



Correspondence

To the Editor:

Is it naïveté? Is it arrogance? Is it unreconstructed old-fashioned imperialism? What explains the offering of resolutions to the Annual Business Meeting of the Association calling for Americans to interfere directly in the political affairs of the Philippines, Rumania and Burundi?

Of the six resolutions moved at the 87th Annual Meeting, only one, number 4, on "Language-minority Americans," was a matter on which the majority of members of the Association could and should act as citizens of the US and its states. Resolution 2, on discrimination against AIDS carriers, may have been stimulated by political debates within the US but made no reference to these debates; we can only presume the mover intends to use the Association's approval of his resolution in support of political action. Resolution 6, on the Caiha Norte Project in Brazil, is worded in a politically sophisticated manner, specifying the relevance of the cause to the Association (reported also in the October AN) and the means by which the Association can effect the mover's intent to influence a foreign government. The remaining three resolutions were inappropriate, at least as worded, and therefore a waste of members' time; they belonged at the Open Forum.

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[The following letter from Maria Manuela Carneiro da Cunha was addressed originally to the AAA Committee on Ethics. Subsequently, the president of the Brazilian Anthropological Association (ABA), Antonio Augusto Arantes, stating that Carneiro da Cunha's letter "expresses the (Brazilian Anthropological) Association's point of view about Prof Chagnon's (Science) article" (cited below), asked that the letter be published in AN. We herein publish the exchange between Carneiro da Cunha and Napoleon Chagnon (California-Santa Barbara), which will appear concurrently in Portuguese in the ABA's bulletin. Ordinarily, AN Correspondence submissions are not to exceed 500 words. This exchange, between one of our own distinguished members and another national anthropological association, is extraordinary and an exception to the rule.

— Ed.]

To the Editor:

The recent appearance in the Brazilian press of two articles on the Yanomami Indians based on Napoleon Chagnon's latest paper on Yanomami "violence" ("Life Histories, Blood Revenge, and Warfare in a Tribal Population," *Science* 239:985-992, 1988) has prompted us to call your attention to the extremely serious consequences that such publicity can have for the land rights and survival of the Yanomami in Brazil. The articles in question appeared in two major newspapers, *O Estado de São Paulo* ("Violência, marca dos Yanomami," March 1, 1988, p 14), and *O Globo* ("Antropólogo aponta violência entre índios," March 1, 1988, p 6), both translations into Portuguese of pieces that originally came out in *The Los Angeles Times* ("Anthropologists Study Homicidal Yanomamos: Remote Tribe Shows Streak of Violence," February 26, 1988, p 34), and *The Washington Post* ("Sexual Competition and Violence: Researcher Advances New Theory for Amazon Tribe's Homicides," February 29, 1988, p A3).

Without getting into the academic details of Chagnon's arguments, we would like to

stress some points that bear directly on the appropriation that the press has been making of his writings.

First, he affirms that violence is the principal cause of death among the Yanomami, with 44% of the males in his samples having killed someone. Correlating these figures with male reproductive performance, he concludes that killing is biologically advantageous for reproduction—killers are more efficient at securing mates. This argument, appealing as it may be for sensationalist purposes, is actually built on shaky grounds. The statistical tables with "hard" figures are supplemented with a text riddled with expressions such as the following:

"I can only speculate about the mechanisms that link a high reproductive success with *unokai* ['killer'] status" (p 989);

killers "seem to be more attractive as mates than non-*unokais*" (p 989);

"achieving cultural success appears to lead to biological (genetic) success" (p 985);

"intensity of grief appears to follow patterns predicted from kin selection theory" (p 991, emphasis ours)

We all know that figures do not speak for themselves, but are collected and analyzed by the human beings doing the research. Indeed, figures are as vulnerable to misrepresentation as any other kind of research tool, as Leach, for example, once realized: "The numerical apparatus in which these conclusions are embedded seems to me to be very largely a complicated piece of self-deception" (*Anthropologists in the Field*, Jongmans and Gutkind, eds, 1967, p 76).

Chagnon's 1988 figures are all the more puzzling when we turn to the PhD dissertation of one of his students and read that of the broad categories of causes of death, violence falls rather low. From 1970 through 1974, infectious diseases due to contact with whites killed 69%, violence 12%, accidental trauma 7% and degenerative diseases 6% (Thomas Melancon, *Marriage and Reproduction among the Yanomamö Indians of Venezuela*, PhD dissertation, UMI, 1982, p 42). Melancon wrote his study entirely based on Chagnon's data. Combined with Melancon's study are the figures Chagnon himself gives in his 1974 book, *Studying the Yanomamö*. In a rather disturbing table on p 160, we find that of a total of 555 deaths that occurred in two villages, 53% were due to contagious diseases and 20% to warfare. The figures, however, are so arranged as to make the numbers for deaths by warfare stand out as very large because the deaths due to infectious diseases are spread out throughout six categories, two of which, given in Yanomamö language, are mutual synonyms (*shawara* and *waywayu*), as explained in a footnote. Now, in 1988, Chagnon claims that Yanomami killings are the main cause of deaths. One wonders why the figures changed so much from 1982 (the date of Melancon's approved dissertation) to 1987 (the date of Chagnon's latest field trip) to reverse the trend, when the most notable change in Yanomami lives has been the intensification of contact with whites and their diseases.

Second, the alleged correlation between successful killing and successful reproduction, the thrust of Chagnon's article, has been contested by various specialists of Yanomami culture, including Melancon himself. Furthermore, the category of "killer" (*unokai*), translated as "assassin" by the media, has been demonstrated for another subgroup of the Yanomami to be a symbolic rather than a practical category (Bruce Albert, *Temps du Sang, Temps des Cendres*, doctoral disserta-

tion, University of Paris X, 1985). A man in the condition of *unokai* enters a period of purification when he "kills" someone's spirits, when he kills someone or when he thrusts his arrow into a body killed by someone else. Actual killings are, thus, much less frequent than symbolic killings, and both are the appropriate occasion for men to become *unokai*, ie, who have undergone ritual purification. It is interesting to point out that Chagnon admits that he never witnessed any violent deaths: "I did not accompany raiding parties and did not witness the killings that occurred while I lived there" (p 991); also, "Many raiding parties turn back before reaching their destination" (p 987). What this shows is the lack of anthropological sensitivity on his part to distinguish between real practices and ideological elaborations.

Third, Chagnon's article ends with the following passage:

A particularly acute insight into the power of law to thwart killing from revenge was provided to me by a young Yanomamö man in 1987. He had been taught Spanish by missionaries and sent to the territorial capital for training in practical nursing. There he discovered police and laws. He excitedly told me that he had visited the town's largest *pata* (the territorial governor) and urged him to make law and police available to his people so that they would not have to engage any longer in their wars of revenge and have to live in constant fear. (p 990)

The damaging effect of this passage, which is extraneous to the overt purpose of the article, is immediately visible in the echoes it produced in *The Washington Post* and *O Globo* articles. It provides the State with arguments for a complete control over the Indians.

The concern for the consequences of our professional activities was pointed out as early as 1967 by J A Barnes, the English anthropologist who expressed himself as follows:

[the modern ethnographer] is aware that what he writes may well become the basis for action designed to alter what he describes and will therefore either take special steps to prevent this happening or, alternatively, he will seek consciously to influence and even to take responsibility for such action. (*Anthropologists in the Field*, Jongmans and Gutkind, eds, 1967, p 195)

Barnes's warning gives the ethnographer the benefit of the doubt, referring to possible alterations of his writings by others. But this is not Chagnon's case. If we examine the three articles—Chagnon's and the two pieces in the American and Brazilian newspapers—we notice a remarkable fidelity of the journalistic material to its academic source.

We would like, therefore, to emphasize that the academic reification of "violence" and "sexual competition" as the dominant features of Yanomami society, as well as the tendency to encourage their propagation in the mass media with all the sensationalism it generates are not devoid of serious implications for the people who become the object of these public images. This is a very grave matter and leads us to ponder on the social responsibility of anthropological work.

On May 10, 1976, *Time Magazine* (p 17) published a highly biased article, "Beastly or Manly?" about the Yanomami, based on Chagnon's writings. Since his first book, published in 1968, he has labeled them "The Fierce People"; this epithet has turned into a stereotype that is difficult to avoid even among university audiences. Chagnon's publications not only contribute to reinforcing the negative prejudices which usually weigh on indigenous populations (something of a paradox for anthropological research), but also their appearance in the media has con-

sequences that are even more directly damaging for the Yanomami. Thus, less than a year after the *Time Magazine* piece came out, top-level officials of the Brazilian Indian Service (Fundação Nacional do Índio-Funai) referred to the Yanomami "violence" as sufficient justification for a plan to cut up their lands into 21 micro-reserves that were to be surrounded by corridors for the installation of regional economic projects, a plan that was intended to put an end to the aggressive practices of the Indians.

The recent publicizing of Chagnon's writings in Brazil through the mass media is a repetition of the same situation. Precisely at this moment the over-9000 Yanomami in Brazil are suffering the effects of an invasion by nearly 20,000 miners in the largest gold rush of Amazonia since Serra Pelada in the State of Pará. At the same time, an interministerial committee is once again carrying out a preliminary survey prior to the definition and demarcation of Yanomami territory. Wide publicity about Yanomami "violence" in racist terms at precisely this time and in this context is being used by the powerful lobby of mining interests as an excuse for the invasion of these Indians' lands. Four Yanomami were already killed by miners in August 1987, not to mention the untold numbers of Yanomami who have died since 1974 as a result of constant epidemics due to invasions by miners, highway workers and colonization projects. It is very difficult to know what is happening in the area now, as researchers, journalists, missionaries and members of support organizations are prohibited from going into Yanomami territory by Funai and the military.

To conclude, the Brazilian Anthropological Association (ABA) feels that it is fundamental to insist on the need to bring to the awareness of North American anthropologists the political consequences of the academic images they build about the peoples they study. The case of the Yanomami in Brazil, who have been suffering a brutal process of land expropriation which is justified in discriminatory images based on dubious scientific conclusions, are in this respect a particularly grave and revealing case. Since 1979, the American Anthropological Association has taken an active role in the international concern for the rights of the Yanomami people, through resolutions and as a cosigner of a complaint to the Organization of American States in 1980-81. We urge the AAA to take the necessary steps to call to the attention of the North American anthropological community the ethical and moral repercussions of their writings for critical situations such as this.

Maria Manuela Carneiro da Cunha
Past President, Brazilian Anthropological Association

To the Editor:

Most of the ABA criticisms of my work in general and my recent *Science* article in particular fall into four broad categories: (1) sensational press coverage of science reports, (2) the accuracy of my ethnographic portrayal of the Yanomamö, (3) the use of biological theory in explanations of human behavior, and (4) my alleged complicity in Yanomamö genocide. I will address only these and end with (5) a comment on the AAA policy of "reciprocity."

1. I agree that some members of the press wrote stories based on my *Science* article that

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deserve condemnation for their senseless, inaccurate and irresponsible portrayal of the Yanomamö. I cannot control what journalists say about my published scientific works. Perhaps the most useful outcome of this exchange will be to sensitize the press to important issues. Freedom of the press has costs and benefits. One draconian way to handle the press would be to advocate censorship, which none of us would be willing to consider seriously. The other side of the coin is that journalists can be very helpful in publicizing native problems, and we should encourage them to do so. Boyce Rensberger of the *Washington Post*, criticized in the ABA document, carried a very sympathetic front page story on the plight of the Brazilian Yanomamö (April 4, 1988) and is to be congratulated for this. Those of us interested in native rights must rely on the press. We should try to make journalists more aware of the kinds of issues raised by this element of the ABA document. My agreement with the ABA document ends here.

2. I have spent a considerable amount of time trying to put Yanomamö warfare and aggressiveness into a global and historical perspective. I have never claimed that they are the most warlike or violent people on earth, despite what some journalists assert. What little quantitative ethnographic data we have on comparable peoples is that the Yanomamö have moderate levels of mortality due to violence. I have also consistently argued that their military patterns should be viewed as we view our own: that defending one's kin group and culture with forceful means is a common attribute of all sovereign people, isolated tribesmen as well as citizens of large nation-states.

The charge that I have manipulated my data on causes of death to exaggerate the importance of violence is *ad hominem*. My published data clearly show that diseases are the primary cause of death in the Yanomamö population. The authors of the ABA document are manipulating my published data on Yanomamö mortality for their own purposes. In fact, the most reliable large source of data on Yanomamö mortality due to diseases comes from my published works, and it is primarily because of my fieldwork that we know how significant disease is in their demographic profile. My statistics on mortality due to violence take into consideration diseases, some of them recently introduced. If these introduced diseases had not occurred, the rates of mortality due to violence under "aboriginal" conditions would be very much higher. I have the impression that the ABA authors would like to see me report more mortality due to introduced diseases or, perhaps, only that kind of mortality, concealing other kinds. The assumption seems to be that only then will "good" people want to save them. Deliberately manipulating anthropological evidence, even for good purposes, would probably increase the peril of the Yanomamö. When the falsehoods are discovered, some will argue that anything anthropologists say about them should be dismissed because anthropologists lie. My position is that we should try to "save" them, whatever the reasons for their mortality patterns. I am devoting a significant fraction of my time attempting to help the Venezuelan Yanomamö meet the new challenges that face them, with the support of others. Those of us working together in Venezuela do not feel that the fact that they have the same "defects" we ourselves have disqualifies them from being worthy of our efforts on their behalf.

The ABA claim that the doctoral thesis of one of my former students, T Melancon, contests my findings is ridiculous. The ABA authors have manipulated the facts and are demonstrating their ignorance of statistical procedures. They have deliberately confused my clearly stated distinction between mortality

due to violence among adult males with rates for all age grades and both sexes in the whole population. While Melancon's thesis focused only on a fraction of the villages in which I collected the data, and on a very short time span in the histories of those villages, his calculated rates of violent death among adult males not only are consistent with what I have reported; they are, if anything, slightly higher. Had the authors of the ABA document read Melancon's thesis more carefully, they would have noted (p 45) that he also broke down mortality into sex and age components, showing that 29% of the males 15 years or older and 42% between 15 and 49 years at time of death died violently in the villages on which his thesis is based. My several publications indicate that mortality due to violence among adult males ranges from about 24% to slightly over 30%, depending on the date of the sample and the population under consideration at the time of publication. In my *Science* article, I stated (p 985) "approximately 30% of adult male deaths are due to violence . . ." and repeated this again on p 986. Perhaps the ABA authors should themselves take heed of the message they quote from E R Leach.

Bruce Albert claims that *unokai* status has a different meaning in the part of the Yanomamö area in which he worked, implying that it has only "symbolic" meaning among all Yanomamö. I eliminated "symbolic" *unokai* in my *Science* report. The Yanomamö where I work clearly distinguish between "true" *unokais* (*unokai a yai*) and "false" *unokais* (*unokai horemou*), as I suspect they do where Albert worked. We will probably have to ask Giovanni Saffirio, a Consolata priest and PhD in anthropology (U Pittsburgh, 1985) how many "true" *unokais* there are in the Catrimani area, the mission source of much of Albert's data. Where I have worked, men who deflower prepubescent virgins must *unokai*. I did not count these. Men who *unokai*ed and later learned that the victim recovered were also eliminated from my analysis. All *unokais* in my report were men who delivered intended fatal blows or shot arrows (and, in a few cases, shotgun pellets) into a living, real person who died as a consequence. There are, of course, more "unokai" events than there are victims, which I clearly distinguished in my *Science* article. The ABA suggestion that I cannot tell a "symbolic" from a "real" death is nothing short of silly, which provokes me to treat with considerable suspicion all ethnographic reports of Mr Albert and Ms Ramos, for whom such a distinction appears to be hardly more than an arbitrary desideratum. [A somewhat shorter version of the ABA document was submitted in May 1988, to *Science*, signed by B Albert and A Ramos. The *Science* editors accepted it for publication and sent it to me for comments. The authors then withdrew it.] In my view of anthropology as a science, the ethnographer should attempt to make observations and report facts that can be replicated by another observer. I am confident that facts I report on the basis of some 50-odd months of living in many Yanomamö villages can be verified by any competent scientific field researcher who is willing to spend the time and effort required to check on the accuracy of my reporting. Those interested in a totally independent, nonscientific but highly informative view of the people I studied would do well to read the account of Helena Valero's life among many of the same people. I doubt that she confused symbolic deaths with real ones: she had the advantage of not being anthropologically trained to confuse symbols with the things they stand for. Husiwé (Fusiwe or Husiwé in her accounts), her former Patanowa-teri husband, by the way, is counted in my *Science* article as one of the "real" victims whose body I did not see.

The ABA document argues that since, by my own admission, I did not witness the killings I reported or make "body counts," the violent deaths therefore may, have not actually occurred. I also did not witness the vast majority of deaths that were attributed to "introduced diseases." Nor did I witness the

brutal killing and desecration of the bodies of four Yanomamö men killed in August 1987 by Brazilian *garimpeiros*. But the authors of the ABA document are willing to believe, even enthusiastically, my reports on deaths due to introduced diseases and sensational press reports of the killing of four Yanomamö by *garimpeiros*. This suggests that the authors of the ABA document choose arbitrarily to believe what they want to believe because it is useful for their own purposes, requiring body counts for intratribal violent deaths, but willing to accept just about any form of evidence on violent deaths caused by outsiders or diseases introduced by outsiders.

3. I did not conclude in my *Science* article, or anywhere else, that killing or homicide is biologically advantageous as a general principle in Yanomamö culture, or in all cultures everywhere and at all times. An untimely violent death certainly doesn't do much good for the biological future of the victim. Humans have both a natural history and cultural history, and a scientific understanding of human behavior requires an understanding of both biology and cultural anthropology, regardless of what some schools of thought in anthropology might argue. In my *Science* article I did demonstrate that there was a positive and statistically significant correlation between the male status *unokai* (a ritually purified killer of another real person) and two other variables: (1) marital success and (2) reproductive success. This might be the first time this has been demonstrated for any human population, and it is an important scientific finding. I left open the ultimate explanation for this correlation because, as a scientist, I do not have additional facts that would enable me to conclude that aggressiveness alone leads to reproductive success in this historical-cultural-ecological context. I did suggest a number of possible avenues of further inquiry that might explain this correlation and very cautiously chose words like "speculate," "seem," "appears," etc to let the reader know that I do not have the definitive explanation for this correlation, but did have some informed suggestions. This is a standard procedure in scientific reporting, and scientific readers are normally aware that when a colleague says he is speculating about something suggested by his empirical findings he is not suggesting that his findings are simply free inventions of his mind. However, it is entirely possible that the single most important variable explaining higher reproductive success among Yanomamö *unokais* is their greater willingness to take mortal risks and demonstrate their aggressiveness, however repugnant this might be to some anthropologists or others who believe, as a matter of faith, in extreme forms of cultural or environmental determinism.

4. The suggestion in the ABA document that I am encouraging or promoting genocide by my ethnographic descriptions of the Yanomamö and my alleged racist manipulations of data on their violence is gratuitous and insulting. It is also libelous. Those of us in the Americas who come from European backgrounds have been systematically causing the extermination or disappearance of native Americans for nearly 500 years. Among large nations in the Americas, Brazil in particular has a sterling track record in this regard, followed closely by my own country. We have accomplished most of it in complete ignorance of anthropology, and we continue to inflict enormous harm on native peoples not only regardless of what anthropologists do or say, but usually in spite of those things anthropologists do or say that are deliberately intended to have the opposite effect. Anthropologists are an easy target, a convenient scapegoat: they "know" about native peoples and are often highly visible. But the power, control and influence attributed to us is mythical: we are generally incapable of having much of an effect when what we are opposing is the spread of a vast, powerful and economically gigantic process. I am struck with the similarity of the Brazilian government's treatment of US anthropologist Darrell Posey and his Kayapo informants and the ABA's accusations against me: both Brazilian groups blame a US anthropologist for their own frustrated attempts to achieve what it is that each is striving for.

5. Despite the disclaimer by the AAA that it does not "endorse" the position of either the ABA or me, this exchange has some serious implications for ethnographic reporting by US researchers working in other countries. The AAA's policy of "reciprocity" (guaranteed publication) to sister AA organizations might be opening the door to an avalanche of complaints that, like this one, are rather more political, not to mention libelous, than they are professional, scientific or ethical. I am astonished that the AAA has accepted for publication in the *AN* an accusation against one of its members, without considering its possible accuracy, that he is (1) falsifying and manipulating data, (2) doing so with a "fidelity" that fosters genocidal practices and (3) implies he is describing the people among whom he has worked in racist terms. This policy, I believe, should be discussed in the *AN* and at a future AAA business meeting. Whence and when did this policy emerge? Are there any guidelines regarding what the AAA guarantees to publish in the *AN* if submitted by a "sister" AA?

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