could we go back now to the time when you were in New York? You were at the New School for Social Research during and after the war, wasn't it?

CLS: No. I had been at the New School since I arrived in New York, that is in 1941 until I was called back to France by the government, right after Paris' liberation, that is to say during the '44–'45 winter. Actually I left the New School at the end of '44 and arrived in Paris early '45. I went back to New York as cultural counselor at the French Embassy and had therefore no more link with the New School.

BB: Was it at that time you were introduced to and became interested in communication theory?

CLS: Well, I got interested in it when Shannon's[4] and Wiener's[5] work appeared. The curious thing is that, during several years, between '41 and '45, Claude Shannon and I lived in the same house at 51 West 11th Street. It was a small brownstone belonging to an old Italian who rented studios. Shannon was in one and I in another.

BB: Without ever meeting?

CLS: Without ever meeting, except that we had a common friend in the building and I remember she told me once: "I know someone in our house who works on artificial brains." It seemed to me quite strange, but I did not take note of it.

BB: What about your relationship with Jakobson? You speak so much of the importance of structural linguistics for your work. Was it really a revelation for you to read and meet Jakobson and is it afterwards, once you had read this work or even had conversations with him, that you sought to draw its consequences for anthropology or were you already . . .

CLS: Absolutely not. Let's put it this way: when I arrived in New York, I was already a structuralist, but like Monsieur Jourdain, without knowing it. I had never heard about structuralism, but I was at bottom a structuralist. Meeting Jakobson has been a revelation of what was in my mind in an inarticulate form, and already existed in a discipline like linguistics. What baffled me is the fact that during all these years at the Sorbonne, no one, at least among my professors, had ever said: you must read Saussure.

BB: A last and more general question: I wonder if, retrospectively, under the impact of critiques launched at you, or merely because of an internal evolution or of newly made discoveries, there are elements in your work you would like to change or, on the contrary, if it would prompt you to reinforce your positions?

CLS: I think that of my theoretical positions as they have been formulated in my first articles, in my first books, I have absolutely nothing to change. However, many things I wrote in the beginning, I would not write now, or at least, not that way. I realize many were unwise and not sufficiently grounded in facts, certain parallels between linguistics and anthropology, for instance. Let's say that I went a bit too far or too fast. I would now express myself more carefully, but with the same basic convictions.

notes

The interview with Claude Lévi-Strauss was conducted at the Laboratoire d'Anthropologie Sociale of the Collège de France. Dr. Bucher, an Associate Professor in the Humanities Division at Fordham University, has known Professor Lévi-Strauss for 20 years, initially as a graduate student in Paris, and then as a doctoral candidate under his supervision. She has remained as Associate Member of the Laboratoire d'Anthropologie Sociale, which she continues to visit frequently.

[1]The interview was originally tape recorded in French. The present text is my translation—*B.B.*

[2]A summary of the course has just been published in *Paroles Données* (Lévi-Strauss 1984:189–241).

[3]Professor Heritier succeeded Professor Lévi-Strauss as Director of the Laboratoire d'Anthropologie Sociale at the Collège de France.

[4]Shannon, an exponent of the theory of communication, was an early influence on Lévi-Strauss (see Shannon and Weaver 1950).

[5]See Wiener (1948).

references cited

Lévi-Strauss, Claude
1984 Parole Données. Paris: Plon.
Shannon, C., and W. Weaver
1950 The Mathematical Theory of Communication. Urbana: University of Illinois Press.
Wiener, N.
1948 Cybernetics and Communication in the Animal and the Machine. Cambridge, MA: M.I.T. Press.

Received 28 November 1984
Accepted 22 January 1985
Final version received 25 January 1985