

YANOMAMA WARFARE, PROTEIN CAPTURE, AND CULTURAL ECOLOGY: A CRITICAL ANALYSIS OF THE ARGUMENTS OF THE OPPONENTS

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The protein hypothesis and derivative hypotheses have generated considerable debate among anthropologists in recent years. Proponents of the protein hypothesis assert that animals are a scarce resource in the Amazon, and that this limits the size, distribution, and permanence of traditional human populations in the forest. Some proponents have gone further in developing a derivative hypothesis in which they assert that warfare functions as a homeostatic mechanism to regulate the Yanomama population in relation to its prey populations.¹ Napoleon Chagnon, an anthropologist who is well known for his extensive research with Yanomama in Venezuela, joined Raymond Hames in an *Interciencia* article in which they challenge such ecological hypotheses.

In general I agree with Chagnon and Hames (1979, 1980) that, as formulated thus far, the animal protein hypothesis does not adequately explain Yanomama warfare. Nevertheless, in their article in *Interciencia* these critics of the animal protein hypothesis, cultural ecology, cultural materialism, and related matters, perhaps in their zeal, have neglected to consider some important points while distorting and/or confusing others. Their essay cannot be left unchallenged;

issues must be clarified so as not to perpetuate further misunderstandings.²

Chagnon and Hames (1980) present a concise summary of certain aspects of three themes: (1) studies of indigenous warfare in the Amazon, (2) history of the application of various facets of cultural ecology, cultural materialism, and cultural evolutionism to several theoretical issues concerning Amazonian indigenes—especially the animal protein hypothesis, and (3) their Yanomama fieldwork along with selected comparative data from other cultures as these bear on the protein hypothesis.

The review by Chagnon and Hames of the first theme provides a valuable up-dating of Metraux's classic study, although the review is far from complete (e.g., Divale 1973, Fernandes 1952, J. Ross 1980: 34-35). Also they do well to call attention to the strikingly small number of studies on Amazon warfare since Metraux.

Even though space is limited, their critical review of the protein hypothesis and related concerns omitted significant points. Henry Bates (1962: 104, 193, 274, 294, 298), the 19th century naturalist and Carl Sauer (1958: 186), the Berkeley geographer, each anticipated a number of the ideas

propounded by subsequent students of the cultural ecology of the Amazon such as Meggers, Carneiro, and Lathrap. In the case of the Yanomama, Cocco (1972: 189) and Wilbert (1972:40) both suggested that game depletion may be a problem, while Migliazza (1972:395) noted the coincidence between greater violence and less game in the central region. Anduze (1960:321) related warfare to territorial invasions as game became scarce locally. Yet the most glaring omission of all is the lack of attention to Marvin Harris, one of the most prominent proponents of the application of the protein hypothesis to the interpretation of Yanomama warfare, infanticide, and other practices. Such omissions raise the question of what additional arguments and evidence may have been ignored by Chagnon and Hames in advocating their position in the controversy.

The third theme has already been addressed elsewhere by Chagnon and Hames (1979). While their discussion in *Interciencia* is more elaborate than their previous essay, it does not reveal much that is new. However, Chagnon and Hames are to be congratulated for publishing in Spanish in the host country where they have conducted their field research.

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Cultural ecology and materialism are not synonymous

In the introduction to their article, and to varying extents in some later sections, Chagnon and Hames confuse cultural ecology with cultural materialism. While on occasion there is some overlap between these two orientations within cultural anthropology, more often they are quite distinct. Not all cultural ecologists are materialists, and the converse holds as well. Indeed, sometimes there is little if any ecology in the works of materialists. Neither do all cultural ecologists ascribe to the primacy of the technological and economic components of the cultural system. That is a tenet of cultural materialism, although the version presented by Chagnon and Hames is an oversimplification of the subject (Harris 1979b). The writings of Nietschmann (1972, 1973) and Reichel Dolmatoff (1960, 1976) are among the many publications on cultural ecology which clearly invalidate the images created by Chagnon and Hames. Many cultural ecologists are merely intellectually attracted to pursuing the legitimate question of how culture relates human societies to their environment. It is unfortunate that some students of other persuasions, who obviously are not familiar with cultural ecology and cultural materialism, allow their prejudices to falsely impart more to cultural ecology than is actually the case.

In their failure to clearly distinguish between cultural ecology and cultural materialism, Chagnon and Hames falsely derive cultural ecology from the superorganic and culturological positions. Accordingly, they claim that cultural ecologists consider the individual to be merely a passive or neutral element in the cultural system. Unfortunately this claim not only confuses cultural ecology and cultural materialism, but it ignores advances in cultural ecology during recent years such as Rappaport's (1975: 271-272) distinction between cognicized and operational models, and the investigation of the decision-making process as it relates to environmental problems (Moran 1979: 17-20, 97-102; Orlove 1980: 245-248). Ethnoecology, which often overlaps with cultural ecology, offers yet another major set of exceptions to the generalization of Chagnon and Hames (e. g. Berlin and Berlin 1978). To derive cultural ecology from the superorganic and/or culturology positions is to seriously misunderstand and distort the nature and history of all three subjects (e. g. Kaplan and Mammers 1979). For instance, Steward (1955:

36, 1974) explicitly developed cultural ecology as a counter to the circular explanation (culture causes culture) of the superorganic and culturology positions.

Contrary to Chagnon and Hames (1980: 593), the inspiration for cultural ecology does not derive only, or even mainly, from biology and its concept of adaptation. The concept was only adopted by cultural anthropologists relatively recently (Carneiro 1968, and compare Steward 1955, 1974). Likewise, the following statement by Chagnon and Hames (1980: 346) is simply not true:

"La única modificación significativa que hacen los ecólogos culturales de esa visión de la cultura como entidad superorgánica consiste en la introducción del medio ambiente, tal como está escrito más arriba, lo cual 'abre' el sistema de la cultura para admitir que influencias tales como perturbaciones ambientales producen cambios en el sistema."

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This assertion ignores biologist Martson Bate's (1960a: 140-141, 1960b: 554) distinction of the total, effective, and perceived environments, Steward's (1955:37) concept of the culture core, Rappaport's (1975: 271-272) cognicized and operational models, and a host of other theoretical and methodological developments which are considered in recent text books and other sources.³

Given the wealth of readily available reviews on the nature and history of cultural ecology, it is difficult to understand how Chagnon and Hames could arrive at such an unformed, distorted, and confused view.⁴

Cultural ecology is not synonymous with the so-called animal protein school

There is no homogeneous field of cultural ecology. Instead the term encompasses diverse and sometimes conflicting approaches, and these can only be grouped together because they

share a concern for the relationship between culture and environment. Thus it should not be surprising that Beckerman (1980), one of the critics of the protein hypothesis, is a cultural ecologist. Carneiro (1978: 19-20), who is well-known for his long interest in cultural ecology and evolution in the Amazon, points out that favoring ecological interpretations in general does not commit one to accept any one of them in particular. He goes on to state:

"Indeed, we must guard against the tendency to push ecological explanations too far. We should beware of the Pangloss in our midst who would claim that every belief and practice, no matter how obscure or perverse, is at bottom an adaptive response to ecological conditions. . . . Thus there is always a residue of culture that may never be explained ecologically. The size of the residue may be debated, but its existence cannot be denied."

It is also noteworthy that some of the most ardent critics of cultural ecology and the protein hypothesis have made significant contributions to these subjects through their collection of field data to oppose the hypothesis. In fact Lizot (1977, 1979) and Chagnon and Hames (1979, 1980) are among these contributors.⁵

Finally, the tendency of Chagnon and Hames to equate cultural ecology with cultural materialism and the protein hypothesis is unfortunate because it detracts from numerous substantial contributions of cultural ecology in the Amazon and elsewhere. These authors (p. 347) mention that the Amazon and New Guinea have provided fertile ground for studies of cultural ecology, but actually the same could be said for the Canadian arctic, African savannas and deserts, Australia, Oceania, the Alps and Andes. (Numerous examples are cited in the textbooks listed earlier.) Cultural ecology will not stand or fall on the basis of studies in the Amazon or the protein hypothesis.

Protein availability, acquisition, and consumption are not synonymous

Chagnon and Hames (1980: 354) are wrong when they assert that evidence of an adequate or surplus dietary intake of animal protein disproves the hypothesis that animal resources are a limiting factor in the Amazon. Animal resources may be a limiting factor regardless of the level of protein in the diet, although a deficiency would be most suggestive. Animal protein as a limiting factor actually refers to the *availability* of faunal resources which is determined by a complex of behavioral and ecological factors of the prey species (Sponsel 1981: 156-197). The level of protein *consumption* in the diet depends on how successful the population is in coping with the adaptive challenge of animal resources as a limiting factor (cf. Harris 1979a: 32). Moreover not all of the protein consumed is necessarily *assimilated*, depending on such factors as parasitism, disease, and stress (Ross, E. 1979: 153, Scrimshaw and Young 1976: 59). All three factors are pertinent in the case of the Yanomama villages where Chagnon (1968: 20) reports that 54% of all adult deaths are due to malaria and other epidemic diseases, while 24% of adult male deaths result from warfare.⁶ It is noteworthy that among the Aguaruna Jivaro of Ecuador, Berlin and Berlin (1977: 69) found a correlation between a high level of parasite infestation and high intake of protein and other nutrients. Also, recently Spath (1981) has suggested that dependence on manioc as a staple crop may increase the protein requirements of a population. (Traditionally Yanomama cultivated bananas and plantains, but through contact with neighboring Ye'cuana they have been acquiring manioc, in some areas for several decades.)

It is difficult to reconcile the assertion by Chagnon and Hames (1979, 1980) that the Yanomama consume sufficient protein with Chagnon's (1968) statements elsewhere:

"Meat is always the most desirable food and is always considered to be in short supply." (p. 91)
 "The jungle simply does not produce enough wild foods to sustain larger groups..." (p. 98)
 "The tropical forest is much less productive and reliable than one would imagine." (p. 33).⁷

Are we to accept these and other qualitative statements which Chagnon made before the protein controversy, or his opposite quantitative

statements made during the controversy? Unfortunately even the quantitative statements are problematical. They are based on a 216-day sample of hunting and 60-day sample of fishing in the village of Toropo on the Padamo River. It is not clear how representative this study site is: the Yanomama traditionally live in the interior of the forest, yet Toropo harvested 31% of its protein from fishing on a main river.⁸ Toropo is also problematical because it is a satellite to a Ye'cuana village. For example, Hames (1979: 231-233) mentions that Yanomama occasionally join Ye'cuana on nocturnal riverine hunts where they take advantage of the Ye'cuana's headlamps, shotguns, and motorized canoes. Hames also notes that the Ye'cuana sometimes kill species such as the giant anteater which they cannot consume because of taboos, so they give such prey to their Yanomama neighbors. Finally, the representativeness of Toropo must be evaluated in relation to a sample drawn from the more than 50 villages in the vast and varied range of the Yanomama. More convincing would be comparative data collected simultaneously in several villages over a period of a few years.

Validity and utility of the protein hypothesis and derivative hypotheses

It is crucial to realize that the assertion that animal protein is

anthropological and biological, for the assertion that animal protein may be a limiting factor in at least some ecosystems of the Amazon. On the basis of an extensive survey of the literature Gross (1975) has marshalled three main lines of evidence to support the hypothesis that animal protein is a limiting factor on the size, distribution, and permanence of human populations in the Amazon. (1) For traditional societies (a) their staple plant foods which are mainly root crops, manioc being the most important one, provide low quality protein; (b) hunting is difficult everywhere—but especially in the interior forest, and there the protein problem is compounded by the poor fishing conditions; and (c) animals have not been domesticated as an alternative source of quality protein. Gross cites some biological studies which indicate that the populations of prey species have a low density and patchy distribution. Also he asserts that most animals are arboreal and/or nocturnal. (2) Gross extracted data from the literature to estimate the protein yield from hunting and fishing. From a sample of 9 populations he derives a mean of 35g./capital/day, and claims that 63 is the standard according to most nutritionists. (3) Gross surveys evidence which is mostly anecdotal and indicates that indigenous people express a concern for the scarcity of meat through (a) special words for meat hunger⁹ (b)

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a limiting factor in the Amazon is *one* hypothesis, and its application and elaboration to explain cultural phenomena such as warfare (Harris 1974, 1978, 1979c; J. Ross 1980), faunal taboos (McDonald 1977, E. Ross 1978, 1979, 1980, E. Ross and J. Ross 1980), and intravillage sexual politics (Siskind 1973) involves a whole set of *derivate hypotheses*. Whereas the second set of hypotheses, depends on the initial hypothesis, the converse does not hold. While I am not persuaded by the derivative hypotheses as they have been formulated and sustained by arguments and evidence thus far, I am more favorably inclined toward accepting the probable validity of the first hypothesis.

Although it is far from proven, there are solid grounds, both

prestige of a successful hunter, and (c) women's sexual favors to a good hunter (cf. Siskind 1973 and Vickers 1975).¹⁰

Several additional observations may be offered in support of this argument: (1) Even if prey were relatively abundant, they are not readily available because the majority of species are small, solitary, nocturnal, arboreal, and camouflaged. White-lipped peccary are the only outstanding exception, yet they have formidable defense mechanisms and an unusually large home range (cf. Kiltie 1980). (2) Most anthropologists have looked at hunting only from the perspective of the hunter. A biological view — predation dynamics and community ecology, reveals that humans do not enjoy, a monopoly on prey. Many other species are predators,

and therefore competitors with humans, such as the harpy eagle, jaguar, ocelote, anaconda, and caiman. (3) Comparisons between the human predator and other predator species reveals the former's distinctive combination of large body size and large group size. Only the jaguar comes close in body size, but it is solitary. (4) The wet season dispersal of fish and other animals markedly lowers their population density, and accordingly, their availability.¹¹

In any case, regardless of the validity of the idea that animal protein in particular is a limiting factor, ecology teaches that there are always limiting factors. Limitations on the intrinsic rate of population increase are inevitable, and the fact that we do not observe a continuous exponential rate of population growth in the Amazon indicates the operation of intrinsic and/or extrinsic limiting factors. To deny this fact of life is to reject much of demographic and ecological thought from Malthus onward (Boughey 1973: 12-19). Moreover, rather than a single limiting factor, it is more likely that a combination of factors operate, although the specific combination may vary in space and time (Sutton and Harmon 1976: 213, Vickers 1979, J. Ross 1980: 38). Another candidate for a limiting factor operating on Amazonian indigenous populations during the last few centuries is disease resulting from culture contact (Beckerman 1980: 553, Davis 1976: 15-20). If some investigators can suspend their biases towards salvage ethnography and the evolutionary reconstruction of past cultural conditions, then disease becomes a major problem in the study of the *contemporary* cultural ecology and adaptation of Amazonian societies.

Regardless of their total or partial validity, the protein hypothesis and derivative hypotheses have proven to be of heuristic value in promoting the collection and analysis of empirical field data on aspects of cultural ecology which had previously been ignored, especially human predation.¹² This fact is demonstrated by a comparison of the literature before and after the review by Gross. (This is not to suggest that there have not been other important contributions prior to Gross.) In 1971 Bennett (pp. 35-36), a pioneer in the cultural ecology of Latin America, wrote:

"The exploitation of the wild animal resource of food has received inadequate attention from scholars who have studied Amerind societies. Too often a list of animals said to be eaten by the group investigated is offered as sufficient information.



This clearly inadequate treatment is further exacerbated, in some cases, by inaccurate names of the animals to be obtained and this makes the entire list suspect. One is generally told little or nothing about the quantities of a given taxon taken, seasonal aspects (quantitative and qualitative) of hunting and fishing activities, the nutritional contribution made by such animal foods to human diets and the details of meat and fish storage and/or preparation."

This evaluation applies equally well to cultural ecologists and students of other persuasions prior to the protein controversy, but since then the situation has improved considerably (e.g. Hames 1980b). Nevertheless, the subject of protein capture in the Amazon, or more broadly —human-animal relations, has been far from exhausted, even though one suspects that the protein hypothesis and derivative hypotheses may have run their course in terms of heuristic value.

Finally, it should be stressed that these issues are not all trivial academic concerns by any means. If animals are a scarce resource in the Amazon the problem may be seriously aggravated by cultural changes with contact such as the introduction of new technology for hunting and fishing which may lead to more rapid depletion of animal resources (e.g. Hames 1979) and resettlement with an increase in local population density. Another complication is that colonists become competitors with indigenes, since they usually also depend on local animal resources for protein (Smith 1976, 1978). In all of this it must be realized that most indigenes do not have the economic resources to raise imported domestic animals as a food source or to rely on canned meats and fish. Development projects have already proven that the introduction of domestic animals to the Amazon ecosystem is problematic in many respects (Barbira-Scazzocchio 1980). Thus local

wild animal resources are likely to remain important in the diet of indigenes and colonists in the future, and likewise the protein issue will remain important.

Yanomama warfare

Not all cultural ecologists advocate a simplistic, reductionistic, single-factor explanation of warfare, contrary to the claim of Chagnon and Hames (1979: 913). Vayda (1976: 2-5), who is well-known for his many years of studies of cultural ecology and warfare, is a case in point. Jane Ross (1980: 38), a proponent of a materialist interpretation of warfare, also explicitly disavows the idea of a single-factor explanation.

In the case of the Yanomama, despite years of studies, there is a remarkable absence of clear, systematic, and adequate documentation on a quantitative basis of the frequency and duration of the various types of aggressive behavior for a representative sample of villages. Moreover, of the more than two dozen anthropologists who have worked with the Yanomama off and on over a century, only two (Biocca and Chagnon) stress aggressive aspects of this society. Anthropologists Jacques Lizot and Kenneth Good (personal communications), who have extensive experience with Yanomama, do not fully accept the image of "Hobbesian savagery" which Chagnon imparts in *Yanomama: The Fierce People*. Goetz (1969: 55) and Smole (1976: 14-15) have provided a more balanced characterization of Yanomama life which includes non-aggressive aspects. The linguist Migliazza (1972: 421-422) writes: "The term *waiteri* has a semantic range from brave, courageous, daring fearless to savage, furious, wild, aggressive, and fierce depending mainly on the context and situation." Thus Chagnon's gloss of the term *waiteri* as the fierce people is not applicable in all circumstances. In my own fieldwork with Sanemá, a northern variant of the Yanomama culture, many other attributes impressed me more than their aggressiveness. It is also important to recognize that, despite some author's illusions, the Yanomama are far from a homogenous group in terms of their ecology, genetics, culture, language, and history. This is apparent if one considers that this large population (estimates range from 10,000 to 30,000) occupies a vast area (estimate range from 15,000 to 259,000 km.²), one with a diversity of environments from main river and hot lowland to headwaters and cool highlands. Migliazza (1972: 439-447) has defined four major language

groups each with several dialects, and describes a similar degree of cultural variation. Spielman and others (1974) have discussed genetic and linguistic differences among Yanomama. This variation deserves more documentation as well as explanation.¹³ It has been ignored by both the proponents and opponents of the animal protein hypothesis, except to some extent in the work of Eric Ross.

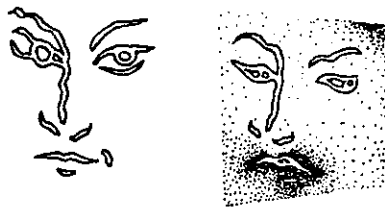
Even if Yanomama warfare was the result of purely ideological, or more broadly, cultural, causes, this would not preclude the possibility of the activity having ecological consequences and functions.¹⁴ On the basis of the mortality and other statistics reported by Chagnon, it is impossible to ignore let alone reject the effect of warfare in reducing population size and redistributing population. Other components of Harris' argument may be salvaged as well. It is unfortunate that the critics of such hypotheses tend to dismiss the entire hypothesis on the basis of identifying flaws in some aspects. Would it not be more in accord with scientific procedures to discard the invalid components and proceed to work with the remaining ones to construct another hypothesis,¹⁵ There may be a possibility of reconciling the seemingly extreme positions of Chagnon and Harris, if we take the former's explanation to represent the proximate level of causality and the latter's explanation to represent the ultimate level of causality.¹⁶

In the Amazon protein may be a long-term problem arising with occasional perturbations in the environment (cf. Colson 1979, Hayden 1975, Meggers 1976). In any case, the original explanation of Yanomama warfare which Chagnon (1968) offered has been recognized as tautological as well as culturological (Crocker 1969: 742). Thus I must agree with Harris (1974:7):

"Please remember that like any scientist, I hope to present probable and reasonable solutions, not certainties. Yet imperfect as they may be, probable solutions must take precedence over no solutions at all... Like any scientist, I welcome alternative explanations, provided that they better fulfill the standards of scientific evidence and as long as they explain as much."

Conclusion

By now it should be clear that cultural ecology, cultural materialism, and the animal protein hypothesis and derivative hypotheses all have their defect as does the critical review



of these subjects by Chagnon and Hames, although the controversy made substantial contributions to the knowledge and understanding of the cultural ecology of the Amazon. Despite the impression given by Chagnon and Hames, cultural ecology is not moribund, but it continues to thrive and develop, and it will do so as long as the question of the relationship between culture and environment remains pertinent for scholarly consideration. To deny the relevance of culture in dealing with the practical problems of human life in a given environment is to deny a unique aspect of the human phenomenon. Indeed, such a denial is no more than another form of reductionism. To dismiss cultural ecology is to dismiss the question of human survival. In the protein controversy, as is often the case with heated debates, the truth probably lies somewhere in the middle rather than at either extreme. Ultimately it is not a matter of mentalism versus materialism, but of recognizing and attending to the reality of the human phenomenon—that *Homo sapiens* is simultaneously a biological, cultural, and intellectual being. The gravest misunderstanding of all in the case of some critics of cultural ecology is that they fail to see that cultural ecology, in conjunction with ethnoecology, is at least potentially a catalyst for the reconciliation of the mentalist and materialist positions, unless one side denies the other a priori.

NOTES

- 1 For convenience the term Yanomama is used here following Migliazza's (1972:26) suggestion, since it refers to all variants of the indigenous group in question where as Yanomamö is more restricted.
- 2 These misunderstandings are not limited to the *Interciencia* article (e.g. Chagnon 1980).
- 3 Bennet (1976), Hardesty (1977), Moran (1979), and Netting (1977).
- 4 Abbot (1970), Anderson (1973), Ellen (1978), Freilich (1967), Grossman (1977), Harris (1968), Hatch (1973), Heider (1972), Helm (1962), Orlove (1980), Rappaport (1975), Shalins (1977), Shimkin and Lowe (1978), Speth (1978), Steward (1974), Vayda and McCay (1975), Vayda and Rappaport (1968), and Voget (1974).
- 5 Hames (1980b) has edited a collection of articles which attempt to refute the animal protein hypothesis.

- 6 Unfortunately Chagnon does not provide details on the basis of this sample nor evaluate its representativeness for the Yanomama as a whole.
- 7 Smole (1976:163) arrived at similar conclusions for Yanomama in the Sierra Parima area.
- 8 The Padamo Yanomama have been adapting to a fluvial environment for over 30 years (Hames 1980a:58).
- 9 Montgomery (1970:101) states that psychological tests on the Sanemá subgroup of the Yanomama reveal a preoccupation with hunger, especially with reference to meat and hunting. The Yanomama have a general word for hunger (ohi) and a specific one for meat hunger (a naiki) according to Lizot (1975:62).
- 10 The problems with Gross's argument and evidence have been considered elsewhere (Chagnon and Hames 1979, 1980, Lizot 1977, Roosevelt 1980: 49-54, Sponsel 1981: 306-321, Beckerman 1980).
- 11 These and other observations have been discussed in detail by Sponsel (1981).
- 12 Hames (1980a:3) has also recognized this fact.
- 13 Moran (1980:213-230) has recently addressed the problem of generalizing in spite of variation and the distinction between macro— and micro-analysis.
- 14 The idea that aggression may have biological functions and consequences irrespective of its causes is nothing new nor alarming to biologists (e.g. King 1973). Nor is the idea new to anthropologists. For example, on islands in Oceania aggression has been one recurrent response to resource depletion (e.g. Alkire 1978).
- 15 Despite their allegiance to sociobiology, Chagnon and Hames ignore one of its principal tenets — testing multiple working hypotheses rather than the advocacy of a single favorite position.
- 16 Wilson (1980:23) discusses the distinction between proximate and ultimate causality. Here the former refers to short-term physiological, psychological, and social processes, whereas the latter refers to long-term ecological and evolutionary processes.

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