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**OLD ETHICS DIE HARD
THE YANOMAMI AND SCIENTIFIC WRITING
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When, in 1976, *Time Magazine* published an article titled *Beastly or Manly?* in which Yanomami culture was described as "horrifying" and, like "baboon troops," made "some sense in terms of animal behavior," an era of Yanomami character assassination began. *Time Magazine* acknowledged Napoleon Chagnon as its only source, as in this passage: "Chagnon argues that Yanomamö structures closely parallel those of many primates in breeding patterns, competition for females and recognition of relatives." Since Chagnon never denounced any misuse of his writings by the magazine, one assumes he approved of the way the Yanomami were being portrayed. Two letters to the Editors, one by anthropologist Judith Shapiro, the other by Paul Shadle, a missionary, refuted the article and objected to the sensationalist abuse of Yanomami imagery. While various academic reviews criticized Chagnon's *Yanomamö: The Fierce People* for its methodological and empirical flaws, those two letters were the only public manifestations against that insulting piece of journalism.

Twelve years later, a new flood of denigrating publicity befell the Yanomami. The trigger was an article by Chagnon published in *Science* in which he asserted that 44% of adult Yanomami males were killers and, as killers, they attracted more women, hence, produced more children than non-killers.

The statistical fallacy of Chagnon's argument was amply demonstrated in Bruce Albert (1990). Part of the problem with Chagnon's analysis is a basic empirical blunder. Just as the Yanomami term *waitheri* is simplistically glossed as "fierce" when it has a wide range of semantic possibilities (Ramos 1987), the complex concept of *unokai* is bluntly reduced to "killer." In Chagnon's writings, the *tradutore, traditore* adage ("translator, traitor") could not have found a better illustration.

While the term *unokai* had been conspicuously absent in the successively transformed versions of his first book, all of a sudden it became a key concept in his sociobiological universe. *Unokaimu*, or *kanenemo* in the language of the Sanumá subgroup I studied, refers to the seclusion ritual to remove the condition of impurity attached to someone who has killed someone else either technically or symbolically. Now, the Yanomami notion of "killing" in the context of warfare is far from being the same as in western thinking. An enemy may have been killed by a specific person, but anyone else who shot an arrow at the wounded or even the dead body takes on the condition of *unokai*. In this way, you may have a large number of "killers" for each person killed. Moreover, *unokai* is also the person who has killed an animal judged to be the *rishi* (or *nonoshi* in Sanumá language) - "alter ego," or individual totem, in Durkheim's terms - of someone who lives far away. Westernly speaking, these cases involve the death of an animal, not of a human being.

In light of these ethnographic facts, what is one to make of Chagnon's statistics, even when he claims to have discarded from his sample the cases of "symbolic" killings i.e., of alter ego animals, as though the other modality of *unokaimu*, involving the ancillary gestures of the supporting cast of *unokai* in real raids, were anything but symbolic?

Chagnon's analyses of Yanomami violence suffer from a number of weaknesses. First, the fragile theoretical framework on which he grounds his research puts constraints on the full potential of ethnographic inquiry due to the narrow focus on western-style data gathering on demography, warfare and mobility. Chagnon fails to do what full fledged ethnographers do as the normal course of research, that is, generate knowledge from within the ethnographed culture itself. Put in a nutshell, Chagnon's writings expose his disregard for the "native's point of view." As a result, Chagnon's writings reveal a very cursory attention to the cultural moorings that would make sense of these topics from a Yanomami logical perspective.

Second, undoubtedly as a consequence of this theoretical indigence, he treats the empirical data with a blasé superficiality regarding linguistic complexities. One may feel justified in interpreting this casualness with other people's language in one of two ways: either his command of the local language was very poor indeed, or he chose to ignore its intricacies in order to compose his statistical data more comfortably. For, of course, the more elaborate the empirical facts, the more difficult it is to convert them into a simple chart, and simple Chagnon's charts certainly are. Too simple, in fact. The trouble with such simplifications is that they easily feed into a number of spurious circuits. One of these circuits can be exemplified by the pseudo-scientific concerns of a Marvin Harris whose appropriation of Chagnon's ethnography turned the Yanomami into a caricature of a bunch of savages killing each other for protein. Another of these unfortunate circuits is the lay imagination, fed as it is by sensationalistic pastiches of already poor ethnographies. At the end of this chain reaction of misrepresentations, the Yanomami are reduced to one short phrase: the most primitive people on earth!

A third weakness in Chagnon's prolonged research among the Yanomami in Venezuela is his total silence about the work of other scholars, particularly from the countries where the Yanomami live. This absentmindedness, not to say arrogance - equally the case of other researchers from the US and elsewhere - has produced a great number of misconceptions on the part of the foreign anthropologist about the larger social and political milieu surrounding the site of research. The consequences have been distortions and, in practical terms, the mismanagement of local situations, perfectly avoidable when a professional partnership with local colleagues is established. The lack of a productive dialogue between foreign ethnographers and researchers from the host countries has often resulted in the outright rejection of the former's interpretations by the latter frequently with good reason. Such an attitude is aggravated by ethnographers who feel justified in speaking in the name of their research subjects - a vice still far from uncommon.

Considering these issues that I have been calling weaknesses in Chagnon's work, it is easy to see that the concept of professional ethics covers a lot more ground than simply a matter of anthropologists directly doing harm or "doing good" (in William F. Fisher's words, 1997) to the people they study. For our social responsibility as professionals in the social sciences does not end the moment we interrupt our immediate interaction with our research subjects. Quite the contrary, it is perhaps more evident when we write about them. Why is it so difficult to admit that, whether we want it or not, we are involved in the

consequences of the kind of knowledge we generate? In this sense, to speak of neutrality of anthropology as science is, at best, sheer nonsense, at worst, bad faith. If we describe an entire ethnic group as "fierce," "gentle," or whatever simplified formula, we are directly responsible for whatever may follow from our descriptions. Here is a case in point.

In 1988, the military in Brazil were busy defining indigenous areas along the northern border. The Yanomami, like other peoples whose territory straddles the international frontiers, were targeted to have their lands chopped up in a myriad of parcels separated by corridors reserved for economic exploitation. Chagnon's article came in handy as the perfect justification for robbing the Yanomami of a continuous area. Although I very much doubt that the military actually read Chagnon's writings, they had plenty of opportunity to know about them through the press. Shortly after the publication of Chagnon's *Science* article, US newspapers published pieces referring to the violent habits of the Yanomami. Within days, two major Brazilian dailies picked up on these pieces and published them under sensationalistic headlines such as *Violence, the mark of the Yanomami* (*O Estado de São Paulo*, March 1st, 1988, p. 4. Translated from an article by Thomas Maugh in the *Los Angeles Times*). Around that time, a journalist of the *Miami Herald* told me that, in his interview with an Army high official in Brasilia, the military explained the decision to cut up Yanomami territory by saying that the Yanomami had to be kept separate from each other because they fought too much.

Alarmed by such negative publicity and its grave consequences for the Yanomami, anthropologists in Brazil sent out a warning to the ethics committee of the AAA, pointing out the need for the US association to take a stance on the ethical issues raised by Chagnon's careless writings. The president of the Brazilian Anthropological Association signed the letter which was published in 1989 in the *Anthropology Newsletter*, several months after it was written, due to the extreme reluctance on the part of the AAA president and the Correspondence editor to print it in the newsletter. That reluctance was partially explained by fear of Chagnon's threats to sue the AAA if our letter was published. Space was opened in the *Newsletter* for his response in which he nominally cited the Brazil-based French anthropologist Bruce Albert. Contrary to the ethics of public debates, the letters editor denied Albert the right to reply. In short, the US academy solemnly ignored the plea of the Brazilian anthropologists to curb unethical abuses by its members in foreign lands and even worse, among indigenous peoples. Nor did Albert's and my comments in *Science* (...), following Chagnon's article, draw a reaction from our US colleagues. In fact, anthropologists of Clifford Geertz's stature dismissed the political impact of ethnographic writings as mere "small potatoes" (*Science* ...).

Adding insult to injury, the same *Anthropology Newsletter* came back to rescue Chagnon's scientific reputation when, a few years later, he was under attack by Venezuelan Salesian missionaries. Robin Fox, condemning the anonymous denunciation of Chagnon for bad ethnography and bad conduct, and apparently still remembering our warning call five years earlier, quite extemporaneously, committed the following gaffe, perhaps unaware that Venezuela and Brazil are different countries: "This is based on one highly inaccurate letter published in these columns (from Brazilians with their own confused grievances) and ignores the universal esteem and admiration of the world's anthropologists for Chagnon's unique fieldwork effort among the Yanomamö." Fox ends his letter with an appeal: "American anthropologists, both individually and through their association, should rally to the support of Chagnon and the absolute value of his courageous and brilliant field studies

of Yanomamö culture as well as his practical efforts to save it" (*Anthropology Newsletter* March 1994, p. 2).

There is no end to how much irony history can pour on to us. Six years later, a rather mediocre piece of journalism succeeded where serious anthropologists had failed. Patrick Tierney, with his hyperbolic *Darkness in El Dorado* (Ramos 2001), indeed gave US anthropologists an extraordinary jolt. From our end, we can't help regarding the cyber scandal it caused as a certain measure of poetic justice.

Leaving aside the inaccuracy of Tierney's bottom line, that is, the accusation that the Neel/Chagnon team provoked many deaths among the Yanomami with an experimental measles vaccine, there is a great deal of information on unethical behavior on the part of that team. Although much of what Tierney unveils about Chagnon's field conduct was well known to those who read Chagnon's books (as the latter candidly describes scenes in which he blatant disrespects the Yanomami), the way in which US anthropologists reacted to the scandal has been one of complete surprise. Why is this? Perhaps the cumulative effect of so many sordid details put together, as in *Darkness in El Dorado*, composes a picture that is hard to detect if one reads Chagnon piecemeal. While Jacques Lizot discreetly conceals his pedophilic tastes, as uncovered by Tierney, behind "love stories" (Lizot 1976; see Ramos 1987), Chagnon makes a point of exposing himself as the reckless male who takes no nonsense from the natives. Much of the appeal his writings have among undergraduates comes from the image he projects of himself as the tough guy who is always in control, Schwarzenegger-style. Furthermore, the dubious liaisons Chagnon established with corrupted and corruptible characters among Venezuelan elites (in my view the best part of Tierney's journalistic coverage) in order to dodge legal procedures to enter the field area adds a sinister twist to his questionable fieldwork routines. It is not by chance that Sahllins (2001) associates Chagnon's brand of fieldwork with the mindless frenzy of the US military in Vietnam.

The very harmful exercise of image making has not been limited, in the case of the Yanomami, to the immediate threat of expropriation of traditional lands. There have been opinions proclaimed loud and clear that the Yanomami as a collectivity do not even deserve to live! Here is an example, not from the usual local politicians or economic groups interested in the Yanomami natural resources, but from a remote North American feminist college teacher. On March 21, 1990, the first page of the *Wall Street Journal* published an article titled "An Amazonian Tragedy" which reported on the violent gold rush in Yanomami land. The article also mentioned the alleged habit Yanomami men have of beating their women. Read in a class room at Menlo College in California by teacher Marilyn Faulkenburg, the piece caused a peculiar reaction in the class. A few days later, the same newspaper published a letter by Ms. Faulkenburg. In the name of an improbable universal feminism, and based on Marvin Harris' incursions into protein wars, Ms. Faulkenburg asked the ultimate question: "does this society deserve to be protected against the twentieth century? Or, to put the question another way: are the gold miners the real bandits in this story?" In her opinion, no, it is the anthropologists who are to blame, for they are the only ones who would benefit from the preservation of "such a brutal and primitive" culture (Sotero 1990). If Ms. Faulkenburg were to take her universalism literally, she would also have to propose the extinction of the US society, for wife battering is not exactly an unknown phenomenon in the United States. Nevertheless, this Californian feminist hit some sort of a target, albeit not the one intended, for the only one who really

benefitted from this unfortunate ethnographic fame was Napoleon Chagnon, its sole creator. Involuntarily turned into an object for free public manipulation, the Yanomami have become the hostages of a gratuitous reputation that was established for them despite themselves.

Ten years later, another battery of public fire is being levelled at the Yanomami and especially at those associated with them. While the aftermath of Chagnon's writings in previous decades included periodical blows to the tranquility of the Yanomami, the consequences of Tierney's denunciations are turning into a boomerang laden with complexities, now affecting many more people than just the Yanomami themselves.

Immediately after the news about Tierney's book *Darkness in El Dorado* leaked into cyberspace, a Brazilian senator from the state of Roraima where many Yanomami live, resurrected an old anti-indigenous tactic of defaming pro-Yanomami activists and accused the Pro-Yanomami Commission, a 22 year-old NGO, of making experiments with untested vaccines among the Yanomami. Either in ignorance or in bad faith, this man, Mozarildo Cavalcanti, chose the wrong target for his diatribe, as the health care of the Yanomami is now in the hands of another NGO. Nevertheless, once an accusation is made in public, it stays there until proved wrong.

Another effect of the Neel/Chagnon scandal uncovered by Tierney has been the moratorium on research declared by the Venezuelan government shortly after the scandal broke out. The news of this ban has also circulated in the internet. No research will be allowed in Venezuelan indigenous territories until the whole affair is clarified. Will it ever be satisfactorily elucidated? For how long will honest researchers remain the hostages of other people's unethical behavior? In a country where institutional controls on foreigners' research were meager - and one may attribute the excess of field abuses to this institutional void - it is interesting to see how the Neel/Chagnon scandal has resolved itself in Venezuela with a rather authoritarian measure by means of which ethically correct professionals are penalised along with unethical researchers.

Professional scandals apart, the controversies generated by the publication of *Darkness in El Dorado* promise to activate long due discussions on such issues as informed consent in the context of field research, the claims of neutrality of "real" science, and the nature of the social responsibility of the anthropologist as an author. As befits this day and age, this whole set of extremely important considerations is threatening to dissolve into the thin air of an industry of globalized ethics.

Take informed consent. How informed must consent be in order to insure that it is not simply subtle coercion or friendly persuasion that is employed? How is it constructed in the field, is it passed as a benign version of the infamous *requerimiento* of the violent early days of Spanish colonization, or is it the object of prolonged negotiations? Is it established the day we set foot on a village whose inhabitants do not speak our language, or months later, when we can communicate with our hosts with a minimum of competence? Can it be verbal or does it have to be signed? Is a written form of consent enough to inhibit abuses? On the other hand, will it jeopardize research itself? How empowered must a receiving community be to be able to exercise control over the researcher's acts? Who regulates the process, the host community, the host country, the researcher's professional association, or the researcher's government? Taken to its logical conclusion would informed consent inhibit and in due course obliterate research? What would absence of research mean for the peoples studied? On yet another key, could informed consent cover the results of research,

that is, ethnographic writing? Would it be an apt instrument to contemplate both the relatively short phase of interaction as well as the long term relationship that is established between researcher and researched, regardless of whether they interact or not?

Let me make a quick comparison on different ways of facing up to these issues. While in the United States the problem is being faced with the creation of a fact-finding task force pragmatically aiming at an "informed" conclusion about what happened 34 years ago, the Brazilian Anthropological Association is engaged in a series of workshops organized along two years, starting in December 2000, in various parts of Brazil to ponder on the political, social, legal, educational, and ethical factors involved in research with and on human beings. The positive and negative aspects of the concept of informed consent are being carefully explored in a public, participatory setting. This represents a necessary, but not sufficient, provision for the maintenance of ethical parameters in research. State controls are needed, but how many of them? What instruments and institutions are appropriate to set norms on ethnographic research? What if an excess of control ends up choking one of the most precious aspects of the long term ethnographic experience, which is the complicity established between ethnographer and ethnographed? One or two years of constant interaction creates an indelible tie in the field: we are as much part of their history as they are of ours. Now, to use this complicity for our own ends, gagging our subjects with no effort to convey their point of view, in disrespect of their will and rights, is a serious abuse of confidence. How to avoid this? Certainly, a discussion that does not contemplate specific anthropological ethos, local particularities, differences in state intervention, and different patterns of scientific expression will be an idle exercise in generalities. Conversely, to put an excessive emphasis on a concrete case - such as creating a task force to unravel a specific issue - but failing to use this as a springboard for a wider and deeper discussion amounts to a lost opportunity to reflect on broader issues. The anecdotal takes over and principles get lost.

Old ethics may die hard, yet they give clear signs of aging. Will we be prepared to hail the age of new ethics when the old one dies completely and forever, as the Pygmies would say to their ethnographer?

Just a final note on ethnographic optimism. A true believer of the power of dialectics, I see the repercussions of the Neel/Chagnon scandal as containing in itself the prospect of interesting spiral effect of theses, antitheses, and syntheses. On the one hand, anthropologists of various nationalities and persuasions can now seize this opportunity to seriously reflect upon their role in the world as social and political agents. On the other hand, peoples such the Yanomami are increasingly wiser in their interactions with outsiders. The problems raised in *Darkness in El Dorado* have the great potential of awakening the Yanomami to a new set of issues they previously either ignored or were impotent to handle. The cyber revolution, which has served movements such as the Zapatistas, is now opening new channels for the Yanomami as well. Internet round tables are in the air, and one of the ideas that will soon circulate is the possibility of a lawsuit by the Yanomami against the United States, as the government responsible for the financing, via the Nuclear Energy Commission, of the Neel/Chagnon genetic research during which a large quantity of Yanomami blood was collected and is now being processed in Pennsylvania and Michigan labs with no consent, informed or otherwise, of the Yanomami.

On another front, the Yanomami, portrayed the world over as the most primitive people on earth, following a personal leaning of a not so distinguished US anthropologist, now have an opportunity to show their skills at one of our most cherished fetishes, i.e., reading, writing, and mastering the microscope. In five years of schooling, the Yanomami in Brazil produce their own texts, read malaria slides, and enchant the team of non-Indian teachers with their lively and alert intelligence. Primitive who?

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