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POPULATION, RESOURCES AND WARFARE AMONG THE YANOMAMÍ*

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The possibilities available in the Amazonian forests to the human groups which live there are still poorly known. It has been wrongly supposed, and without taking into account regional variations, that the game resources are limited and the protein supply restricted.

The present article demonstrates, with reference to the Yanomamí, that the diet is balanced and that the growth in population has never broken the equilibrium between man and the environment. Variations in the level of consumption between different communities depend more on the attitude towards work than on the supply of game. Food requirements are easily satisfied by a very moderate amount of work.

The author rejects the interpretation of Yanomamí behaviour given by Marvin Harris, and he presents facts and figures which invalidate the thesis that the aim of warfare is the expropriation of hunting territories.

I

When ecology is given undue emphasis, social anthropology tends to lose a sense of proportion and forgets that various factors combine dialectically to make a society what it is: the natural environment and resources, to be sure, but also the history, the internal dynamics specific to each civilisation—technological innovation or borrowing, etc. . . . One runs the risk of mistaking effect for cause, of deliberately or unknowingly distorting the social facts while collecting them and when interpreting them in order to fit them into pre-established and simplistic frameworks. Some of these bad habits are quite apparent in recent publications where Marvin Harris and Daniel Gross offer an explanation of warfare among the Yanomamí; this is the case to an extreme degree in the former's work, in a more refined and cautious manner in the latter's.¹

In spite of Gross's (1975: 544) denial of the fact Harris's basic argument is as follows: the reason why warfare and violence are so intensive and cruel among the Yanomamí is that their diet is deficient in protein and different communities are competing for the hunting territories.² In order to establish this hypothesis Harris finds himself misinterpreting the ethnographic facts: he exaggerates the intensity of warfare and violence, he overestimates the actual population density; he misjudges the role of women, depicting them as the eager slaves of men, overcome by brutality, subservient and passive in everything; he completely ignores the possibility of a causal connexion between violence and ideology and insists on mistaking effect for cause. I do not really know what has motivated such an attitude and approach, but on the whole I suspect something of a primary materialism not lacking a certain subtlety: it is a case of ideological prejudice, which is distorting and simplistic.

* Translated from the French by Suzanne Hoelgaard.

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In this article I intend to take up some of the questions broached by Harris in order to place the problem in its proper perspective; I wish to reassess the recent history of the Yanomami and certain aspects of their population, the importance of violence and warfare, animal protein resources and hunting.

For Harris, the history of the Yanomami, about which he seems better informed than the specialists themselves, can be summarised as follows:

Their ancestors were nomadic hunters and gatherers living away from the main rivers in small scattered bands that relied on wild forest products for their chief source of subsistence. We can be certain that only in more or less recent times did they begin to depend on bananas and plantains as their staple foods, since these plants were brought to the New World by Portuguese and Spanish settlers (1974: 100–1).

He even goes as far as to specify:

Let me give a somewhat speculative account of recent Yanomamo history. The nomadic Yanomamo who lived in the remote mountains between Venezuela and Brazil began to experiment with banana and plantain gardens. These crops yielded a great increase in the amount of food calories per capita. As a result the population of the Yanomamo also began to increase . . . (1974: 101–2).

Speculating on the possible reasons for the Yanomami population explosion, Harris asserts that it results from a change in the mode of production, in particular the introduction of agriculture and intensive banana and plantain cultivation. According to him the introduction of metal axes and machetes may have led to the adoption of agriculture, or at least to the development thereof; and Harris quotes a text by D. de Basandiaran according to whom it would have taken several days to cut down a single tree before metal axes were introduced.³ Then he adds:

Another factor promoting the intensification of banana and plantain gardens may have been the pacification and/or the extinction of the Arawak and Carib groups who previously dominated all the navigable rivers in yanomamoland (n.d.: 14).

All this is disputable and very ill-informed, not to mention the distortion in interpretation, and it is essential to reconsider each of the questions raised.

As there are no existing records whatsoever, the distant past of the Yanomami remains something of a mystery; their linguistic affiliation has not yet been determined, and we have to admit we are left with mere hypotheses. Still, there are a few scraps of evidence: about one hundred years ago, when the central Yanomami occupied the Sierra de Parima—which was the geographical centre of their subsequent expansion—they were already horticulturists and there is no reason whatever to believe that they had not been so for a very long time; quite the contrary, their knowledge, their practices and traditions point to its early origin; finally, it is not irrelevant to recall here that agriculture appeared in the Amazon basin several thousand years ago, probably with manioc cultivation. On several occasions we have disputed the theory that the Yanomami were hunters and gatherers, who have recently been converted to agriculture⁴; an interpretation devoid of empirical foundations. The travellers who saw the Yanomami for the first time at the beginning of this century in Roraima territory (Brazil) seem to have encountered them in a nomadic phase or while they were on hunting expeditions, far from their usual settlements and gardens, and their testimony is not

decisive; perhaps that is where the myth of the hunting and gathering Yanomami comes from, which survives in spite of the collected data which entirely refute this interpretation. As to the problem of the origin of the banana, let us humbly confess that it has not been resolved with any certainty and its South-American origin, or pre-Columbian diffusion cannot be excluded *a priori*. Whatever the case may be, whether or not the banana has recently been introduced into South America (i.e. in post-Columbian times), it tells us nothing about the age and origin of Yanomami agriculture; intensive banana cultivation may have appeared late among a people, already practising agriculture, who adopted it as the main cultigen in place of other plants (manioc or maize).

The intermittent nomadism practised by the Yanomami has to be placed in complementary relation within the overall economic system, including agriculture and hunting (fishing). There is no reason not to consider subsistence economics as homogeneous wholes, rather than isolating one factor of production from the rest to discover whether or not it is prior to these. The low protein content of bananas and manioc is well known (cf. Gross 1975: 527–8). By exploiting the surrounding forest, and by gathering wild fruits many of which have a high protein content, the Yanomami improve the quality of their diet.

II

Having clarified these points, it is difficult to retrace the history of the Yanomami further back than a century and a half without indulging in more or less risky speculations. However, in order to reply to Marvin Harris, I shall offer my version of their history, as it seems to me that the ethnographic context has been a more faithful guide to me than to my New York colleague. Incidentally, the linguist Migliazza has made an interesting discovery which will help me; in his thesis (1972), he has pointed out that the toponymy of the lower Rio Branco was, or might have been, of Yanomami origin. This fact remains to be considered and verified scrupulously, but it is significant; a rapid and necessarily superficial investigation of this toponymy on ordinary maps would appear to confirm Migliazza's findings. Formerly the Yanomami occupied a territory bordering on the main tributaries of the Upper Amazon, where the Rio Negro and Rio Branco meet. They may have clashed with Arawak warriors inhabiting the region and been driven out and they may also have retreated before slave hunters who were operating in the area after the European conquest. Whatever the case may have been, the Yanomami found refuge in the mountainous region called Sierra de Parima, which marks the border between Brazil and Venezuela. It is inconceivable that they should not already have been engaged in agriculture following their contact with the civilisations along the upper reaches of the Amazon or in the course of previous peregrinations. They did know how to make pottery⁵ and they used stone axes. The choice of sites for making their gardens must have been more important than later when they became equipped with metal tools: at any rate the introduction of axes and machetes took place at a later date (around the 1920's), occurring a good while after the demographic and territorial expansion had begun, and it was in no way decisive. Although a certain number of large trees were acceptable in the garden, too many were not, and fire was used to eliminate those

that were too thick to be cut down with stone axes alone. It is certain that the practice of agriculture was more laborious then and that the plots were smaller, but these were difficulties which the Yanomami shared with all the South American peoples before the advent of steel tools, so that is not as unusual as we are led to believe.

Isolated in mountains inaccessible at the time, the Yanomami were able to adapt to their new environment, perhaps by developing banana cultivation, perhaps also by emphasising somewhat the bent towards nomadism characteristic of many South American peoples. In the healthy mountain region they found a new demographic balance, protected as they were from epidemics which decimated the Indians elsewhere; this balance was followed by a strong population increase and concomitant territorial expansion. This territorial expansion was facilitated, perhaps even prompted, by the presence of vast uninhabited areas, where the population had long since vanished.⁶

It is not inconceivable that the Yanomami were skilled navigators in the past, but that after settling in the Parima mountains they necessarily lost this skill; there the valleys are narrow and deep, the rivers are torrents congested with rocks battered by the foaming waters and there are many falls: any navigation is practically impossible. Around the year 1950, when the Yanomami occupied—or reoccupied—flat, open country with tranquil rivers they had lost all knowledge of navigation and were only able to make foot bridges for crossing the rivers, or let themselves float along the water on rafts. It is not possible to learn overnight how to carve monoxyle canoes. Marvin Harris is mistaken when he says:

They do not know how to construct or paddle canoes, although their settlements are now on the banks of or close by the Orinoco and Mavaca rivers. They do little fishing, although such waters are usually rich in fish and aquatic animals (1974: 101).

Today, less than one tenth of the total Yanomami population dwells along the banks of navigable rivers; the communities which have settled there have adapted in varying degrees to their new habitat, but the Indians on the whole know how to handle boats, some carve paddles, and others are already able to make boats. Some Yanomami along the rio Padamo have become as skilled in canoe building as their Ye'kwana teachers.⁷ As for fishing, the Yanomami have been doing it since time began; they have a good knowledge of fish poisons;⁸ formerly they knew how to make fish-hooks from bone, and when the water was low, they shot the fish with arrows. They went into the swamps and caught fish in nets made from plantfibre or in baskets. Today they have adopted nylon lines and metal hooks, practise fishing with floats and are just as skilled as others in exploiting the rivers.

A number of specialists have established a hypothetical account of Amerindian peoples' history which entirely corroborates my own speculative reconstruction. Thus, Donald W. Lathrap, in his description of the past history of the inhabitants of the tropical forest uplands and areas lying between the main waterways, recalls two theories: one considers Indians there to be backward representatives of a remote and primitive stage of the tropical forest culture; the other suggests that in most cases it is a matter of simpler, less sedentary groups than those who occupy the banks of the main rivers, the degraded representatives of peoples who at a certain time had a more advanced culture (1968: 25).

Rejecting these two interpretations, Lathrap writes:

The more astute students of tropical forest culture . . . have all observed that tropical forest culture is less an adaptation to the forest as such, than to the riverine environment within the tropical forest (1968: 26).

He then posits this remarkable conclusion:

Most of the primitive groups inhabiting the tropical forest uplands away from the major flood plains can be interpreted as the wreckage of evolved agricultural societies forced into an environment unsuitable to the basic economic pattern. Deprived of the riverine resources, such groups had to rely on the hunting of forest game to provide the protein and fat essential to the diet. A more intense orientation to hunting the relatively scarce game led to more nomadism, a decline in agriculture productivity, and a still greater dependence on wild food (1968: 29).

Leaving aside the kind of implicit value judgement in these lines let us only consider the plausible historical outline, and thus: the type of agriculture practised by the Yanomami, their relative nomadism, their dependency on wild fruits, these are the best adaptations to their present environment. Lathrap's contribution raises other problems which will be discussed in the following pages: it is not certain that game is always all that scarce in the forest and unless formal proof can be adduced, it seems unjustified to say that agricultural productivity decreases when one moves away from the river (bank). To put it briefly, let us say that the opposition between the riparian areas of the main waterways (propitious for agriculture, rich in animal life) and the intermediate zones of the uplands (less favourable for human living) is perhaps not as pronounced as is generally believed: the pedological analysis of the soil, the estimation of garden productivity, the observation of hunting, and the study of diet do not evidence any marked differences.

III

Let us now examine the demographic aspect of Harris's various contributions which relate war to an alleged scarcity of protein resources. The central Yanomami⁹ are composed of two population blocks,¹⁰ distributed in thirty-five distinct local groups derived from two original communities: *husirawë* and *irabraobë*. There are strong indications that these two communities may have once represented one single group (about 120–150 years ago). In 1974, complete demographic statistics were compiled for twenty-five of the local groups and a population count made of the remaining communities. The figures for the total population of the central Yanomami are given in table 1.

TABLE 1. Population of the central Yanomami.

<i>Nature of data</i>	<i>Groups from husirawë</i>	<i>Groups from irabraobë</i>	<i>Total</i>
Population counted	574	759	1333
Population estimated	110	625	735
Total	684	1384	2068

TABLE 2. Distribution of the central Yanomami by sex and age.

DISTRIBUTION OF THE POPULATION OF THE CENTRAL YANOMAMI BY SEX AND AGE

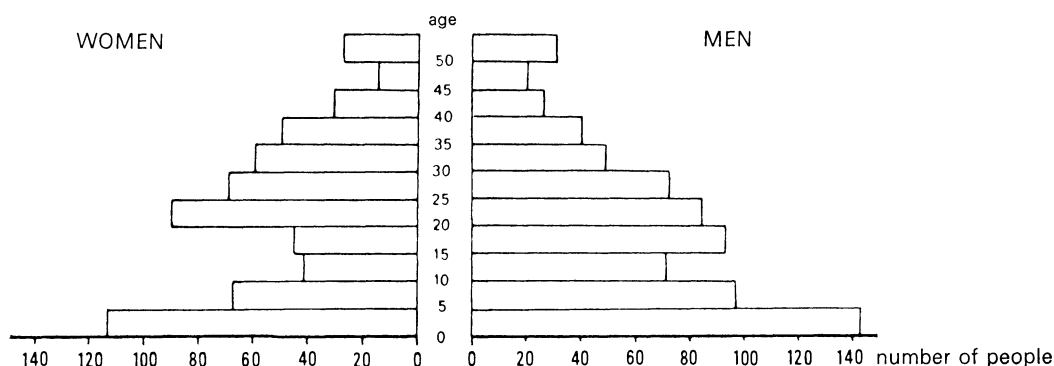


Table 2 gives the distribution in 5-year age cohorts of the population for which complete information was obtained (1333 people).

On the basis of these data we can make a number of observations concerning the demographic structure of the central Yanomami, but let us confine ourselves to the following: the marked imbalance in the *sex ratio* in favour of men for all age brackets ranging from birth to 20 years of age, the balance being resolved between 30 and 35 years of age. The *sex ratio* for the whole sample is 1:19, and this concurs with the data which N. A. Chagnon and I have published previously and does not present any major regional variations.

Harris points to a close correlation between warfare and the *sex ratio* imbalance in favour of men (1971: 265) or, in other words, that warfare seems to be connected with the practice of female infanticide. After making this correct, but commonplace observation, Harris exaggerates the extent of female infanticide and the *sex ratio* imbalance by submitting that the Yanomami kill 50 per cent. of their girls and that in this society there are, globally speaking (i.e. for all age-groups) 154 men to 100 women. Let the reader for a moment check Table 2 and he will note that from birth to 5 years of age there are 143 boys to 114 girls, which is far from the alleged 50 per cent. of girls killed. In the age levels between 0 and 15 years the ratio is 139:100 (139 men to 100 women) and the balance is redressed to 96:100 for all age groups over 25 years of age.

Harris notes a direct relation between the local and temporary intensity of warfare, and the *sex ratio* variations, as though a sudden outbreak of violence, of short duration, would have immediate repercussions on the *sex ratio* equilibrium. This argument prompts him to use in a distorted form the figures I published in 1971, which reflected the short-lived and unstable situation of two localities with dissimilar demographic profiles, although both were equally at war with some of their neighbours (Harris n.d.: 7; Lizot 1971a: 41-2).

What was the Yanomami population growth rate during the last century before the devastating effects of contact? A tentative assessment of the initial population in the two original communities yields the following results: around 1900, *husirawë* and *irabraobë* must have had 150 and 250 inhabitants respectively; in 74

years the population of these two groups rose from 400 inhabitants to 2068, a more than five-fold increase.

The rate of population growth was probably even higher, as the Yanomami ceased to increase in numbers after contact with the white and creole world (in 1950) and *Falciparum* malaria epidemics (1963) and measles (1968) decimated them. If one wishes to estimate what the maximum number of these Indians was in the past, at least 20 per cent. has to be added to the present population figure. Table 3 shows the central Yanomami population representing the most important stages of their evolution since 1900.

TABLE 3. Population development since 1900.

<i>Period</i>	1900-1910	1950	1974
<i>Community</i>			
husirawë	150	821	684
irabraobë	250	1661	1384
Total	400	2482	2068

In 1950 there were 2482 central Yanomami (0.34 inhabitant per sq. k), and between 1900 and 1950 the population in the specified zone had grown six times, which represents a doubling every twenty years and this despite war, infanticide and the late weaning of children (accompanied by the prohibition on sexual relations). During the same time span the number of communities had increased from two to thirty-five subsequent to a fissioning process and highly intensive territorial expansion. One cannot therefore evade the following problem. How sharply can a population rise without threatening the ecological balance and without requiring a change in the economic and political organisation? We are trying to answer the second question here: the first one is too complex and will be dealt with in a work on Yanomami economy currently under preparation, when complete, organised and analysed demographic data are available.

Harris notes that under certain conditions war may have a stabilising effect on the population and help prevent excessive exploitation of critical resources (1971: 264). It has just been demonstrated that war, which is endemic among the Yanomami, has not achieved this two-fold result, but that the precise relationship between population and natural environment remains to be assessed. Our author commits another error when he remarks:

Deaths due to warfare among the Yanomamo, for example, have no long-run effect on the size of the Yanomamo population because like almost all warmaking preindustrial societies, the Yanomamo are polygynous (i.e. each man has several wives) (1971: 265).

The word 'preindustrial' presupposes an evolutionary scheme of societies which I would be inclined to reject, but that is not the issue at hand. The Yanomami do, in fact, practise polygyny, just as they do polyandry which to a certain extent compensates for it and makes it tolerable. But how widespread is it? A brief survey of a few communities indicates that 13.23 per cent. of alliances are polygynous and that only 1.4 per cent. of married men have more than two wives at the same time, which invalidates another of Harris's assertions that 25 per cent. of the men have at

least two wives (1974: 100). In his presentation of the position of women among the Yanomami Harris starts out by quoting a partially distorted myth¹¹; he calls the separate partition where young girls are isolated during their first menses a 'cage' (1974: 90)¹² according to him men are apparently as tyrannical with their spouses as oriental monarchs are with their slaves (1971: 339) and the economic function of women is supposedly neither more nor less than that of an exploited class; (1971: 282) a good deal of the violence which they are subject to is committed for no good reason or motive; men could prostitute their women with the first visitor passing through (1971: 98). All this is untrue.

I cannot help feeling that female infanticide has been greatly exaggerated and that the imbalance of births slanted towards men, real enough though it is, is not a valid indicator. First and foremost a close investigation of the demographic data is indispensable, and this investigation is under way. I suspect that a natural deficit of births of female children makes the disproportion between the two sexes more pronounced, and recent research carried out by a number of biologists on the population of primitive peoples could validate this hypothesis. I observed few cases of infanticide among the Yanomami, but I know that male infants are also killed.

The decision to kill a child is often made before birth, when the sex of the child is still unknown. In many families it is a girl who is the eldest and it is not true, as Harris writes, that the first children are killed systematically until one of the male sex arrives. Once they have been accepted, girls are just as well fed as boys and although they may be disciplined more frequently, one can by no means assert that this relative severity is the cause of a higher mortality.

In the link he establishes between the intensity of warfare, population density and exploitation of natural resources, Harris contrasts a central area where war is deemed to be intensive and population relatively dense, and a peripheral area where warfare is less pronounced, as it were, and population scattered. Perhaps the source of confusion here is to be found in N. A. Chagnon's early writings¹³ where this distinction appears. What exactly is being referred to in this opposition between 'centre' and 'periphery'? It is a matter of relative geographical positions for the Yanomami whom I call central are actually settled in the heart of the territory occupied by the ethnic group, and the so-called *shamat^hari* live on the periphery: this is exactly the inverse of the alleged situation. If, on the other hand the reference applies to the '*Social circumscription*' concept used by Chagnon to describe the situation in certain communities lacking unoccupied territories around them for migratory purposes, then the concept only applies, strictly speaking, to the communities in the Sierra de Parima. That is where Yanomami expansion really began and the groups living there are small, contiguous communities; it is in the Sierra de Parima that population density is probably highest; but contrary to what Harris's theory implies, war and violence there have a rather low profile: the alleged relation between the different variables listed (population density, level of exploitation of natural resources and degree of violence) is not relevant.

One may indeed mention differences which obtain not between a 'centre' and a 'periphery' but between what Chagnon, in a more felicitous if not more elegant formula than the preceding, refers to as 'blocks of population', that is to say collections of communities linked together by a common origin, but separated as a result of fissions and migrations tending to move them further and further apart.

There again a more or less thorough analysis only reveals some rather subtle variations, which do not warrant the contrasting of the various 'population clusters' on the basis of the selected criteria. The southern clusters (*namowei-t^heri* and *shamat^haryi*, studied by Chagnon) and the clusters formed by the central Yanomami (*husizawë-t^heri* and *ɨrabraobë-t^heri*) share major similarities. The internal violence of the former is more marked because of the size of these communities which may have more than two hundred inhabitants.¹⁴ But what parameters can be adduced to measure the intensity of war? It has been noted that the *sex ratio* is not a reliable indicator, it varies only slightly from one 'cluster' to the next (between 1:19 and 1:21). The only objective index remains the number of individuals deceased as a result of violent action as compared with the total number of deaths, but here once more there is no noteworthy difference between the groups surveyed. The southern communities are relatively large, they are frequently separated by considerable distances; the central communities are smaller and closer together. To sum up, although the aspect of settlements may differ somewhat, population density is the same, population growth has been following an identical rate (between 1.5 and 2 per cent. per year) and the distribution of resources *per capita* (i.e. the average available surface of territory per person) is not essentially different.

IV

The moving of plantations to new sites involves moving the dwelling to keep it as close to the gardens as possible. A change of dwelling is often prompted by supernatural or technical considerations, and the Indians take the opportunity to readjust by moving the new *shabono* close to the main garden sites, in order to shorten the distance to travel and to ensure a minimum of surveillance. Only excessively vigorous military pressure from an enemy can force a community into large scale migration¹⁵ and not soil depletion or shortage of game. Some Yanomami have been living in the same place since 1950 without any adverse effect on economic activities.

The Yanomami depend above all on the banana plantain for nourishment. It has been noted that this foodstuff provides sufficient energy, but is poor in proteins (only 1 per cent. of the weight). It would be wrong, however, to underestimate the importance of the *Guilielma gasipaes*, a cultivated palm-tree, which bears fruits plentiful in protein (7.81 gr. per 100 gr. of edible product) (Hunter 1969)¹⁶ and represents a month and a half's food supply per annum. The wild fruits gathered by the Indians are rich in vegetal protein and form a complete annual cycle; the most noteworthy are:

- a Lecythidaceae: *Bertholletia excelsa*, or Brazil nut (*hawari koko*)
- a Caryocaceae: *Caryocar villosum* (*kumato*)
- four palm varieties: *Oenocarpus bataua* (*hoko*),
Mauritia flexuosa (*eteweshi*)
Jessenia polycarpa (*kareshi*)
Maximilia regia (*yei*)

The protein scarcity of the banana/plantain is largely counterbalanced by these fruits.

Furthermore we have only a fragmentary and superficial knowledge of the forest, its resources and ecological balance, and thus we must be all the more modest and cautious in our judgements. We know little or nothing of plant associations, nothing about the plants and their relation to the composition of the soil, nothing or next to nothing about the habits of animals, their supply and the distribution of their habitats, nothing about the complex set of relations established between animal life and the plant cover which serves as auxiliary food. Just to mention a few studies and a few cases: the hand book *Plantas comunes de Venezuela* by L. Schnee (1973), based on the Pittier files, omits a large number of plants commonly used by the Indians for nutritional and technological purposes; the leguminous plants of the *Inga* species which grow in the Upper Orinoco are not listed; an inventory of the Caryocaceae was made only because of my own ethnographic researches. Napier & Napier's *Handbook of living primates* (1967) documents well African monkeys but fails to mention the weight of the *Saki satan*, although this is one of the most common Cebidae. In order to carry out my research on Yanomami economy I have therefore had to take the initiative in collaborating with botanists, zoologists and pedologists, and to study problems not directly related to social anthropology.

If we on the other hand turn to Amerindian economics, we cannot but realise how little has been done up to now. No complete work has been published to date and in order to give an opinion one is forced to regurgitate and misuse some meagre facts. The human geographer K. Ruddle's worthwhile thesis (1970) deals especially with agriculture among the Yukpa, necessarily leaving a number of important problems aside; as for Lapointe's thesis (1970) on the Wayana often mentioned as a reference source, it is distressingly threadbare and one can never be sure when the author is measuring and when he is evaluating. Thus Gross and Harris are reduced to supporting their argument with mediocre data; one need only consider what dangerous extrapolations they are forced into to realise this.

For example, Gross's general table of the game and fish supply in Amazonia (1975: 529-30), completely disregards the regional variations the importance of which ichthyologists and zoologists are just beginning to discover. Harris and Gross also endeavour to demonstrate that there is game shortage in the forest and that hunting is a gamble with a highly unpredictable output, but how are we to reconcile the impressionistic image they construe of it on the basis of accounts of unequal value, with their apparent concern to conduct a rigorous analysis substantiated by figures and precise calculation methods. The discrepancy between facts, methods and objectives of analysis is obvious. To gather an approximate idea of the animal population in a given area, one must first know it intimately, travel with a chronometer in one's hand, and take note of all animals encountered on one's way, list and weigh game killed by hunters and mark the boundaries of the hunting territories.

Having joined in countless hunting expeditions both as observer and participant, I am in a position to claim and to prove that game is not so scarce in this area as we are led to believe: in some areas game abounds, one rarely comes back empty-handed from a hunt, in unfrequented places there are numerous hordes of wild pigs, an abundance of tapirs, Cracidae and Tinamidae in profusion, the trumpeters

are not very wild, monkeys live in large bands; these are the animals most frequently eaten by the Indians.

To ensure their daily meal requirements, the hunters roam through the surrounding forest for five to seven hours and return to the dwelling when the sun has passed its zenith. The women gather and slaughter the smaller animals. When a large quantity of meat is required for a major feast to which neighbours have been invited, or for a funerary rite, the hunters go into areas not normally visited. They set up camp there where they remain from four to six days. During these expeditions food is abundant and the amount of meat which is smoked is often impressive; as an example, here is the yield from three hunts performed with traditional weapons (bow and arrow).

TABLE 4. Returns on hunts of extended duration.

<i>Location of hunt</i>	<i>Duration</i>	<i>Number of participants</i>	<i>Weight of meat (animals eviscerated)</i>	<i>Weight of meat per hunter per day</i>
Wanabui	3 days	11	177·695 kg	5·384 kg
Makorima	5 days	8	142·850 kg	3·571 kg
Karohi	3 days	12	163·800 kg	4·550 kg

V

Little is known about the idea Amazonian groups have of their territory, or about their economic practices; we have more information about the areas occupied by gibbons or gorillas; to ascertain this one has only to compare the two pages of generalities, which Leroi-Gourham (1964: 213–14) in the absence of appropriate documentation, devotes to the matter, with the condensed and precise facts contained in Jolly's *Evolution of primate behaviour* (1972: 102–9). Nevertheless, Harris peremptorily writes the following with respect to the Yanomami:

extended expeditions inevitably cross and recross hunting territories that are used by villages other than one's own (1974: 103).

In fact each Yanomami community has its own hunting territories. These territories are exclusive and determined by use alone, and they are never the object of dispute. Since neither game nor space is scarce, there is no competition for resources and the community is sovereign.

To assess the protein and energy content of the diet, we have chosen two local groups for purposes of comparison. The first is *karohi-t^heri*, on the banks of the River Manaviche occupying the peneplain formed by the Orinoco and its main affluents. The second, *kakashiwë-t^heri* which is more representative of the groups of the interior, deploys its activities in hilly terrain, has a garden in the mountains and enjoys access to a small river. The information we offer here represents a mere fraction of what is available to us, but as it will be seen, does show useful variations and adequately reflects the phenomena evoked here. The facts and figures are presented in a series of tables; the first indicates the composition of population in the two communities; the second specifies the sources of animal protein, and the

third the sources of vegetal protein; with a distinction drawn between men's and women's respective contributions; the fourth, finally, is an evaluation of the diet in terms of each product.

TABLE 5. Population of Karohi and Kakashiwë (consumption).*

<i>Age and coefficient</i>		<i>Karohi</i>			<i>Kakashiwë</i>		
<i>Age</i>	<i>Coef. of Consumption</i>	<i>M</i>	<i>F</i>	<i>Consumption (Pop. × coef. of Consumption)</i>	<i>M</i>	<i>F</i>	<i>Consumption</i>
0-2 years	0	5	4	0	—	2	0
3-12 years	0.5	2	5	3.5	4	2	3
12-16 years	0.8	3	4	5.6	3	1	3.2
16-60 years	1	21	15	36	11	10	21
+ 60 years	0.8	—	3	2.4	1	1	1.6
Total	—	31	31	47.5	19	16	28.8

* In the two communities studied, the population has been divided into age classes and a coefficient of consumption associated with each of them. The number of consumers is obtained by multiplying the number of people by the coefficient; there are 47.5 consumers at Karohi and 28.8 at Kakashiwë. The coefficient that I have assigned to each class is not entirely satisfactory. I shall subsequently refine the method since it is the weight of individuals that must be taken into account.

Without undertaking a detailed analysis of the results obtained let us simply calculate the daily calorie protein ration per person in each of the two communities. Observations in *karohi* were carried out for a 42-day period, there are 47.5 consumers, which equals 1995 days of food-intake; observations were made in *kakashiwë* for 28 days and there are 2.88 consumers, that is 806.4 days of food-intake.

When one attempts to explain the differences in protein value between the groups one discovers that these are not due to a greater or lesser abundance of game in the areas under consideration, but rather to the attitude to work of members in these communities. To come to the point, *kakashiwë* and *karohi* are engaged in equally intensive and murderous war. The forest is more abundant in game at *kakashiwë* than at *karohi*, but hunting is more intensive at *karohi* than at *kakashiwë*. In the vicinity of *kakashiwë* one encounters the first animal, along well-used paths, after 29 minutes walk and after that an animal is driven out from cover whenever one has walked for 22 minutes, on an average. The first camp for long duration hunts is located at a distance of 1 hour and 22 minutes from the *shabono*, and the most remote is 2 hours 14 minutes away. At *karohi*, one comes across the first animal after 38 minutes walk and after that encounters one after every 37 minutes of territory covered. The first camp for long hunting expeditions is 2 hours and 13 minutes away from the *shabono*, the furthest 3 hours and 25 minutes away. Why then, if the forest around *kakashiwë* is more abundant in game, do they eat less meat there than in *karohi*? For the simple reason that people are lazier in *kakashiwë* than in *karohi*: their gardens are smaller, they hunt less frequently and for shorter periods, prefer to eat less well and take it easy instead. When *karohi* hunters take

TABLE 6. Animals consumed: distribution and origin (weight in kg).*

Community	Karohi			Kakashiwë		
	Men weight per cent.	Women weight per cent.	Total weight	Men weight per cent.	Women weight per cent.	Proportion Total weight
Produce						
Game	742.160 100	-	742.160	149.82 100	-	149.82 87.92
Fish	57.810 53.67	49.893 46.33	107.694	12.816 100	-	12.816 7.53
Batrachia	-	6.978 100	6.978	3.26 100	-	3.26 1.91
Crabs, Shell-fish	-	4.925 100	4.925	-	-	-
Caterpillars	29.505 80.84	6.993 19.16	36.498	-	1.950 100	1.950
Palm worms	-	-	-	0.955 50%	0.955 50	1.91 2.58
Termites	-	1.829 100	1.829	-	-	-
Insects Brood comb (bees and wasps)	-	1.630 100	1.630	-	0.550 100	0.550
Total	829.475kg 91.98%	72.248kg 8.02%	901.714kg 100 %	166.851kg 97.97%	3.453kg 2.03%	170.306kg 100%

*The data presented in Tables 6 and 7 represent 42 days of observation at Karohi and 28 days at Kakashiwë. Percentages indicate the parts played by men and women, and proportions the relative importance of a product to other products. The weights given are for consumable parts only, deductions have been made for waste matter (bones, skin, etc.).

TABLE 7. Vegetable products consumed: distribution and origin.*

Produce	Proportion consumable %	Karohi		Kakashiivë	
		Men	Women	Men	Women
Agriculture	75	2,334	—	1,032	—
	77	—	—	7,700	—
Forest (wild fruits)	15	—	7,552	—	—
	95	—	84,395	—	—
	20	—	118,291	—	40,34
	10	2,505	—	4,450	—
	60	49,950	—	—	—
	8	0,520	—	—	—
	80	—	—	0,820	—
	95	—	—	—	19,950
	100	—	0,225	0,292	—
	—	2,386,975 kg 92.25% of total	210,463 kg 7.75% of total	1,044,262 kg 94.54% of total	60,290 kg 5.46% of total
Total weight proportion	—	2,386,975 kg 92.25% of total	210,463 kg 7.75% of total	1,044,262 kg 94.54% of total	60,290 kg 5.46% of total

*Once again the weights have been given after deductions have been made for waste matter (skin, stones, inedible parts). The consumable proportion of each product has been indicated; these were calculated from food samples before consumption.

TABLE 8. Protein and calorie content of produce consumed.*

Community		Karohi			Kakashivë				
Produce		Weight	Proport.	Calories	Protein	Weight	Proport.	Calories	Protein
Agriculture	Plantain — banana	2,234	72.19	2,725.480	22.340	1,032	85.85	1,259.040	10.320
Hunting and fishing	Game (60%)	445.296	14.56	650.136	131.363	89.892	7.48	131.242	26.518
	Fish (80%)	86.155	2.82	87.016	15.421	10.252	0.85	10.354	1.835
	Animals (various) (80%)	41.736	1.36	42.153	7.470	4.136	0.35	4.177	.740
Gathering (wild fruits)	High protein content	68.818	2.24	75.699	6.881	45.610	3.79	45.610	4.561
	Low protein content	184.620	6.03	203.083	1.846	20.242	1.68	20.242	.202
Total		3,060.585	100%	3,783.567	185.321	1,202.132	100%	1,470.665	44.176

*For certain products whose protein and calorie content is as yet unknown, I have made an assessment on a deliberately low scale; I do not therefore believe that I am guilty of overestimating the value of foodstuffs and of having biased the data in favour of my argument.

TABLE 9. Proteins and calories per person per day.

Community	<i>karohi</i>		<i>Kakashiwë</i>		<i>Average</i>	
	<i>Calories</i> (Kcal)	<i>Proteins</i> (gr.)	<i>Calories</i> (Kcal)	<i>Proteins</i> (gr.)	<i>Calories</i> (Kcal)	<i>Proteins</i> (gr.)
<i>Origin</i>						
Animal	390.6	77.32	180.0	36.09	285.7	56.70
Vegetable	1505.9	15.57	1613.8	18.71	1559.8	17.14
Total	1896.5	92.89	1793.8	54.80	1845.5	73.84

off on a hunt of long duration, they are gone for five or six days and do not return until they are honourably laden with smoked meat—in *kakashiwë* such expeditions do not last more than three or four days and the men come back no matter how much game they have killed. During one of the weeks I spent in *kakashiwë* obtaining the nutritional information presented here, the men did not go hunting once, they had just collected their favourite hallucinogen (*Anadenanthera peregrina*) and spent entire days taking drugs; the women complained there was no meat, but the men turned a deaf ear, and that week the amount of protein consumed had fallen to 11 grams per person per day (which lowered the general average considerably). In other words it is not because the forest is short of game that the diet is less rich in *kakashiwë*, but because the inhabitants there are more idle; and warfare is not the more intensive for it either. Here is an unexpected variable which Harris's model had not provided for: the attitude to work.

In estimating the protein content of the Indians' food, Gross (1975:532) uses documentation which is neither consistently reliable nor precise; lacking the necessary information, he entirely disregards plant protein which, according to the season, represents 15 to 30 per cent. of the total food intake. Gross does not base his evaluation of requirements on the weight per person, but on general standards which when applied to the Indians, who are usually of small stature, are probably excessive; finally, the energy expenditure of the activities is not taken into account. With regard to the standards themselves, there is by no means any agreement among the specialists in human dietetics. Without specifying to which populations these standards apply, the author indicates that F.A.O. considers a ratio of 0.30–0.35 grs of protein per kg of weight to be the minimum daily requirement; according to some specialists these standards may reach 0.65–0.80 gr. per kg. The average weight of an adult Yanomami male is 48 kg, so according to the first ratio the adequate protein ration would be 16.8 gr per day, and according to the second 38.4 gr; the average weight of an adult female being 40 kg, her requirements in each case would be 14.10 gr and 32.24 gr respectively. Finally Gross considers a daily protein ration of 40 to 50 gr to be indispensable for Amazon Indians; these figures overrate the requirements defined by our previous calculation. To avoid such a fluctuation it is essential to base any estimate on the weight of the Indians of each age group, and to assess as precisely as possible the energy expenditure of work. The present study does not adequately meet these criteria, but our future publications will.

Whatever the case may be, with 74 gr of protein per person per day, the Yanomami come far above the minimum requirement. If one compares this

gross result with the data presented by Gross for nine Amazonian societies (1975: 531) one notices that the Yanomami hold second place in terms of the quality of their nutrition, just after the Sharanahua of Peru. This is a serious matter for if there is a relation between the intensity of warfare within a given society and the degree of exploitation of natural resources, then why are the Yanomami, who enjoy a rich and varied diet and work so little, by far the most bellicose of all the peoples singled out by Gross? Infinitely more warlike than the Bayana Cuna of Panama with their modest 18 gr of protein per person per day? Harris of course opts for a comfortable retreat when he in the end concedes that war breaks out to regularise the situation long before the upper threshold of the exploitation of resources has been reached. But whether this is a rule or a law the mechanism must work at all times and in all places in order to exist. This is simply not the case for the very obvious reason that the postulated, but undemonstrated, relation is absent and the explanation of warfare has to be sought elsewhere.

It remains to be determined to what extent agriculture, hunting, fishing, and gathering each contribute to the Yanomami diet, how much time is devoted to the various subsistence activities and how much women participate.

TABLE 10. Respective contribution of each activity and proportional cost in time.

<i>Activity</i>	<i>In weight</i>	<i>In calories</i>	<i>In protein</i>	<i>Working time</i>
	%	%	%	%
Agriculture	79.4	75.8	14.2	33
Hunting	11	14.8	68.8	22
Fishing	1.8	1.9	7.6	22
Gathering	7.8	7.5	9.4	23
Total	100%	100%	100%	100%

Table 10 shows that agriculture and hunting are the most productive activities: while representing only 55 per cent. of working time, they contribute 90.6 per cent. of the calories and 83 per cent. of the protein.

In order to eat quite adequately, it is sufficient for the Yanomami to work between 2 hours and 26 minutes (women) and 2 hours and 50 minutes (men) a day, a minimal effort to achieve this excellent result. Would the same be true if nature were stingy with its products and the environment had reached an extreme stage of degradation? These are Indians who are supposed to be on the brink of starvation and material want who work so little and prefer to indulge in cruel warfare rather than working a bit harder. Such are the absurd conclusions established by prejudices and the theoretical preconceptions of vulgar materialism.

A great selectivity in the choice of their foods (in the forest, the Indians do not gather everything available), the localisation of those forest areas which are really exploited, abundant and varied food, unexacting work, all these are signs of abundance. It is voluntarily that the Yanomami neglect work requiring too great exertion and limit themselves to satisfying the daily subsistence with as little effort as possible. The Indians never work more than is strictly necessary. Rather than

TABLE II. Time spent on each activity by sex. (All seasons & all communities)

<i>Working time</i>	<i>Agriculture</i>		<i>Hunting</i>		<i>Fishing</i>		<i>Gathering</i>		<i>All activities</i>	
	<i>Men</i>	<i>Women</i>	<i>Men</i>	<i>Women</i>	<i>Men</i>	<i>Women</i>	<i>Men</i>	<i>Women</i>	<i>Men</i>	<i>Women</i>
Monthly time	30 h	20 h	34 h 50	9 h 16	15 h 30	9 h 16	5 h 40	29 h 38	85 h 20	58 h 54
Daily time	1 h	0 h 40	1 h 09	0 h 18	0 h 31	0 h 18	0 h 11	1 h	2 h 51	1 h 58
Number of times	18	12	9	3	5	3	2	8	34	23
Average time devoted on each occasion	1 h 40	1 h 40	3 h 52	3 h 05	3 h 06	3 h 05	2 h 50	3 h 42	—	—

repair a hammock cord which is defective they prefer to wait for it to break and fall apart. Rather than replacing the broken and rotten roof of the *shabono*, they let the water drip until the situation becomes unbearable. Sometimes the yield from a garden turns out to be totally inadequate due to insufficient work; the Indians then resort to resources ordinarily disdained: they eat the plants and fruits which they did not previously condescend to collect, they work harder, nomadise more, and receive the hospitality of allied neighbouring communities.

No, the Indians have not 'eaten the forest' nor destroyed the environment. On the contrary, their economy is in harmony with the possibilities of the natural environment and is perfectly integrated with it. The reasonable limits of population growth have never been reached and territorial expansion could still be carried out into uninhabited areas.

Who can deny that in particular cases war may have served the purpose of conquest among certain primitive tribal peoples; the Tsembaga example has become a classic in ethnographic literature and Rappaport's demonstration is conclusive. But should we on the basis of one proven example establish an automatic link between warfare and the appropriation of neighbouring territories? This causal relation has never been demonstrated. The Yanomami do not make war to conquer hunting territories, nor to capture marriageable women either, and even though they themselves offer us the latter rationalisation, there is no reason for us to go along with them: the ideology of a people is one thing, the deeper, underlying motivations for action another. The Yanomami make war to punish insults, avenge deaths, and fulfil kinship obligations.

That one of the consequences of warfare and internal violence in the communities happens to have led to a better adaptation to the environment is an effect and not a cause. Vayda (1961) and Gross are not wrong when they stick to such general observations and claim that war has profound economic and demographic repercussions. But have they explained the nature of the phenomenon of war? They have not. I, in this article, am only saying what war is not.

If ecological anthropologists are to lead us into such misconceptions it would be better to turn away from them, give up their gadgets and refrain from using their distorting mirror. A certain caution compels us to shun fads like the plague.

NOTES

I am grateful to Raymond Hames for his helpful comments on the manuscript.

¹ See the works mentioned in the bibliography under the name of the authors. The interview given by Marvin Harris in the review *Psychology Today* is particularly irritating and his use of extravagant language makes it difficult to know whom he is addressing. Ethnologists or readers of *Playboy Magazine*? A much better argued study is his 'The Yanomamö and the cause of war in band and village societies' (n.d. multigr.).

² See the interview in *Psychology Today* where this abrupt assertion appears: 'I think that the Yanomamo fight and kill their daughters because they are hungry' (p. 65). In the preface to *Cows, pigs, wars and witches*, Harris in trying to explain why the Yanomamo fight says: 'I think there may be a shortage of protein there' (1974: vi), in the same work one reads: 'Amazonian specialists Jane and Eric Ross suggest that protein scarcities and not libidinal surpluses account for the constant fissioning and feuding among Yanomamö villages' (1974: 102). The idea is thus clearly expressed.

³ 14. The size of the tree and the hardness of the wood are not specified, whereas this information is essential.

⁴ Cf. Lizot 1971a: 36-7 and 1971b: 157. N. A. Chagnon shares this opinion and it is expressed in his 1966 thesis. Harris is not unacquainted with these texts since he quotes them.

⁵ Harris asserts: 'They lack knowledge of how to make cooking pots although plantains are best prepared by boiling' (1974: 101). Plantains are mainly eaten roasted on embers and the Yanomami knew how to make pottery.

⁶ Cf. Lizot 1974: 18–19. The Ye'kwana (Makiritare) are much less numerous than the Yanomami and it is not they who have prevented the latter from settling along the Orinoco and the Mavaca as Harris contends.

⁷ Raymond Hames says that the Ye'kwana often have the Yanomami do this work for them.

⁸ Lizot 1972. For the importance of fish in the diet see table 1 below.

⁹ I have been studying the central Yanomami since 1968, and I have already explained this term which simply refers to the geographical location. The reader will recall that after constant fissions and movements of the local groups, their number and location change from one year to another.

¹⁰ According to N. A. Chagnon's interpretation of this concept: a population cluster is formed from groups having emerged after fissions and migrations from a single, original community. For a succinct history of the movements of the central Yanomami, consult Lizot 1971b: 140–2.

¹¹ On two occasions he offers a fairly altered version of the myth of the origin of women (1974: 90 and 1971: 562). According to Harris's version the ancestor *kanabororima* (read *kanabororiwë*) became pregnant in both legs, from the left the first woman emerged and from the right created feminine men, those who are faint-hearted and afraid of fighting. The interested reader can compare this with my version of the same myth, which I have extracted from the most reliable sources (Lizot 1975: 10–12).

¹² In *Le cercle des feux* I describe the first menses of a young girl; nowhere do I give the impression that she has been struck by a misfortune.

¹³ Harris uses this for his own very individual purpose. For N. A. Chagnon's position, refer to 1968: 114.

¹⁴ It must be mentioned that one may remain for a whole year in a community without witnessing any act of violence, or a single war expedition. All communities fission at one time or another, both small and large. Fissioning is above all a socio-political phenomenon, with remote and secondary economic effects.

¹⁵ The distinction between the micro-movements of communities around gardens and macro-movements prompted by war stems from N. A. Chagnon. On this subject see his studies published in 1968a and 1968b.

¹⁶ I wish to thank Robert Carneiro who has been kind enough to lend me documents on the analysis of foodstuffs, particularly the following bibliographic sources: B. S. Platt (1962), McCance *et al.* (1960), and J. R. Hunter (1969).

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