

***Biomedical research, ethnic labels, and
anthropological responsibility:
Further comments
(Part II)***

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Although all of us made efforts in Round One of our debate to rise above the factional manicheism of the Chagnon/Tierney dispute that has been raging ever since the galley proofs of *Darkness in El Dorado* circulated and the revised book was published, perhaps we did not entirely get beyond its initial terms. However, it is clear that all contributors demonstrated a deep concern for the Yanomami's condition; as Kim Hill put it so well, "the health and welfare of the study population must always take precedence over any academic goal." If we keep such principles in mind, we will surely continue our progress in discussing ethical questions directly relevant to the rights and survival of the Yanomami (and other indigenous peoples).

Most contributions focused on a major theme of discussion (Terry Turner and I on the aspect of biomedical research, Lêda Martins and Ray Hames on ethnographic images), while the two other authors raised a wider range of ethical themes (Kim Hill and John Peters). In this paper, I will comment on these contributions in this same order.

On the 1968 Orinoco epidemic and biomedical research

Questions concerning James Neel's biomedical research and vaccinations in 1968, to which I dedicated a lot of attention in my contribution (and previous efforts), were also the focus of Terry Turner's piece. His document research (along with J. Stevens) on James Neel's papers and correspondence in the archives of the American Philosophical Society (Philadelphia) is a very important initiative. It contributes a great deal toward moving the debate about Chapter 5 ("Outbreak") of *Darkness in El Dorado* beyond the unrigorous journalism and biased polemic that have been raging since September, 2000. Such was the intention of the research I commissioned (and assisted on anthropological points) from a group of experienced Brazilian physicians of the Federal University of Rio de Janeiro (Lobo *et al.* 2000). I am thus particularly satisfied that these two research initiatives converge on several fundamental points and complete each other on many others (see my first contribution for a summary of the Brazilian medical report). In fact, these complementary findings substantially alter the way in which Neel's research and vaccinations have been seen and discussed up to now.

The paranoid, nightmarish scenario of experiments and eugenics imagined by Patrick Tierney in the preliminary version of his book—subsequently muddled and attenuated to the point of being self-contradictory in the published version—has by now been completely and definitively discredited. But, as Turner and I made clear, several aspects of Neel's expedition in 1968 still need to be evaluated in terms of biomedical ethical norms. The Brazilian physicians' report focused on three main points: possible experimentation during vaccinations with and without immunoglobulin (MIG); inadequate planning and training to cope with the epidemic; and failure to properly obtain informed consent while collecting biological samples. Turner's research sheds further light on these points by demonstrating that the vaccinations were originally planned as a research tool rather than as a health care measure, and confirming the low priority that Neel gave to immunizations compared with his research agenda, even when the measles epidemic was raging along the Rio Orinoco (with the result that most of the vaccinations were not administered in time to reduce mortality).

The question of Neel's official authorizations, at least in Brazil, is getting clarified. In mid-March, I consulted a research report on this matter written by an official at the National Indian Agency (*Fundação Nacional do Índio*, or FUNAI for short) in Brasília (Furtado Filho 2001). This report shows, to the benefit of James Neel and his U.S. and Brazilian colleagues, that official authorizations for their research among the Yanomami were properly granted by different FUNAI Presidents in 1970, 1972, and 1974. No documents have been found yet regarding their first expedition in 1967, probably because the files of the government agency that preceded FUNAI, the

Indian Protection Service (the *Serviço de Proteção aos Índios*, or SPI), were partially destroyed in a fire (1968) and the remaining archives have not been systematically organized. However, the report demonstrates that the President of FUNAI in 1970, General Bandeira de Mello, was informed of Neel's 1967 expedition.

Kim Hill dedicates a part of his contribution to Neel's Orinoco vaccinations and research. His discussion does not take into account the new information contained in the Brazilian medical report (posted on the net since last December in Portuguese, and since February in English: see reference to Lobo *et al.* 2000 in the bibliography for the URL), nor, of course, Turner's research into Neel's archives. His approach largely remains within the spirit of earlier stages of the polemics, defending Neel mainly by targeting Tierney's "ideological warfare" and "ideological terrorism." I hope that the new kinds of information brought to our debate will lead to a different way of discussing the issue.

I am not a "Tierney supporter" and, like the Brazilian physicians, I was shocked by his irresponsible and incompetent writing about the history of the epidemic. But, as a Yanomami supporter, I do not think that Neel's work among the Orinoco Yanomami in 1968 was merely a "combination of treatment and research," nor that his blood samples "were critical to save Yanomami lives." I will wait until the next round of discussions to have, perhaps, an exchange of opinions on this matter with Kim Hill (in response to the contributions by Turner and myself).

Like Kim Hill, I also addressed the issue of radioiodine 131 research in my first contribution, but in very different terms. I do not agree with the idea that, since the Iodine 131 research (from 1958-1970) may have been too complicated to explain to the Yanomami, this was therefore a justification for not bothering with their rights to informed consent, which, I must insist, are not simply a matter of "today's standards," but were in vigor since the Nuremberg Code (1947). This Code does not sustain the idea that, if people do not understand the research and experiments to be made on or about them, they are by definition available to serve as human material for these research projects and experiments without informed consent.

This idea is extremely dangerous. It means that the presence or absence of a common communication ground (linguistic or cultural) could legitimately be used as a criterion for granting to or withholding from a person or people the right to informed consent in biomedical research. This is essentially the same criterion that is used to justify animal experiments, which are rationalized on the principle that animals are not included in the moral community, since they cannot express their interests (classical philosophy restricts the "equality of justice" only to moral beings capable of expression).¹ Let me quote Nadia Farage on this dangerous slide:

...speech, the power of speech, the literal distinction between humans and animals, metaphorically extends to all of us as humans and, as a metaphor, is no longer a question of nature, but, rather, of degree. This explains the fact that experimentation, which is usually restricted to animals, has been applied to social categories whose discourse is confiscated in oppressive political situations. (Farage 1999:6)

Finally, Kim Hill condemns the notion, which he attributes to Patrick Tierney, that research done on an indigenous population that is not designed to help those same population is unethical. It seems to me that the problem is not properly formulated here. The question is, in reality, that, once they are fully informed about a research project, indigenous people have the perfect right not to authorize research among their collectivity on the grounds that it is of no direct benefit to them, or to

¹ On this matter, see Elisabeth de Fontenay 2000.

negotiate with the researchers that some part of the research activity or funds be funneled into something that is more directly beneficial to their community. These negotiations are taking place more frequently as the process of indigenous empowerment advances (Albert 1997). To get authorizations from research and government Indian agencies these days, researchers in the Brazilian Amazon have to negotiate the conditions under which their projects will be conducted, as well as with Indian leaders and/or Indian organizations (which number more than 180; see Albert 2001). In fact, this negotiation process is now a matter of official regulation.²

Besides, it sounds somehow paternalistic (as if to suggest that "behind every Indian is a white man") to think that indigenous people need to be "indoctrinated" (in Hill's words) to reach the opinion that biomedical research done among them should have some kind of benefit to their families and communities, when, in reality, they suffer from precarious or nonexistent health care. This indigenous view does not seem to me to be an affront to science, but a matter of justice. Through their research projects, scientists gain a direct benefit to their careers. Let us admit that their work could also bring a universal benefit to humanity, as Kim Hill insists. If so, scientists would gain double benefits, both direct and indirect, as researchers and as members of humanity. Why couldn't indigenous people be granted the same privilege: a negotiated direct benefit as communities collaborating in the research (if they decide so) and an indirect benefit as members of humanity? Or should they really be left to suffer their horrible health conditions with a few trinkets and the injunction to be proud of contributing to the universal advancement of science?

Ethnographic images and political responsibilities

Implied in Chagnon's finding so far is a notion startling to traditional anthropology: the rather horrifying Yanomamo culture makes some sense in terms of animal behavior. (Time Magazine, May 10, 1976, page 69)

Studying and publishing works on Yanomami warfare (sociopolitical, cultural, or other aspects), as many of us have done, is one thing. Pinning this ethnic minority with the exoticizing, stereotypical label of the "Fierce People" (the title of Chagnon's book from 1968 to 1992), knowing full well how vulnerable they are to dramatic local threats (racial discrimination, land invasion, and physical violence), is quite another.

Nobody maintains that the Yanomami do not practice warfare or that Yanomami individuals are not occasionally violent (true for most societies, including the U.S., where some kids even shoot up their schools).³ But many people do maintain that it is unethical and politically damaging to reduce the richness of Yanomami society and culture to the stereotypical image of "the barbaric violence [that] Chagnon documented" (*Time Magazine* 1995). It requires only a minimal ethical sensibility and political awareness to understand that such long-term pejorative labeling and its apparent scientific authority can be (and have been) used by anti-Indian agitators to *rationalize* and *encourage* violations of Yanomami rights—nobody ever said such labeling *caused* them. Does one really need to be a left-wing radical to fear the impact of articles like the one in *Time Magazine*, "Beastly or Manly?," once they ramify in the Brazilian press, in reinforcing the racist justification of

² Resolution 304 of August 9, 2000 of the National Health Council in Brazil requires that a proposal for research in indigenous communities include: "1) a commitment to obtain the consent of the communities involved and a description of the process of obtaining this consent; 2) a description of the process of obtaining and recording the Terms of Free and Enlightened Consent, demonstrating the adequacy [of the process] to the cultural and linguistic particularities of those involved."

³ See the impressive cover of *Time Magazine* March 19, 2001, vol.157, #11.

military officials as they plan the dismemberment of Yanomami lands?⁴ Does one need to be an enemy of sociobiology to understand that a researcher bears some ethical responsibility if he publishes and invites widespread media coverage of a paper about "blood revenge" and "Yanomamö killers" (Chagnon 1988), during a gold rush into Yanomami lands in Brazil? After all, the invasion in Roraima (1987-1990) involved almost forty thousand gold panners trying to expropriate those lands or exterminate their legitimate owners, leading to the deaths of about 1,200 to 1,500 Yanomami.

If the recent reaffirmation by the Brazilian Anthropological Association (ABA) of the contents of its 1988 letters of protest to the American Anthropological Association (AAA) on this case (see Oliven 2000) is not yet considered sufficient evidence,⁵ then Lêda Martins's contribution to this debate offers more vivid, direct testimony about the impact of Chagnon's publications in Brazil in the 1990s.

In view of this context, it is clearly reasonable to hold that an ethics of responsibility is involved in creating ethnic labels for the people with whom we work and contributing to the spread of such labels through mass media. This basic responsibility, which should be the concern of any anthropologist, involves also avoiding and fighting, as much as possible, the misuse of our ethnographies against the societies they describe. In his contribution, Kim Hill expresses a deep concern about this point, which he shares with many so-called "Chagnon opponents." Indeed, his own work, as he described it, is exemplary in this respect. Ray Hames agrees, too, with the principle that "we have an obligation to ensure that what we produce is not used by others to harm the people we study and, if necessary, to engage in political action to defend injustices meted to those we study." Unfortunately, in his (generous) effort on Chagnon's behalf, he gradually shifts the problem away from its original context and onto a generic level where it gets lost. He begins by saying that Chagnon was unduly accused, but that, anyway, he changed his ways (so then why did this supposed mutation take place, if the accusations were irrelevant?). He then argues that, whatever forms our ethnographic accounts take, they have no impact on the fate of indigenous people anyway (so then why did he affirm the principle quoted above?). He goes on to criticize pro-Indian NGOs for spreading the image of the "noble savage" and ends his paper by offering them a lesson in human rights. In the end, one wonders where this juxtaposition of arguments is supposed to lead. Does it mean that anthropological writing transcends ethics and that NGOs should heed our armchair preaching and carry out the job for us?

Finally, going back to the Yanomami realities, we need to ask why Napoleon Chagnon never publicly came out to condemn the use of his work by sensationalist journalists and unscrupulous politicians, or to support the international movement in defense of Yanomami survival that began at

⁴ Let me briefly quote a Brazilian military document written in 1977 about the Yanomami:

We see that...the group lives in fiefs, each one made up of 50 to 200 Indians, and that each group is hostile to the others, leading us to conclude that the physical relations between man and woman occurs between siblings, father and daughters, mother and sons, and perhaps even between grandsons and grandmothers, and granddaughters and grandfathers, constituting true incest, which, over the centuries, has been causing the physical and intellectual atrophy of this indigenous group. (Oliveira 1977):

This racist delirium served as the justification for sending a study group to the field in March, 1978. This group's report became the basis for a project of dismembering Yanomami lands in Brazil in nineteen "islands." The Pro-Yanomami Commission (CCPY) originated in the fight against this expropriation.

⁵ Hames writes that Alcida Ramos and I were "instrumental in drafting the ABA denunciation of Chagnon." I certainly was consulted at the time as a Yanomami ethnographer and advocate by the ABA President. However, the decision to write the letter and adopt a critical stance was entirely ABA's initiative, taken when its President at the time, Manuela Carneiro da Cunha, was also, like CCPY, at the forefront of the struggle for Yanomami survival.

the end of the seventies. Instead, he dedicated his time and energy to waging a media war against advocates for Yanomami land and human rights. For instance, consider his three-page interview in *Veja* (the main news magazine in Brazil) in 1995, where he criticized the coordinated actions among NGOs on behalf of the Yanomami as amounting to nothing more than a competition to become the "exclusive owner of the Yanomami cause" just to make money. Consider also his accusation against Davi Kopenawa, a major Yanomami spokesperson, as being no more than "a parrot of human rights groups" (Monaghan 1994:A10).

Adding insult to injury, this reduplication of ethical irresponsibility may even be worse than his original offense of persistently labeling the Yanomami in such negative terms—which resulted in decades of media caricatures of them as savage/prehistorical/primates.

Undoubtedly shaken by the debates swirling around his 1988 article in *Science*, Chagnon suddenly announced in 1989 the creation of a "Yanomamö Survival Fund," which remained inactive, at least until 1997 (Rabben 1998:138, n. 7). In 1992, he changed the title of his famous book to "Yanomamö: The Last Days of Eden," including in its final chapter some lyrical statements about his future dedication to Yanomami rights (Rabben 1998:36-37). Will the shallow ethics of such editorial plastic surgery be sufficient to erase the stigma of the "Fierce People" label pinned on the Yanomamö for so much time? One can entertain doubts about this.

Writing a few declarations on anthropological advocacy here and there, on the one hand, and trying to transform the dramatic realities confronted by indigenous people through effective forms of social engagement, on the other, are two quite different things. The comfortable confusion of the two is a very common, but no less ethically dubious, artifice.

Yanomami ethical miscellanea: trade goods, genealogical methods, and redistribution of gains

Trade goods and conflicts

Kim Hill raises the issue of the extent to which trade goods cause conflicts in the population under study. Most of us use trade goods to reciprocate our informants and others for many services (food, transport, guiding, etc.) or simply to give presents to our hosts. In a society like the Yanomami, trading is embedded in every social relation, and, as Mauss put it, "*le bien remplace le lien*" (1991 [1925]). What is at stake with Chagnon's fieldwork is a very different problem. His research was not the usual type of anthropological fieldwork. He was the "jungle advance man" (Sahlins 2000) of Neel's huge project for the Atomic Energy Commission (AEC) from 1966 to 1972, endowed with a budget of more than 2.5 million dollars. He had to follow an intensive research agenda for collecting blood and genealogies, filming, and performing many other services for Neel's project. For years, he spent his time passing back and forth through some forty to fifty Yanomami villages at a frantic pace, distributing huge amounts of trade goods to the Indians as payment and to gain their good will and collaboration with the AEC project. For any ethnographer of the Yanomami or other Amazonian groups, it would not be a surprise that such unusually hectic fieldwork and forms of compensation could have generated so many conflicts between Chagnon and the Yanomami, and between the Yanomami themselves, as each village competed to get the biggest part possible of this incredible *mana* from U.S. Atomic Energy Commission funding.

Unethical name collecting

As Kim Hill rightly guesses, I began my fieldwork in 1975 like many, if not all Yanomami ethnographers, collecting names of individuals and their relatives to study their kinship. Since the mid-'80s, I have often had to do this again, this time for more pressing reasons as part of medical emergency field missions (for instance, patients needed personal identification for malaria exams and treatment). The Yanomami never utter their own names when asked, their classical answer being "I don't know. I have no name." Traditional Yanomami names, which are nicknames and frequently pejorative to one degree or another, cannot be pronounced in front of a person or his/her close relatives—"to insult" is a synonym of "to name" in Yanomami (Albert 1985:394-404). But these nicknames circulate freely at a distance among unrelated people. I described a simple methodology for getting these names during medical missions in a linguistic field manual on Yanomami health published a few years ago. As the following passage demonstrates, it does not involve "bribing," tricking, or offending anybody:

If the person does not have a Portuguese nickname, one should find out his or her Yanomami name from another person who is not a relative, preferably coming from another village. The question should be made discretely, out of earshot of the person named and close relatives. Children or leaders can be of great help in identifying Yanomami names: the former, because it is a fun game, the latter because no one is going to complain about being named by them (since publicly naming people is a demonstration of courage). (Albert and Gomez 1997:182-183)

Here, once again, the atypical "hit-and-run" fieldwork methods used by Chagnon in his frenetic schedule of collecting genealogies and blood for the AEC must have induced him to invent *ad hoc* measures for getting around Yanomami name secrecy in ways that were more aggressive and less ethical. Had he used the more typical slow pace and low-profile attitude that most anthropologists use during fieldwork, he would never have found himself in situations of having to resort to bribery, trickery, or offensive behaviors to collect names. The chaotic and peripatetic nature of his AEC agenda probably did force him into such situations. It is crucial to keep in mind that much of Chagnon's core ethnography is a by-product of the work commissioned by the AEC from 1966 to 1972 (mostly genealogical, demographic, and settlement pattern data). This probably also explains why his ethnography is so weak on the cultural and linguistic side of Yanomami reality.

Redistribution of financial gains and the "anti-Chagnon plot"

A concrete commitment from Chagnon to help the Yanomami (for example, in coping with their extremely precarious, and at times tragic, health situation) and to fairly redistribute the economic benefits he gained from them during his long career is still awaited. I agree with Kim Hill when he writes that Chagnon should clearly explain what kind of assistance (if any) he provided or intends to provide to the Yanomami and how he intends to redistribute what he gained from them: a lifetime career and probably a considerable amount of money on copyrights of books, films, and photos. (I also agree completely with the idea of redistributing the royalties of *Yanomamö: The Fierce People* and *Darkness in El Dorado* to the Yanomami!).⁶

⁶Although not a "Tierney ally," I wish to answer Kim Hill's challenge about book royalties. When Gomez and I published a linguistic and cultural field manual on Yanomami disease conceptions (Albert and Gomez 1997), half of the copies were bought by the French IRD (Research Institute for Development) and distributed to health workers among the Yanomami in the states of Roraima and Amazonas. When Milliken and I published a book on Yanomami ethnobotany (Milliken and Albert 1999), we donated the rights to Survival International. Besides paying my Yanomami informants, I am a co-

However, I disagree when Hill suggests that the reasons why Chagnon has not contributed anything to the Yanomami cause or welfare is because his enemies prevent him from going back to the field to make agreements with the people he worked with. I disagree, first, because Chagnon seems to have done nothing especially remarkable on this count while he was allowed to do research in Venezuela for many years (and, given his skills in media promotion, we would probably know about it); and, second, because no omnipotent, terrorist anti-sociobiological enemies are preventing Chagnon from entering the field. In Brazil, for example, it was the government Indian agency FUNAI that did so because he was trying to smuggle blood samples out of Yanomami territory. A new FUNAI document on the subject, entitled "Napoleon Chagnon Case," states:

In 1995, Chagnon was granted authorization to enter the Yanomami area for an article for the magazine *Veja*. He was accompanied by the photographer Antonio Luis Torrey. When they began working in the area, he tried to collect blood samples from the Indians. When this was reported, FUNAI intervened and ordered him to leave the area. In 1997, the same anthropologist requested new authorization, in conjunction with the University of Roraima, this time for research. FUNAI denied the request. (Furtado Filho 2001)

As to an "anti-sociobiological plot" against him, my impression is, on the contrary, that the use of Napoleon Chagnon by some as a media symbol of sociobiological studies was not a very productive move for that brand of research. Given his controversial way of speaking about the Yanomami to the media and his ethically dubious field methods, in the final analysis, he probably generated more bad publicity for sociobiology than anything else. If we put aside academic debates over the political ideology underlying sociobiology and the ethnographic validity of its hypotheses, it is not far-fetched to suggest that, in the context of the Yanomami debate, sociobiology has been publicly spurned in Venezuela and Brazil more *because* of Chagnon's behavior than the contrary.

Anthropological and missionary views on social change (and the "spiritual world")

I will end this contribution with a few comments on John Peters' paper. First I want to set his mind at rest: I know and appreciate his demographic work with John Early (a valuable contribution to Yanomami studies), and I won't discriminate against him because he is an ex-missionary of the Unevangelized Fields Mission. Obviously, this does not mean that I agree with the missionary under the anthropologist. However, I must add that I do not hold the absurd view that Indian societies must not change, a notion that he attributes mysteriously to "some NGO".⁷ I simply think that indigenous peoples must have the chance to decide for themselves if and how they want to change, and be given the means to keep control over the changes that *they* (not anthropologists, NGOs, or missionaries) eventually decide to make.

Thus, the notion that we should "keep the status quo in [an] indigenous culture" (supposedly a NGO conception) or practice "advocacy for what the indigenous peoples themselves might do in terms of the problematic aspects of their society" (the missionary position Peters favors), seems to me to be

founder and board member of two NGOs that have been working on behalf of Yanomami rights and welfare in Brazil since the 1970s.

⁷ Elsewhere he clarifies that this NGO is the Pro-Yanomami Commission (CCPY). Peters speaks well of CCPY's health and political work in many places of his last book (Peters 1998), but at the end, he states, "They (CCPY and FUNAI) want the Yanomami to retain traditional practices, even to the point of returning to the wearing of the loin cloth for the men and the small apron for the women, but the Xilixana will never return to the form of clothing used before contact" (1998:261). I certainly hope that Peters will admit that, as anthropologists, both Alcida Ramos (President of CCPY, who has worked with the Yanomami since 1967) and I (a member of the CCPY board of directors, having worked with the Yanomami since 1975) have a more sophisticated view of the problem of social change.

equally paternalistic and unacceptable views on the subject of indigenous social and cultural change. As an anthropologist and member of pro-Yanomami NGOs, I think—in opposition to these views—that the Indians must have their collective rights respected (to land, health care, adapted education, and to political, cultural, and linguistic autonomy) and that they must have the choice to decide for themselves what they want to do with their lives and their society (which includes the right to go on with their own "spiritual world"). This is also what is established in paragraph 231 of the Brazilian Constitution of 1988.

A parenthesis here: John Peters writes that "no researcher comprehends the Yanomami spiritual world". I don't exactly understand what he means here, but perhaps I need to remind him that several people have at least tried to understand the Yanomami shamanic, ritual, and mythological world. For example, Wilbert and Simoneau (1990) published a compilation of 364 Yanomami myths collected by nine Yanomami ethnographers, one of whom is Don Borgman, a fellow missionary of Peters. I personally wrote a doctoral thesis of eight hundred and something pages on the Yanomami ritual system (Albert 1985); also, the health field guide I wrote with Gale Goodwin Gomez (Albert and Gomez 1997) presents many Yanomami concepts on diseases and shamanism. Well before that, Kenneth Taylor wrote about Yanomami shamanism (1974), and students in Brazil are still doing so at present (e.g., Smiljanic Borges 1999).

I am not sure that John Peters, here more a missionary than an anthropologist, really contributes much himself to "comprehend[ing] the Yanomami spiritual world" when he writes:

The approach of both FUNAI [the government Indian bureau] and CCPY [our NGO] is to encourage shamanism and not curb sorcery. The missionaries, for their part, see the Yanomami world of spirits as integral to the peoples' lives, but they believe in another viable option as well. They believe that most Yanomami spiritual forces enslave the people, bringing fear and retaliation, and that God's power liberates. At least a few Xilixana [a Yanomami sub-group] have shown their belief that they have experienced this liberation. (Peters 1998:262) (*The rest of the page continues in the same vein of evangelical proselytizing.*)

Finally, I agree with the sensitive critique Peters makes of the "arrogant stance" of anthropologists and anthropology (on which an abundant literature has been produced since at least the mid-'80s). However, I was disappointed that he did not exercise the same sensible critical thinking about the effects of missionary proselytizing on indigenous cultures. "Christian missions have changed," he writes. I agree with that. They attenuated the "arrogant stance" of their proselytizing in the field, largely because of pressures from FUNAI and indigenous people, as Peters (1998:262) himself explains in the case of the Brazilian Yanomami. But do missionaries actually reflect critically on their own ethnocentric and paternalistic views of indigenous cultures and societies (as anthropologists certainly do; see Albert 1997)? I am not really sure of that when I read, amidst John Peter's admirable anthropological *mea culpa*, an extreme and superfluous condemnation of Yanomami shamanic practices three times on the same page (describing them as "destructive to life," "pronouncements of death," and prejudicial to health!), although these practices constitute a central part of their "spiritual world," culture, and society.

To conclude, let me quote my friend Davi Kopenawa Yanomami, a strident shaman and a strong-spirited Yanomami spokesperson:

White people make designs for their words because their thinking is so full of forgetting. We keep the words of our ancestors within us for a long time and continue to pass them on to our sons and daughters. Children, who know nothing about the *xapiripë* shaman's spirits,

listen to the songs of the shamans and then want to see the spirits for themselves. That is how the words of the shamanic spirits, despite being very ancient, always get renewed. They are what enhance our thinking. They are what make us see and understand faraway things, the things of our ancestors. This is our form of study, which teaches us to dream. In this way, whoever does not drink the breath of the spirits has thoughts that are full of smoke and oblivion; whoever is not seen by the *xapiripë* spirits does not dream, but only sleeps, like an axe on the ground.⁸

So, taking inspiration from Davi's statement—setting aside for once our thinking so "full of smoke and oblivion"—let us try to focus on the best interests of the Yanomami and make our debate useful to them. To do that, let me reiterate what I said in my first paper, that we focus our efforts on dealing concretely with the serious ethical breaches of biomedical research still being committed against the Yanomami, and, similarly, denounce the lack of medical assistance that is still decimating them, especially in Venezuela. We must also publicly support the land rights of the Yanomami, which continue to be threatened by the military, as demonstrated in a very recent statement by the Defense Minister of Brazil:

The Minister of Defense, Geraldo Quintão, described the demarcation of the eight million hectares for the Yanomami reservation in Roraima as an "error" and suggested that the decision by ex-President Fernando Collor [to ratify it] could be revoked. "It is a very delicate question, here and abroad, but it must be debated by our society," he argued. "...We can never permit, under any circumstances whatsoever, an indigenous tribe to be called an indigenous nation. It is an act of violence against the integrity of our territory, which we cannot tolerate," said Quintão, condemning the demarcation of integral reservations. "These lands continue to belong to the Union, which has total dominion over them," the Minister added, criticizing the Yanomami area, demarcated with one continuous boundary, as "a horrible example." (Monteiro 2001)

⁸Recorded September, 1998, at "Windy Mountain" village. Translation by Bruce Albert.