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ABOUT PARTICIPATION

Let us begin by a commonsense remark. An implicit philosophy – that is to say not recognised as such – is at work in the anthropological texts on witchcraft. Although I was unable to identify this philosophy, from the first moments in the field I opposed some of its applications. And this I did of course in the name of another philosophy, just as implicit. I would like to justify these two kinds of implicits, after having given a few details about my work.

I was in the field from 1968 to 1972. In 1977, I published “Les mots, la mort, les sorts,” translated into English as “Deadly Words,” its main theme being the gradual introduction of the reader to the world of Bocage witchcraft. In 1981, I wanted to show more directly the phases of my own entry into the world of spells. In “Corps pour corps,” Josée Contreras and I rewrote the first half of my field diary, from my arrival in the Bocage up to the time I accepted to be unbewitched.

Since then, I have been trying to understand the mechanisms of unbewitching, which the two preceding books had left aside. Josée Contreras (who is a therapist) and I worked for three years on the unbewitching sessions I had recorded in the field. We have published some articles on the therapeutic mechanisms of unbewitching. For my part, I have analyzed the profound changes which seem to have affected Bocage witchcraft over the last century. Lastly, “Unbewitching as therapy” is an attempt at a sociological explanation: unbewitching does cure, but who, how and why?

When I was in the field, I had no choice but to accept being affected by witchcraft. At the same time, I set up a methodological device which would later enable me to come to a certain understanding of witchcraft. I am going to show how my activity consisted neither in participant observation nor in empathy.

When I left for the Bocage in 1978, an abundant anthropological literature on witchcraft was available. It comprised two heterogeneous bodies of work which ignored each other’s existence: on the one hand were the European folklorists (who had recently assumed the flattering title of “ethnographers,” although their methods remained unchanged), and on the other stood the Anglo-Saxon anthropologists, mostly Africanists and functionalists.

The European folklorists had no direct knowledge of rural witchcraft. Following the directives of Van Gennep, they carried out regional surveys, contacting the local elites (the least likely to know anything about it) or sending them questionnaires, as well as inquiring of a few peasants whether they “still believed in it.” The answers they received were as uniform as the questions:

“We don’t here, but they do in the neighboring village; they’re backward, there.” This was usually followed by a few skeptical anecdotes that made fun of the believers. Let us say that where witchcraft is concerned, the French ethnographers bothered neither to observe nor to participate (and this is still true in 1987).

The Anglo-Saxon anthropologists at least claimed to practice “participant observation.” It took me some time to derive from their works on witchcraft the empirical content that could be attributed to this strange utterance. In rhetoric, it is called an oxymoron: to observe while participating, or to participate while observing, is about as obvious as savoring a burning hot ice cream. In the field, my colleagues seemed to combine two types of behavior: one, of an active kind, meant regular work with paid informants whom they questioned and observed; the other, of a passive kind, involved attendance at events related to witchcraft (quarrels, consultations with soothsayers, etc.). The first type of behavior cannot possibly be described as “participation” (the informant, on the other hand, does seem to “participate” in the work of the ethnographer); as for the second type of behavior, “participating” simply means trying to be on the spot, such “participation” being the minimal requirement to make observation possible.

What counted, therefore, for these anthropologists, was not participation, but observation. Their conception of it was, moreover, a rather narrow one: their analysis of witchcraft was limited to that of accusations because, they claimed, these were the only “facts” that an ethnographer could “observe.” Accusing is for them a “behavior,” and even the typical witchcraft behavior, since it is the only one that is empirically verifiable. Everything else is little more than native imagination and errors. (We may point out that, for these authors, talking is neither behavior nor an act subject to observation.) These anthropologists gave precise answers to a specific question – who accuses whom of having bewitched him in such and such a society? –but they remained silent on all the others: how do you get caught up in a cycle of witchcraft, and how do you get out of it? What are the ideas, the experiences and the practices of the bewitched and their magicians? Even an author as meticulous as Turner does not help much, and to get any insight, one must go back to the writings of Evans-Pritchard (1937).

Generally speaking, there was, in this literature, a constant shift of meaning between certain notions which should have been kept distinctly apart: “truth” overlapped with “reality,” which in turn overlapped with “observable.” With “observable” there was further confusion between the observable as empirically verifiable knowledge, and the observable as knowledge independent of the natives’ statements. Then, the “observable” overlapped with “fact,” “act” and “behavior.” The only common characteristic of this nebula of meanings was that each of the terms it contained had a symmetrical opposite: “error” overlapped with “imaginary,” which in turn overlapped with “unobservable,” “belief,” and lastly native “discourse.”

In all this, however, the status of native discourse remains totally unclear: sometimes it is classified as behavior (as in accusing), and at other times as false propositions (for example invoking witchcraft to explain a sickness). Speech activity – enunciation – is discounted and nothing remains of native discourse but its product, that is utterances wrongly treated as propositions: as for symbolic activity, it is reduced to the voicing of false propositions.

We can see that all these confusions revolve around a common point: the discrediting of native discourse in favor of that of the ethnographer, whose activity seems to consist of making a detour via Africa to prove that he alone possesses – what exactly? – a set of polythetic notions amounting to what he considers to be the truth.

To return to my study on witchcraft in the Bocage: when I read the Anglo-Saxon literature to assist me in my fieldwork, I was struck by a curious obsession I noticed in all the introductions. The authors (and the great Evans-Pritchard was no exception) systematically denied the very existence of rural witchcraft in present day Europe. Yet not only was I up to my neck in witchcraft, but its existence was widely testified to in several other regions, at least by European folklorists. Why such an obvious, enormous and widespread empirical mistake? Most likely, it was an absurd attempt to renew the Great Divide between “them” and “us” (“we” too once believed in witches, but that was three hundred years ago, when “we” were “they”). It may also have served to protect the ethnographer (that acultural being whose brain contains only true propositions) from any contamination by the object of his study.

Now, this may have been possible in Africa, but I was in France. The Bocage peasants stubbornly refused to play the game of the Great Divide with me, knowing well where it would lead us: I would have the good position (that of “knowledge,” “science,” “truth,” “reality,” see the list above), and they would have the bad ones. The press, television, the Church, the medical profession, all the national organs of ideological control, placed them beyond the pale whenever a case of witchcraft went wrong: witchcraft was presented as the ultimate feature of the peasant condition, and the peasant condition as the ultimate in backwardness and stupidity. So the Bocage folk, to forbid outside access to an institution that provided them with such valuable services, had erected a solid barrier of silence around it with justifications such as: “In witchcraft, those who aren’t caught up in it can’t talk about it,” or “you can’t talk to them about it.”

So they started talking to me only when they thought I was “caught up in it,” that is, when reactions escaping my voluntary control showed them that I was affected by the real – often devastating – impact of certain words and ritual acts. At this point some thought I was an unbewitcher and spoke up to ask me to officiate, and others thought that I was bewitched and spoke up to help me out. Except for the local bigwigs (who readily talked about witchcraft, but only to discredit it), nobody thought of speaking to me about it because I was an ethnographer.

Whether I was still an ethnographer or not was open to doubt. Of course, I never took it to be a true proposition that a witch could harm me by casting spells or pronouncing incantations. But I also doubt that the peasants themselves took it to be so. Actually, they were demanding that I experience for my own sake – and not for that of science – the actual effects of the peculiar network of human communication, which is what witchcraft is all about. In other words they wanted me to enter the network as a partner and to commit my own existence as it stood at the time. At first, I felt torn between what seemed to be two pitfalls: if I “participated,” my fieldwork would turn into a personal adventure, the opposite of a professional venture; if I tried to “observe,” that is to keep my distance, there would be nothing left for me to “observe.” In the first case my scientific project was threatened; in the second it was completely ruled out.

Although during my fieldwork I never knew what I was doing or why, I am struck today by the clarity of my methodological choices at the time: it all took place as if I had decided to make “participation” a tool for understanding. In my meetings with the bewitched or the unbewitched, I let myself be affected, without trying to inquire or even to understand and remember. Every time I got back home, I would write a sort of chronicle of these enigmatic events. (But sometimes the situations were so overwhelmingly intense that I couldn’t even take notes afterwards.) This field diary, which for a long time was my only material, had two goals.

The first very short-term goal was to try to understand what was wanted of me, to find answers to the then most pressing questions: “What does X take me for?” (a bewitched person, an unbewitcher, or what...) “What does Y want from me?” (to be unbewitched, or what?). It was in my interest to find the right answer, since the next time we met I would be asked to do something. But usually I lacked the means: the ethnographic literature on witchcraft, Anglo-Saxon as well as French, provided no clues as to the rules of the game of positions that constitutes witchcraft. I therefore had to learn them for myself, often at the expense of my own equanimity.

The other goal was long term: although I was undergoing a fascinating personal adventure, I was never resigned to the idea of not understanding, although I did not know very well for whose sake I wanted to understand: for my own, for the sake of anthropology, or for the sake of European conscience. But I organized my fieldnotes so that I could rely on them, later on, when and if it came to understanding. My notes were obsessively precise, so that I was able later to re-hallucinate the events, and because I was then no longer “caught” in them but became “caught” in them again, I did eventually understand.

Those who have read “Corps pour Corps,” the first eighteen months of my field diary, will have noticed that it is in no way similar to those of Malinowski or Metraux. Their field diaries provided them with a private space, where they

could at last let themselves go and come around, after hours of work during which they had been obliged to “face up” to the natives. In other words, a space for personal re-creation, in the literal sense of the term. On the contrary, private or subjective considerations are completely absent from my own diary except in those instances when my personal life had been brought up by my interlocutors, that is to say included in the network of witchcraft communication.

One of the situations I was experiencing in the field practically defied writing down. It was so complex that it defied recall, and in any case it affected me too much. I’m referring to the unbewitching sessions which I attended either as a bewitched person (my personal life was dissected and I was ordered to change it) or as a witness, either for the clients or for the unbewitcher (I was often asked to intervene on the spot). At first, I took a lot of notes on my return home, but it was more to calm my own anxiety about my personal involvement. Once I had accepted to take the position that had been assigned to me in the sessions, I almost never took notes again: everything was moving too fast. I let the situations develop without asking questions, and from the first session to the last I hardly understood what was actually going on. But I discreetly recorded about thirty sessions out of the two hundred or so I attended, so as to provide myself with material on which I would be able to work later on.

In order to avoid misunderstandings, I would like to point out the following: to accept to “participate” and be affected has nothing to do with understanding through empathy, whatever meaning is given to this latter term. I shall consider the two principal meanings of “empathy,” and show that neither of them concerns what I practiced in the field.

According to the first meaning (taken from the “Encyclopedia of Psychology”), to empathize consists in, for a person, “vicariously experiencing the feelings, perceptions and thoughts of another.” By definition, this type of empathy presupposes some distance between the subjects: it is because one is not in the other’s place that he or she tries to represent or imagine what it would be like to be there, what “feelings, perceptions and thoughts” one would then have. But I was precisely in the position of the native, shaken by the “feelings, perceptions and thoughts” of one who occupies a position in the witchcraft system. If I claim that one has to accept to actually take up a position rather than imagine oneself there, it is for the simple reason that what occurs there is literally unimagineable, at least for an ethnographer, used to working on representations: when one is in such a position, one is bombarded by specific intensities (let us call them affects) which generally do not have meaning. Thus, one must experience a position and the intensities that go with it: and it is the only way to gain access to them.

A second definition of empathy – “Einfühlung,” which can be translated by affective communion – insists, on the contrary, on the immediacy of the communication, on the fusion with the other that one is supposed to attain by

identifying with him. This definition says nothing about the mechanism of identification but insists on its result, on the fact that it enables one to know the affects of others.

Now I say that to occupy such a position in the witchcraft system gives me no information as to the other's affects; to occupy such a position affects me, that is to say mobilizes or modifies my own set of images without in any way informing me about those of my partner.

But – and I insist on this point, since this is where the kind of understanding I'm aiming at becomes possible – the very fact that I accept to occupy this position and be affected by it opens up a specific form of communication with the natives: always an involuntary communication, unintentional, and which may or may not be verbal.

When it is verbal, the following can take place: something urges me to talk (let us say, a non-represented affect), but I don't know what, any more than I know why it urges me to say just that. For example, I say to a peasant, echoing something he said to me: "Exactly, I dreamt that..." and I would find it hard to comment this "exactly." Or again, my interlocutor remarks, without establishing a particular link: "The other day, So and So said to you./ Today, you have these spots on your face.." What is being said here, implicitly, is that I'm obviously affected: in the first case, I myself see the evidence; in the second, the other sees it.

When this communication is not verbal, what then is communicated, and how? We are dealing with immediate communication, which is evoked by the term "Einführung." And yet, what is communicated to me is only the intensity that affects the other (in technical terms, one would speak of a quantum of affect, of an energetic charge). The images, which for him and for him alone are associated to this intensity, escape this communication. For my part, I absorb the shock of this energetic charge in my own way: I have, let us say, a temporary distortion of perception, a quasi hallucination or change of dimensions; or again, I am overwhelmed by a sense of panic, or massive anxiety. This is not necessarily (and indeed very rarely) happening to my partner: he can be apparently completely unaffected.

Suppose I don't fight against this state but take it in as a communication of something I don't know. This pushes me to talk, but in the way I evoked before ("Ah, I dreamt that..."), or else to say nothing. At these moments, if I am capable of forgetting that I am in the field, at work, if I am capable of forgetting my set of questions to ask... if I am capable of telling myself that communication (ethnographic or not, this is no longer the problem) is actually operating, at that moment, in this unbearable and incomprehensible way, then I am attuned to a particular variety of human experience – being bewitched, for example – because I am affected by it.

Between people who are equally affected because they occupy one or another

of the witchcraft positions, something can happen which an ethnographer can never witness, things are said that ethnographies never mention, or things are not said, but this is communication too. By experiencing intensities related to a given position one can in fact identify each one as presenting a kind of objectivity: only a certain order of events can occur and one can only be affected in a certain way.

It can be seen that for an ethnographer to accept being affected does not imply that he identifies with the native point of view, or that he takes advantage of the experience of fieldwork to tickle his narcissism. To accept being affected, however, supposes that one takes the risk of seeing one's ethnographic project vanish. For if this project is omnipresent, nothing happens. But if and when something does happen, and the project has not been drowned in the adventure, then an ethnography is possible. An ethnography which, I believe, has four distinctive features.

The starting point is the recognition that ordinary ethnographic communication – a verbal, voluntary and intentional communication, aimed at learning a native system of representations – constitutes one of the poorest varieties of human communication. It is especially unadapted to give the kind of information on the nonverbal and involuntary aspects of human experience.

I would in fact like to note that when ethnographers remember what was unique about their stay in the field, they always evoke those situations in which they were unable to put into practice this poor communication because they were overwhelmed by a situation and/or by their own affects. In ethnographies, these situations of involuntary and unintentional communication, although they are commonplace and recurrent, are never taken into consideration for what they are: the "information" they have brought to the ethnographer appears in the text, but without any reference to the affective intensity which went with it in reality, and this "information" is placed exactly on the same level as any other produced by voluntary and intentional communication. One could in fact say that to be a professional ethnographer is to be capable of automatically masking any episode of one's fieldwork in a voluntary and intentional communication aimed at learning a system of native representations.

For my part I have chosen, on the contrary, to give an epistemological status to these involuntary and unintentional communication situations: it is by examining them over and over again that I am building up my ethnography.

The second distinctive feature of this ethnography supposes that the researcher is willing to experience a kind of split life: according to the moment, either he allows himself to be affected, malleable, or modified by the experience of fieldwork; or he gives precedence to that in him which wants to record and to understand the experience – in other words to turn his fieldwork into a scientific subject.

Thirdly, the process of understanding is spread out in time and disjointed.

When one is most affected, one cannot record the experience; when one is recording it, one cannot understand it. Analysis can only take place later.

And fourthly, the materials collected are especially dense and any analysis will tend to make breaches in scientific certitude. To take but one example, "Unbewitching as Therapy" looks sufficiently like an ethnographic text to be published in the "American Ethnologist." But it radically questions what we thought we knew about witchcraft, rituals, the relationship between the sexes in the French countryside, the access to the status of farmer.

Let us consider, for instance, the unbewitching rituals. If I had not been thus affected, if I had not witnessed so many informal episodes, I would have given central importance to rituals. Firstly, because as an ethnographer I am supposed to give priority to symbol analysis. Secondly, because they play a central role in the typical accounts of witchcraft. But having spent so much time among the bewitched and the unbewitchers before, during, and after the sessions, and heard such a variety of spontaneous discourses, in addition to the conventional ones, and having seen so many actions that cannot be described as rituals, I have learnt the following: ritual, important and spectacular as it is, is only one of the factors enabling the unbewitcher to demonstrate the existence of "abnormal forces," the life and death stakes in the crisis besetting his clients, and the possibility of victory. But victory (one might speak of "symbolic effectiveness") implies the setting up of a very complex therapeutic device well before and long after the performance of the ritual. This device can of course be described only by someone who has been able to come close to it, that is, who has taken the risk of "participating." It can never, at any rate, be merely "observed."

At this point, I would like to make some comments on Renato Rosaldo's article, "Grief and a Headhunter's Rage: On the Cultural Force of Emotions" (1984). As it is well known, Rosaldo wrote this paper some months after having suddenly lost his wife, Michelle Rosaldo, in an accident as they were doing fieldwork among the Ilongot of northeast Manila. In this article, Rosaldo tries to confront his personal experience of grief and bereaving with his past ethnographic activity, which he had had in common with the dead for fourteen years. It happens that the Rosaldos had been precisely working on a bereavement practice peculiar to the Ilongot, headhunting, on which they had already written two interesting books. The Ilongot had always explained to them the practice of severing and tossing away the victim's head as a way of venting and possibly throwing away the anger of bereavement, but the two ethnographers could never understand what exactly the Ilongot meant.

Now, in "Grief and a Headhunter's Rage," Rosaldo reviews his past attempts to understand those comments by Ilongot folk: he just came to experience for himself what anger in bereavement could be like, so he can begin to understand the experiential meaning of Ilongot headhunting. In passing, he discovers some

serious inadequacies in the anthropological descriptions and explanations concerning death and bereavement.

At first, the paper's general assumption seems a mere platitude: Rosaldo reminds us that the ethnographic activity is practiced by human subjects. Yet things become less evident when he gives us a detailed and vivid description of his past ethnographic activity: all of us can recognise in it our own – and incredible – deafness, blindness and stupidity. One could only regret that anthropology needs such a disaster, Michelle Rosaldo's sudden death in the field, for such a description to appear in literature. Other ethnographers tried to do so, and some of them with elegance, but Rosaldo has been caught up in an exceptional situation: that of being affected precisely by what he had been studying for years without having perceived the emotional force behind the phenomenon. And we all have to pay tribute to Rosaldo, as an anthropologist as well as a human being, for having tried to think something about this whole situation. (I point out in passing that in the first part of "Deadly Words" I tried to give an epistemological status to those phenomena of blindness, deafness and stupidity of the ethnographer, as normal consequences of being "caught up" in a process of intercommunication.)

I completely agree with Rosaldo's criticism of the way in which the ethnography of Death and Funerary Rituals ignores the emotional force of the phenomena it is supposed to understand. I also agree with Rosaldo's criticism of the way in which anthropology overestimates the cultural importance of ritual events compared with more ordinary events. By doing so, it is asserting implicitly that everyday life is devoid of cultural meaning, of cultural thickness, of cultural depth. (But there, I think that he lacked analytical clarity.) And I obviously agree with his notion of the "positioned subject": every ethnographer, Rosaldo says, is equipped not only with scientific knowledge. He is also equipped with certain psychic peculiarities and with a certain lived experience, which can either help or impede him or her in grasping certain ethnographic phenomena. I would say that the peculiarity of my fieldwork in the Bocage was that I had to occupy certain positions in the native's system (bewitched, unbewitcher, assistant to an unbewitcher), but that I had to occupy them as a "positioned subject" à la Rosaldo, that is for my own sake.

There is only one point on which I completely disagree with Rosaldo: his concept of the "*cultural force* of emotions" seems to me completely misleading. This expression appears in the subtitle of the paper and in the first sentence. And the first paragraph presents this so-called "cultural force" of emotions as one dimension of cultural facts, or one dimension of symbolism. One dimension among others, one dimension situated on the same level as the others we use to take into account. One dimension among others, which escaped from the attention of symbolic anthropologists. So Rosaldo's paper presupposes that if only the so-called "cultural force" of emotions could be taken into account,

symbolic anthropology would be perfectly satisfying. He only wants to add something to anthropology, without asking himself why this precisely is missing. I quote him: "The vocabulary for symbolic analysis (...) can *expand* by *adding* the term *force* to more familiar concepts such as *thick description*, *multivocality*, *polysemy*, *richness* and *texture*." I would only like to make three assertions:

(1) Emotions obviously convey "force," but this "force" cannot be said to be a "cultural" one. That is, the "force" is in emotions, not in symbols, nor in representations. And the fact that emotions are culturally patterned, or that rituals channel emotions, does not contradict what I say.

(2) If Rosaldo speaks of the necessity of giving attention to everyday life it is because he experienced, on two occasions where he had to conduct a funeral ritual as a chief mourner, that he would have had nothing to say to an ethnographer but platitudes: he expressed and elaborated his grief out of these ritual situations. I would add the following to his argument: in everyday life, the natives have eventually not more to say, but because there is nothing there to which any dense cultural meaning could be attributed (as in ritual), the ethnographer is obliged to recognize that something in this situation *cannot be coped with*. In "Deadly Words" I evoke, for example, cases of what I call "misknowledge," and also cases where expression and symbolism find their *limits*. Rosaldo's argument about emotions could have been much more sharp if he had given a status to this notion of the limits of human expressivity.

(3) By speaking of the "cultural force of emotions," and not only of the "force of emotions," Rosaldo partially ruins the simple but very great discovery he makes in his paper. The notion of a "cultural force of emotions" – exactly like those of a "symbolic effectiveness" or of "participant-observation" – the notion of a "cultural force of emotions" is a kind of oxymoron aimed at magically solving the problem it raises, which is indeed a major problem of anthropology: that of understanding from where comes the "force" in social life. Probably not from symbolism, but from that which limits the human capacity of symbolization.

To conclude on my fieldwork, I would like to make two points, the first on methodology, and the second on the philosophy of the anthropological enquiry.

1. Since we cannot approach ethnographic "facts" except through and in a communication relationship, these "facts" are therefore the products of this relationship between the ethnographer and his natives. Thus, to describe an ethnographic "fact" is also to describe the situation of intercommunication, by taking into account all the partners involved, including the ethnographer. (I am not speaking here of interlocution because, in the field, one does not communicate only words but also, and among other things, affects devoid of representation.)

2. Jorion (1985) shows that Anglo-Saxon anthropology presupposes that the

human subject is essentially transparent to himself. This is probably more true of British than of American anthropology, but I think that even the thick descriptions, the forests of symbols, the networks of cultural signifiers, presupposes a human subject of this kind. I would only add that my experience of fieldwork – because it gave room for non-verbal, unintentional and involuntary communication, to the eruption and free play of affects devoid of representations – my experience of fieldwork forced me to explore the thousand-and-one aspects of an essential opaqueness of the subject to himself. This notion is in fact as old as tragedy itself, and it has underlain for a century the whole of therapeutic literature. No matter what name is given to this opacity (the “unconscious,” or whatever), the important point, especially for an anthropology of therapies, from now on is to postulate it and to place it at the centre of our analyses.

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