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On Yanomami Warfare: Rejoinder¹

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In my critique of Chagnon's 1988 article (CA 30: 637-40) I showed that his measure of "the amount of violence in Yanomamö culture" as well as his statement that an alleged status of "killer" ("unokai") promotes higher male reproductive success are questionable on both ethnographic and theoretical grounds. I also traced the historical roots of the cultural assumptions that underlie his image of the Yanomami as "fierce people." Since his only comment on this point is that I find Darwinian theory "repulsive" (CA 31:51) I will let it go at that² and examine his other assertions (pp. 49-53).

1. Several months of work with physicians among the Yanomami of Brazil have prevented me from writing this rejoinder sooner. I am grateful to D. Buchillet, P. Menget, A. Ramos, and A. Quesnel for their helpful comments.

2. Except to note that on a recent rereading of *Leviathan* I have found that Chagnon is closer to Hobbes than I had suggested (CA 30:639 n. 10): "It may peradventure be thought, there was never such a time, nor condition of Warre as this; and I believe it was never generally so, over all the world; but there are many places, where they live so now. For the savage people in many places of America, except the government of small Families, the concord whereof dependeth on natural lust, have no government at all; and live at this day in that brutish manner, as I said before" (Hobbes 1988[1651]:187).

1. *Warfare and interethnic contact*. I showed that the Shamatari Yanomami of the Mavaca-Siapa region (Orinoco River basin Venezuela) studied by Chagnon present a much higher percentage of male mortality in warfare than any other Yanomami subgroup on which such data are available and that this percentage is smaller than those for other Amazonian groups³ whose ethnographic images are much less conspicuously warlike. Stressing Shamatari specificity (which Chagnon once emphasized and now plays down [see 1974:chap. 4 and 1988:991 n. 21]), I stated that it may be related to early historical changes introduced in this population well before "first contacts" with whites through direct or indirect contact with surrounding indigenous groups (see Posey 1987). This is the only part of my argument to which Chagnon objects,⁴ accusing me of "invoking unknown and undocumentable factors such as mysterious effects of a 'population explosion' or the alleged hostility of unknown Arawaks who lived in this region before the Yanomamö penetrated it" (p. 51). Ironically, my "invocation" of these "undocumentable factors" was based on Chagnon's (1966:167) own attribution of the Shamatari's demographic expansion to their early acquisition of steel tools and their free access to open territories (on the Shamatari's higher rate of population growth, fission, and warfare than their Namoweiteri neighbours, see Chagnon 1974:129-32). As a matter of fact, although ethnohistorical research on the Yanomami is still incipient, it has been established that, during the 19th century, in several regions they were at war or trading—often to acquire steel tools—with various neighbouring Caribs, Arawaks, and other groups whose territory they now occupy in whole or in part (see Albert 1985:40-42). As for the "unknown Arawaks," the record shows that the Yanomami who preceded the Shamatari in the Mavaca-Siapa drainage (see CA 30:638 n. 5) were engaged in fighting with the Arawak-speaking Anauyá and Kuriobana (see Lopes de Araujo 1884:54, 56; Cerqueira 1928:78-79; Stradelli 1889:23; Chaffanjon 1889:247, 292, 295).⁵ It is also known that the Baré of the Cassiquiare Canal used to take captives from among these Yanomami (see Spruce 1908:316, 356) and that the Man-

3. Waorani and Achuará—to which we could add the Mekranoti Kayapo, with pre-contact male mortality in warfare calculated as 42% (Werner 1983:24, table 7).

4. In a footnote (n. 7) Chagnon attempts to support his assertion of "the signal importance of violence as a determining factor" in Yanomami culture by mentioning Alès (1984) on the Parima highland Yanomami, but Alès explicitly disagrees with his utilitarian conceptions about competition over women and the related theory of Yanomami warfare (see pp. 97-99 and p. 111 n. 1). Moreover, Smole's data on the Parima highlands (1968-70) suggest a low warfare intensity in that area (1976:74, 233 n. 105).

5. During the 19th century another Arawak group, the Mandawaca, also occupied the Siapa (Schomburgk 1841:249) and Pacimoni (Spruce 1908:427) Rivers, while the Mawaka, also Arawakan, were on the Mavaca River (Spruce 1908:408). Humboldt (1819:572) encountered Yanomami at the colonial settlement of La Esmeralda (upper Orinoco) in 1800, and in 1857 Michelena y Rojas (1867:354) heard from acculturated Indians of Santa Isabel de Mavaca (upper Orinoco) that they used to trade with the Yanomami (known as Guaicás or Guaharibos).

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dawaka and Yabaana (also Arawakan) were expelled by the Yanomami from the Cauaboris and Marauí Rivers (upper Rio Negro, Brazil) at the beginning of the 20th century (Knobloch 1975:143-44). Finally, two recent studies have provided persuasive data and discussions on the influence (ancient and/or recent) of techno-economic change and contact on warfare intensity among the Yanomami (see Colchester n.d., Ferguson 1989b). All this evidence seriously challenges Chagnon's rendition of the Yanomami he studied as an isolated group retaining pristine patterns of warfare (1983[1968]:1; see Headland and Reid 1989 on the anthropological attachment to the representation of small-scale societies as pristine isolates).

2. *Unokaimu ritual and "killers" record.* I objected to Chagnon's taking the percentage (44%) of males 25 years or older who had performed the *unokaimu* ritual⁶ as a measure of "the amount of violence in Yanomamö culture" (1988:989) on two grounds, one empirical and the other logical. I noted that the accuracy of his record of men who "have participated in the killing of someone" ("*unokais*") might have been impaired by the distorted way in which he used the *unokai* category and then pointed out that "participation" in a killing in this context cannot be lumped with the Western notion of homicide. Chagnon's counterarguments regarding my first objection transform it into a methodological straw man. He attributes to me several fictitious assumptions about his research in order to contrast them with his "real" procedures of recording victims and "killers." My criticism was centered on the misuse of the *unokai* ritual category, and the only assumption underlying it was, of course, that he must have used that category in his inquiry. His 1988 paper indicates as much when it states that the "*unokais*" are "widely known within the village and in most neighbouring villages" (p. 987). In a subsequent publication he says that he "eliminated 'symbolic' *unokai*" from his record of "killers" through employment of an alleged native distinction between "false" and "true" "*unokais*" (1989a:24), again implying that he used that concept. Now (p. 50) he suggests that he never employed it except to confirm the obvious, i.e., that the "killers" he identified (with what concept but *unokai*?) had undergone the *unokai* condition.

To my second objection, that is, the validity of his "*unokais*" record for "measuring" Yanomami "violence" in a comparative perspective, Chagnon has no response. Yanomami killings in warfare include collective arrow shooting of the same victims in combat⁷ and subsequent wounding of dying or even dead victims (see also Lizot 1989:109). All warriors who have injured an enemy in these different ways consider themselves to be in the state of *unokai* (see Albert 1985: chap. 11). Can a

record of these "*unokaied*" men be equated with statistics on homicides in the Western sense of the term (see Knauft 1987:463)? It is doubtful, and that is my point.

3. *Raiding and abduction of women.* In questioning Chagnon's speculation that the abduction of women by "*unokaied*" warriors might enhance their reproductive success, I quoted the only quantitative data available at the moment on marriages by abduction in the context of Yanomami warfare—the figure of 0.8% calculated for a region very close to Chagnon's research site (see Lizot 1988:540-41). I could have added that in the Parima highlands, adjacent to Lizot's area of study and taken to be the historical center of Yanomami territory, abduction in warfare is said to be practically nonexistent (Smole 1976:230 n. 22; Alès 1984:97). Chagnon claims (p. 51) never to have said that Yanomami raids begin *exclusively* as fights over women or are *exclusively* motivated by abduction strategies. Nevertheless, many of his descriptions of Yanomami conflicts over women come very close to this:⁸

... most drastically, the woman shortage is remedied by raiding other villages to abduct women. [1966:69]

... most Yanomamö warfare and intra-village fighting is directly attributable to competition over women. [1972:274]

... wars between villages usually begin in a contest over the possession of some woman. [1976:17]

... much of the fighting ... is explained, by the Yanomamö, as attempts to get revenge. Still, the wars almost invariably begin in dispute over women. — [1979:87 n. 2]

Most fights begin over sexual issues: ... The most common explanation given for raids ... is revenge ... and the most common explanation for the initial cause of the fighting is "women." [1988:986]

He goes on to raise doubts about the accuracy of Lizot's data, despite the fact that he has made favorable remarks on Lizot's data collecting and on the collaborative work they developed together (see Chagnon 1975:109 n.2; 1976:14-15).

Finally addressing my point, Chagnon argues first that to calculate a percentage of abductions only on the basis of current marriages underestimates their rate and that Lizot's data are irrelevant for his research area. I agree that the rate of abductions in current marriages is not a satisfactory index, but, although not ideal, it is something that Chagnon has never provided. He would be the one to provide quantitative data to support his assertion on the marital success of Yanomami "killers" through mate abduction, and until my 1989 CA criticism this does not seem to have crossed his mind.⁹

8. Even his more nuanced formulation of the cause of Yanomami warfare stresses that "although few raids are initiated solely with the intention of capturing women ... once raiding has begun between two villages ... the raiders all hope to acquire women" and that "the Yanomamö themselves regard fights over women as the primary causes of their wars" (Chagnon 1983[1968]:175-76).

9. Similarly, for over ten years he insisted on a link between Yanomami warfare and female selective infanticide without giving

⁶ *Unokai* is a condition of symbolic impurity resulting from the physical or supernatural killing of an enemy; *unokaimu* is the ritual that neutralizes it (see Albert 1985: chaps. 7-11).

⁷ Forty-eight percent of the victims recorded in Chagnon's data were killed by more than one warrior and 24% by 3 to 15 warriors (p. 50, fig. 1).

Challenged by my criticism, Chagnon comes up with a (preliminary) rate of marriage by abduction in his area of study: 17%. Unfortunately, he does not distinguish abduction from enemy villages and from allied villages as Lizot did (0.8% and 0.9%), although he notes that "most abductions are not the consequence of raiding" (p. 51). Two conclusions can be drawn from this: that the rate of abduction of women in warfare in his area of study is low, as in Lizot's research area and in the Parima highlands, and that his 1988 hypothesis is that "unokaid" warrior might gain reproductive success not by taking captives on raids from their enemies but by securing women from their allies. These points modify his portrayal of Yanomami warfare.

Nevertheless, the discrepancy in levels of abduction of women from allied villages between Lizot's and Chagnon's data remains a puzzle. In view of the differences between the Shamadari and their Namoweiteri neighbours (Chagnon 1974:127-32), whose populations were pooled in Chagnon's data base, it might be interesting to check whether this rate is characteristic of both "population blocs" or specific to the Shamadari. At any rate, Chagnon has not yet provided any statistical evidence of the gains "killers" ("unokais") may have in marital and reproductive success through the abduction of women, whether from allied or enemy villages.

4. *Unokai and "cultural success."* Pointing out the precariousness of his ethnographic evidence, I challenged Chagnon's postulation of a prestigious status for warfare "killers"/"unokais" (see also Chagnon 1990:95) that, as an indicator of cultural success, would confer upon them a special "attractiveness" as mates in marriage arrangements. Chagnon's reply to this is anecdotal except for the statement that he is "aware of other meanings and nuances" that the concepts of *unokai* and *waitheri* "have in other contexts" but his article "was not intended as a contribution to semantics or lexicography" (p. 50). The issue at stake in my comments on these categories is, of course, more serious than this.

We have seen that, because of the organization and ritual context of Yanomami raiding, many men may very well undergo the ritual condition of *unokai* without having manifested bravery. Only a few men achieve a supralocal reputation for valour in intervillage raiding and are said to be *waitheri*.¹⁰ Having undergone the condition of *unokai* repeatedly is one of the qualifications for this reputation.¹¹ Chagnon recognizes this: "Most

killers have unokaid once. Some, however, have a deserved reputation for being *waitheri* (fierce) and have participated in many killings" (1988:987). But this nuance complicates his argument. If we consider the *waitheri* reputation based on multiple *unokai* experiences rather than a hypothetical "unokai status" as an indicator of cultural success, then for his record of "killers" to establish a link between prestige and reproductive success he would have to limit it to multi-"unokaid" men; only 21% of the men (29 of 137) in his *unokai* count have undergone this ritual condition three times or more (1988:986, fig. 1). Moreover, he has recently suggested that men who engage in lethal violence "with some moderation" do better reproductively than people who "are excessively prone" to it (1989b:566). The hypothesis thus becomes even more difficult to handle: its universe, initially all "unokaid" men, has had to be reduced first to multi-"unokaid"/*waitheri* men and now to "moderately" multi-"unokaid"/*waitheri* men. The problem, of course, is how to draw the lines between these categories.

Chagnon has presented no proof of the existence of a causal relationship between a "status achieved via . . . prowess in military activities" (p. 53) and reproductive advantage. At best, he offers an apparent statistical correlation between participation to any degree in warfare killings—mostly by occasional warriors without special prestige—and higher marital/reproductive performance (1988:989, tables 2 and 3). As he himself has put it, he has "only speculated" on the mechanism that might connect these phenomena (1988:989) that, to him, "seem to be correlated" (p. 53). This correlation may well be due to confounding variables. Chagnon has discussed some possibilities (1988: 989-90), and so has Ferguson in his criticism of that paper (1989b). At least one has not yet been satisfactorily resolved: that the "apparent higher fertility" of "unokais" might be "achieved at the expense of higher mortality rates" (Chagnon 1988:990; see also Ferguson 1989a:564; Chagnon 1989b:566). Chagnon raises this question only to reject it on the basis either of anecdotal information (1988:990) or of "impressions" (1989b:566). No conclusive evidence has yet been provided. This is a crucial question in view of the fact that many Yanomami ethnographers (including Chagnon) have stressed that men taken to be responsible for deaths in warfare (especially the multi-"unokaid" warriors) are preferred targets for revenge raids (Albert 1985:305; Alès 1984:108; Chagnon 1983[1968]:180; 1988:985; Lizot 1989:104).

If there is any correlation between warrior reputation and reproductive advantage left after the ethnographic and methodological shortcomings pointed out so far have been resolved, Chagnon will still have to provide a satisfactorily deductive model of causality linking these two orders of phenomena. Such a model will of course have to be built on more than rhetoric—for example,

any statistical evidence for it. He finally abandoned this assertion [see Chagnon, Flinn, and Melancon 1979:301-3, 308-10 and Melancon 1982:227].

10. Contradicting himself about its prestige, Chagnon stresses the (secondary) negative sense ("aggressive," "wild") that *waitheri* has in some contexts (see Migliazza 1972:421-22; Lizot 1989:107).

11. Bravery is a necessary but not a sufficient condition for being considered *waitheri*, as this concept also connotes, outside the raiding context, being daring and tough and having authority, initiative, and a generous and humorous temperament [see Albert 1985:97-98]. No special status is accorded *waitheri* men either in my area of study or near where Chagnon worked [see Alès 1984:108; Lizot 1989:104]. *Waitheri*, in the sense of "resistant to

pain" or "ready to defend oneself," is also applied to women and children [see Bortoli 1983:18; Lizot 1989:107].

assertions about "tribal violence," cross-cultural military prestige, or female preference for male "winners." To explain Yanomami warfare, it will also have to be freed from what Bateson called "the old teleological trick" (see Houseman and Severi 1986:8), that is, the functionalist confusion of effects with transcendental final causes. Chagnon will therefore need to establish the mechanisms through which reproductive strategies may lead to warfare among the Yanomami and not the other way round (see Robarchek 1989:906). Then the challenge for him will be to make sense of his model in genetic terms (see Morange 1986:133-34 and Weiss 1976:369-70).¹²

4. "*Tribal violence*" as an ethnocentric construct. There is, of course, no such thing as violence per se: the characterization of any social interaction as "violence" is culture-bound (see Jamin and Lenclud 1984:9-10, 17-18; Michaud 1988:chap. 1). The Western notion of violence, rather than defining a cross-culturally identifiable unitary class of phenomena, covers a wide and varied range of behaviours and representations. The semantic root (*vis*) of the Latin term *violencia* refers to the manifestation of physical force (the Yanomami see *waitheri* as an energetic emanation of the vital principle of animated beings [see Albert 1985:147]). The use of this force against someone constitutes "violence" when there is transgression of a social norm (this notion goes back to the late 18th century [see Domenach 1980:32]). There are, thus, as many forms of violence as there are norms to define it (on the complexity of Western juridical and statistical classifications of types of violence, see Chesnais 1981:12-14; Michaud 1988:chap. 2). The Western notion of violence, being inseparable from the idea of transgression, when applied elsewhere inevitably conveys an ethnocentric normative referent.

From this perspective, claims to measure "violence" in "tribal societies" as if it were a universally defined phenomenon amount to reifying the outcome of a culturally biased assessment. It is obvious, then, that, no matter how objective the statistics of "violence" may seem to be, they cannot be completely free of implicit value judgements. Even the cross-cultural use of the classic homicide rate per 100,000 is problematical because of the cultural diversity of types of killing and of subsets of the population involved (see Knauft 1987:463

for a discussion of some aspects of these difficulties). There is no neutrality, for example, in comparing Yanomami rates of violent death, mostly due to intervillage raiding (collective/legitimate violence), with Western rates of criminal homicide (individual/illegitimate violence). Nor would mortality in warfare be easily comparable, the subpopulations at risk being defined by different social and cultural criteria. Yanomami intervillage raiding avoids, for example, women and children as targets and aims particularly at multi-"*unokaied*" men.¹³ Things are, of course, very different in the context of Western high-technology warfare.

Chagnon has published indices that are supposed to measure Yanomami "violence." His imposing percentages give the impression that this is indeed a violent society. But on what basis is this impression created if none of the indicators used are cross-culturally comparable?

5. *Cultural representations and the anthropology of war*. As a social institution warfare presupposes a culturally defined interdependence among politically organized collectivities (see Aron 1984[1962]; Sahlins 1980a:32-37). The minimal and widely accepted definition of warfare as "armed conflict between political units" is based on these premises (see Aron 1984[1962]:34, 326; Otterbein 1973:923-24; McCauley 1990:1-2). Warfare cannot be reduced to a mere resultant of individual aggressive behaviours and utilitarian strategies unless we are prepared to deny it as a social institution (contrast Chagnon 1974:77 and 1990:79-80, 85-86). Seen in this light, Yanomami intervillage raiding (*niyayu* 'to shoot arrows at each other') is to be understood in the context of a complex cultural construct consisting of a classification of sociopolitical distances, a theory of physical and supernatural aggression, and a system of symbolic exchanges via funerary and war rituals (see Albert 1985; 1988:89-93; n.d.).

Since his Ph.D. dissertation, Chagnon has given minimal ethnographic attention to the intellectual and ritual aspects of Yanomami warfare, treating them as if they were exotic epiphenomena (see 1967:27; 1968:136-37; 1983 [1968]:181-82, 186). With his new biological reductionist approach this disregard for warfare symbolism has been elevated to a theoretical premise. To explain Yanomami raiding in terms of universal male competition over "means of reproduction" (Chagnon 1990:81) is to deny by definition any relevance to the complex framework of indigenous representations and institutions that informs the armed action. This neo-functional approach precludes any comparative perspective on the cultural diversity of warfare (cf. Menget 1985-86).

Indigenous theories of aggression and revenge, conceptions of social difference, and the associated rituals are

12. The remainder of Chagnon's argument is a challenge to me to provide a non-Darwinian explanation of his data on higher variance in male fertility and "competition over females." This challenge is, of course, rhetorical, as it adds no supporting evidence to his 1988 hypothesis. Moreover, male differential fertility results from several factors among which polygyny is not necessarily dominant (see Howell 1979:267-73). It is not, therefore, a priori an indicator of "competition over females." Chagnon has yet to provide an in-depth demographic analysis of male fertility and polygamy from his complete data base (contrast Howell 1979:chap. 13). The two figures presented (p. 52) show a higher fertility variance among males, although inflated to some extent by two men with 23 offspring each, than among females. These figures compare men and women 40 or older living in 1988. Such a comparison seems questionable in that it does not deal with completed reproductive careers and does not take into account the differential mortality between men and women.

13. Percentages of adult female mortality in warfare vary considerably among Amazonian societies: Shamatari 4%, Namoweiteri 7% (after Chagnon 1974:160, table 4.10), Kayapo 23% (Werner 1983:241, table 7), Achuará 27% (Ross 1988:57), Waorani over 50% (after Larrick et al. 1979:167, table 7).

essential constitutive dimensions of warfare as a social institution, regardless of military action as such and the external constraints that may shape its intensity and organization. These ideational and institutional constructs have their own ontological autonomy and cannot be reduced to mere by-products of another order of reality.¹⁴ This does not mean that reproductive, environmental, and contact-induced factors have no effects on various aspects of warfare; it means that these effects manifest themselves through the cultural framework that constitutes warfare in a given society and do not cause it.

To neglect the ideational/ritual dimension of warfare amounts to excluding a priori most of the social reality to be analysed, thus inevitably leading to impoverished (unidimensional) models of explanation. How can an anthropological analysis of warfare omit the symbolic relationship between raiding and supernatural aggression when accusations of killing by sorcery are so important in triggering cycles of intervillage raiding (see Albert 1985:302-4; Alès 1984:99; Chagnon 1968:112; 1983[1968]:170, 175-76; Lizot 1989:106; Peters 1987:81; Smole 1976:50)? How can intervillage raiding be understood apart from its ritual context when the launching of any raid involves an endocannibalistic funerary rite for a victim to be avenged and the ritual simulation of the exocannibalism of the enemies responsible for that death (see Albert 1985:353-60)? The task for a non-reductionist anthropology of warfare is thus to unravel the specific cultural constructs of that phenomenon before analysing the non-cultural factors that may be involved in actual wars.

Chagnon's biological determinism is incapable of accounting for the multidimensional complexity (and regional diversity) of Yanomami warfare in itself or in a comparative perspective. Reducing Yanomami intervillage raiding (like all "primitive warfare") to a biogenetic process—the pursuit of inclusive fitness—is simply explaining it away with an all-purpose hypothesis that dissolves its cultural content into an alleged final genetic imperative. Eliminating the fundamental issue of human cultural variability and reproduction, this reductionism annuls the very object of anthropology (see Sahlins 1980a:chap. 1; 1980b:99-120) and contributes little to the study of the complex specificities and interactions of biological and cultural processes (see Lévi-Strauss 1981, Morange 1986, Paul 1987, Rogers 1988).

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14. This conception is not incompatible with a materialist ontology of cultural representations: see Sperber (1987).

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On Structure and Entropy: Theoretical Pastiche and the Contradictions of "Structuralism"

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"Structuralism," as a form of anthropological thought preeminently associated with the work of Lévi-Strauss, is generally conceived to be founded upon a concept of "structure" imported in more or less equal parts from linguistics and mathematics. The linguistic ideas from which Lévi-Strauss drew inspiration are by now relatively familiar. The mathematical side of the story, however, has been much less well understood. This lack has now been remedied by Almeida's (CA 31:366-77) masterful exposition of the mathematical ideas upon which Lévi-Strauss appears, explicitly or implicitly, to have drawn: group theory with its concepts of group, transformation, and invariance and notions of topology, probability, and entropy derived from other forms of mathematics.

The concept of structure in terms of invariant constraints governing transformations is of course not confined to group theory or to mathematics; it is fundamental to any form of structural analysis. Lévi-Strauss was not the first to apply it to the analysis of human phenomena: Marx's analysis of the transformation of values into prices of production in volume 3 of *Capital* has exactly this form, and many other examples could be cited, among the more notable from the works of Lévi-Strauss's contemporaries Bateson and Piaget. Lévi-Strauss's idiosyncratic manner of applying this concept to anthropological data, however, sharply differentiates him from these thinkers and others who have applied it to social, cultural, and psychological data. This idiosyncratic approach is the result of his attempt to synthesize the concept of the transformation group with the basic ideas of Saussurean linguistics and Prague phonology.

Almeida's article will be of great value to those wish-