

## ECOLOGICAL CONSEQUENCES OF AMAZONIAN WARFARE<sup>1</sup>

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As anthropological investigations of war move in increasingly diverse directions, it is important to keep sight of questions of longstanding concern, such as whether war serves to maintain a balance between people and their natural environment in non-state societies. In 1961, Vayda advanced the ecological position that primitive war tends to be adaptive, in the sense of "being more advantageous than disadvantageous" in "providing people with the means of adjustment to the geographical environment and to other basic conditions of life" (Vayda 1969:203-204 [original 1961]). This paper examines that proposition in regard to warfare in Amazonia.

Cultural ecology developed primarily as a functionalist paradigm (Ferguson 1984a:28-30). Explanation consisted of an argument or demonstration that a given cultural pattern, such as war, has consequences that help to maintain a population within environmental tolerances. Early ecological analyses concentrated on adaptation at the level of societies or regional populations. By 1970, four specific societally adaptive consequences of war had been postulated. Two concern the distribution of people to available resources: by village relocation, and by the creation of buffer zone "game preserves." Two concern the restriction of rates of population growth: through battle casualties and associated effects, and through the development of a pattern of selective female infanticide.

Since that time, ecological studies have undergone a thorough reconceptualization. The concept of adaptation itself has been hotly disputed (Alland and McCay 1973; Bargatzky 1984; Bergerhoff Mulder 1987; Chagnon and Irons 1979; Hames and Vickers 1983; Ruyle et al. 1977; Sponsel 1986). One major trend has been a move by ecological anthropologists away from the concept of entire populations adapting at regional levels, to focus instead on individual responses to local conditions (Ferguson 1984a:33-42; Orlove 1980; Vayda and McCay 1975). Thus the four posited adaptive consequences of war may seem theoretically out of date.

Analytic fashion notwithstanding, they remain as standing hypotheses, and ones which address a general issue of enduring significance: what are the general material consequences of living with war in non-state societies? Moreover, the fundamental question underlying regional adaptation models is all the more compelling because of the shift to an individual perspective: how is it that human populations generally do attain some form of viable adjustment to regional environmental possibilities? Both questions are addressed here. The first, by assessing available evidence regarding the four postulated adaptive consequences. The second, by proposing a different kind of demographic effect, that war pumps population toward demographic sinks.

## DISTRIBUTION OF POPULATION TO RESOURCES

The distribution argument invokes two distinct processes: the relocation of groups to establish a better balance of population to resources, and the creation of unutilized areas where game can replenish itself.

*Village Relocation*

The idea that war prompts the movement of people from densely settled areas to areas of less pressure on resources was suggested in some of the earliest statements of cultural ecology (Suttles 1961; Vayda 1969). It was applied to Amazonia in the controversial "protein hypothesis," which asserts that populations in interriverine habitats encounter a limiting factor in nutritionally critical but effectively scarce game animals (Bennett Ross 1971; Gross 1975; Harris 1977; 1979a; Siskind 1973; and Ferguson n.d.). Basically, the idea is that a settled village depletes locally available game supplies, and that it is prompted to move to less depleted areas by the danger of attack.

Indeed, hunting does lead to diminishing returns for hunting effort around most large interriverine villages (Baksh 1982; Carneiro 1985; Good 1983; Hill and Hawkes 1983; Milton 1984; Paolisso and Sackett 1982; Saffirio and Scaglione 1982; Vickers 1980; and Ferguson n.d.), and war or the threat of attack often does lead Amazonian peoples to move their villages (Arvelo Jimenez 1973:14; Beckerman 1980; Bennett Ross 1980:53; Chagnon 1977:41-44; Clastres 1972:143; Gregor 1977:303-305; Gross 1983:436; Hames 1983; Metraux 1963:392; Murphy and Quain 1955:10-12; Wagley 1983:39-40). It follows that in at least some cases, war will result in people moving from areas of depleted game to areas with better hunting (see Good 1978:21; Hames 1983:421-423).

The problem is that village relocation more commonly occurs without the threat of attack, typically in response to a mix of factors including increasing scarcities of different resources, inter- and intra-village conflicts of varying types and intensities, the death of key persons, and such mundane concerns as insect infestation and decay of house structures (Balee 1984:507; Chagnon 1973:126; Good 1984:4; Gross 1983; Harner 1973:45; Johnson 1982:415; Morey and Metzger 1974:30, 53; Riviere 1969:37; Smole 1976:58-59, 92; Vickers 1983:469-473). If war in some particular case is an outgrowth of local resource depletion, that depletion in itself would eventually lead to relocation without war. Thus the adaptive benefit of war, itself a process with very high costs, seems questionable.

*Game Preserves*

The idea that war creates no-man's-lands between hostile groups which act as game preserves, allowing hunted species to replenish themselves, was first proposed by Hickerson (1962, 1965). It also was applied to Amazonia as part of the protein hypothesis (Bennett Ross 1980:47-48; 1984:97-98; Harris 1977:52-53; Harris and Ross 1987:61).

DeBoer (1981) documents the existence of extensive, war-related buffer zones for people inhabiting the major rivers at the time of contact (and see Myers 1976). However, river peoples usually are not critically dependent on game for protein (Beckerman 1979; Lathrap 1970; Roosevelt 1980), so this

cannot be taken as evidence of a major adaptive benefit of war. I found only a few explicit reports of buffer zones among interriverine peoples (Descola 1981:627; Good 1984:4; Harner 1973:56; Wagley 1983:29). The lack of more reports, I suspect, is because recognition of buffer zones is linked to a multi-community perspective applied to groups actively practicing warfare, and we have relatively few ethnographies which combine those two characteristics. To this researcher, it seems very unlikely that there would not be a tendency to avoid hunting close to people who want to kill you.

At present, there is insufficient information to judge whether war does or does not regularly lead to the creation of buffer zones in interriverine areas. However, even if it does, the adaptive significance of these game preserves is unclear. Maps and geographic descriptions of interriverine peoples indicate that the interior is by no means filled with people (Chagnon 1973:128-129; 1977:41-43; Hames 1983; Henley 1982:6, 9; Holmberg 1969:front map, 10; Wagley 1983:32-33; and see Riviere 1984:16-25; cf. Ross 1978:6). (Steward and Faron's [1959:374, 379] description of "marginals" [below] emphasizes their isolation from other peoples.) Populations cluster, and there are large areas between settlements and/or "tribes" that are not actively hunted. The geographic drift of interriverine groups provides that these areas were hunted in the past, and could be again in the future. Compared to these areas, the additional spacing, possibly attributable to avoidance of enemies is, again, of questionable significance. (Certainly, much of this empty space is due to the effects of western contact. But if my hypothesis [below] that some interior areas are population sinks has merit, empty space would also have existed in pre-contact times.)

Village relocation is, and creation of game preserves may be, a consequence of warfare. But there is reason to doubt that either consequence constitutes a major adaptive benefit, that either leads to a better or more secure distribution of population to resources than would be achieved in the absence of warfare. Moreover, there is another spatial consequence of war that makes its distributional value more dubious.

#### *Population Nucleation*

Many observers have noted that the danger of war causes people to settle in larger villages than they otherwise would (Arhem 1981:54; Bennett Ross 1980:54-55; Carneiro 1987:110; Chagnon 1973:199; Clastres 1972:164; Good 1978:21; Hames 1983:398, 423-424; Oberg 1973:199; Shapiro 1972:38-39; Steward 1949:704). Ross (1978:5-8, 31) argues that large villages deplete local game faster than small villages, and so have to relocate more often (for discussion and documentation of this point, see Ferguson n.d.). The large village pattern represents a less efficient use of available game, and theoretically will lead to a lower population density than would occur with small villages. By compelling nucleation, war in effect lowers the carrying capacity of an area.

Considering this aspect, along with the doubtful status of adaptive benefits related to village relocation and game preserves, it appears that war does not produce a better distribution of population to available natural resources than would be found in the absence of war.

## RESTRICTION OF POPULATION GROWTH

The second posited benefit of war is that war helps to maintain a balance between population and resources by restricting the rate of population growth. Again, this is argued to occur in two distinct ways: through direct casualties of war, and via the creation of a pattern of female infanticide.

*Casualties of War*

Like the others, this idea was also suggested prior to the Amazonia debate. Vayda (1967:87; 1968:470) had been arguing that a common function of war was to reduce population pressure via battle casualties, and Reichel-Dolmatoff (1973:32; original 1961) had suggested that war among (non-Amazonian) Colombian chiefdoms was part of "a mechanism to control population increase." Harris (1971:229) initially endorsed this view, but by the time the debate over Amazonia was joined, he (Harris 1977:39; and see Harris and Ross 1987:55) largely discounted it because most casualties are male, and with polygyny, most males "are reproductively superfluous." Harris instead joined Divale (1976; 1978) in a hypothesis emphasizing the importance of female infanticide (below). Divale (1970), however, places great importance on the demographic significance of male combat deaths, with specific reference to the Yanomamo, and Harris (1984:111), in a recent article on the Yanomamo, includes combat deaths as a form of "demographic regulation" by war.

Only a few sources quantify war mortality in Amazonia, but these show very high rates. For the Yanomamo studied by Chagnon (1988:986), approximately 30 per cent of adult male deaths are violent. For Wao studied by Yost (1981:687), 44 per cent of all deaths are from "intratribal spearing." For Achuara studied by Bennett Ross (1984:96), 59 per cent of adult male and 27 per cent of adult female deaths are in feuds. Other nonquantified reports of fighting suggest comparable levels of mortality (Goldman 1963:162-164; Henry 1964:51-59; Maybury-Lewis 1974:172-178; Wagley 1983:30-40). It should be emphasized that these cases are not typical. They stand out in the literature on recently studied peoples as unusually intense levels of fighting. However, in earlier historical periods, the demographic impact of war, associated with epidemics and the European slave labor system, was probably as great or greater (Hemming 1978, 1987).

Combat deaths will have additional repercussions on population trends. Many Amazonian people are not polygynous. Werner (1983) finds that among the monogamous Mekranoti, male combat deaths have the result of reducing the time when women are reproductively active. He (Werner 1983:243-244; and see Harris and Ross 1987:10) also notes several studies which show that polygyny reduces fertility compared with monogamy.

Men are more than sperm sources. Among other things, they are producers. Men are responsible for bringing home game, an essential source of protein and other nutrients (Ferguson 1988:143-145; n.d.). So loss of a significant number of men will lower nutritional standards, and nutritional stress in mothers is often translated into risks for fetuses and infants (Harris and Ross 1987:7). Loss of men or women will also disrupt for a time the domestically structured system of provisioning. (Neel [1973:172-173] found that much of

the mortality in a measles epidemic he observed was not due to the disease itself, but to the breakdown in provisioning it caused.)

All of these factors indicate that battle casualties can have a significant negative impact on rates of population growth. The remaining question, to which we will return below, is whether these terrible costs of war promote a balance between population growth and environmental potentials.

### *Female Infanticide*

This is a more complicated proposition than previous ones, and it has received more intensive theoretical scrutiny. It was developed during the "grand functionalist" period of cultural ecology (Ferguson 1984a:30-34), and posits a circular, self-regulating chain of variables. It differs from previous hypotheses in being the only proposed adaptive consequence of war that was suggested first by Amazonian material, and then extended to band and village warfare in general.

The link between female infanticide and war was first argued by Divale (1970), who notes that the idea occurred to him in reading Chagnon's descriptions of the Yanomamo (Divale 1970:175). Chagnon reinforced the idea of a linkage shortly thereafter.

The initial excess of males appears to be caused by differential female infanticide, also a direct reflection of the intensity of warfare... The relationship between warfare intensity and differential female infanticide is as follows: males grow up to become warriors and defenders of the village. Females, on the other hand, are considered to be less valuable and are often destroyed at birth... Needless to say, the bias towards killing female children has an effect on the intensity of inter- and intra-village hostility: most Yanomamo warfare and intra-village fighting is directly attributable to competition over women. (Chagnon 1972:273-274; and see Chagnon 1967:139-141; 1973:134-136; 1976:96; Lizot 1977:502; Shapiro 1972:119-120, 187; Smole 1976:72-73, 230 N. 2.)

The most complete theoretical formulation of the relationships between female infanticide and war appeared in a joint article by Divale and Harris (1976; also see Harris 1977:41-44; 1979a; 1979b:68-69). Although clearly based on the Yanomamo material, what they propose is a general theory of war in band and village societies. In highly condensed form, the argument is as follows: war makes necessary the raising of aggressive men for the defense of the group. Fierceness is inculcated in males through an ideology valuing males as superior to females, and reinforced by sexual rewards for dominating men, including polygyny. Relative devaluation of females leads to a preference for male children, and selective female infanticide. Female infanticide and polygyny create a scarcity of marriageable women. That promotes conflict over women, and that generates warfare, thus perpetuating the cycle. Female infanticide, by reducing the number of women who can reproduce, restricts population growth. Divale and Harris (1976) connect this pattern to broader manifestations of a "male supremacist complex," but that is not directly relevant here. For the purposes of this paper, the important questions are: does war lead to selective female infanticide, and if so, does that restrict the rate of population growth in a way that maintains a balance between people and ecological conditions?

There has been a good deal of debate over the cross-cultural statistics Divale and Harris (1976) use to establish a correlation of war and female infanticide (Bates and Lees 1979; Divale and Harris 1978; Divale et al. 1978; Dow 1983; Fjellman 1979; Hawkes 1981; Hirschfeld et al. 1978; Howe 1978; Kang et al. 1979; Norton 1978). One major problem was clarified by Hawkes (1981) to show that the relationships posited in the Divale-Harris model could hold true only in patrilocal societies, where grown fierce men would remain with the rearing group--a point which Harris accepts (1984:112). (This restriction to patrilocal societies is suggested in some earlier statements by Divale and Harris [1976:527, 531] and Harris [1977:60, 63].) Hawkes (1981) points out that in Divale and Harris's (1976:535) statistical sample, of eleven South American cases coded as warring at the time of the census, only three are patrilocal. The average sex ratio for the junior age category in populations of these three is 142 males to 100 females; in the nonpatrilocal eight, it is 103 to 100. This clarification supports the idea of an association between war and female infanticide in some societies (see Hurtado and Hill 1987), but at the same time, it makes it less than a general theory.

This restriction also directs attention back to the patrilocal Yanomamo. As described by Chagnon, they are not just the type case upon which the general theory was constructed, but they are by far the case with the most relevant data. So it must be taken as a major challenge when Chagnon offers a new explanation of biased age ratios in junior age categories, one that excludes deliberate female infanticide.

Chagnon et al. (1979:308-309) argue that the Yanomamo do not conform to the pan-human average of about 105 male to 100 female live births, but instead have a live birth ratio of about 129 to 100. Available information does not support this rather astounding claim. The assertion is based on a sample of 678 reported live births in selected Yanomamo villages in the period from 1964 to 1974. In an earlier discussion of the problems of gathering this data, Chagnon (1972:273) states: "it is almost impossible, therefore, to get an accurate estimate of sex ratio at birth." Might dead female infants be underreported? That possibility seems consistent with the following: "females... are considered to be less valuable and are often destroyed at birth: a common explanation for female infanticide is simply, '... she was female and not valuable, so we killed her'" (Chagnon 1972:273).

That Yanomamo explanation is difficult to reconcile with Chagnon's new position. It is also difficult to understand how biological causation could result in the difference in sex ratios associated with more and less intensive warfare (157:100 vs. 121:100 [Chagnon 1972:273]). That these ratios are, rather, attributable to varying intensities of female infanticide is suggested by the single case presented of relatively comprehensive data: in one village, of 22 women, twelve were ascertained to have destroyed at least one child over a thirteen year period. Of those killed, seven were female and five were male--not a pronounced antifemale bias. The sex ratio in the zero- to thirteen-year old category in this village was 107:100 (Chagnon et al. 1979:302-303). Unless strong new evidence is presented to support the skewed-ratio-at-birth hypothesis, it can be rejected.

While the evidence is far from conclusive, there does seem to be an association of female infanticide and warfare among patrilocal peoples, and particularly among the Yanomamo.<sup>2</sup> But does this have the effect of creating

a balance between population growth and environmental potential? Apparently not. The Yanomamo of the area studied by Chagnon are growing at a rapid pace, variously estimated at from .5 per cent to 3 per cent in recent years (Chagnon et al. 1979:296; Good: personal communication; Harris 1977:49; Lizot 1977:505). Given the extraordinary intensity of warfare among the Yanomamo, this growth rate also undermines any idea that combat deaths and related mortality lead to an ecological balance.

Before leaving the subject, it is appropriate to note that the practice of infanticide, abortion, and other growth reducing techniques are widely reported for Amazonian peoples, including those with no warfare (Janet Chernela, personal communication; Meggers 1971:103-113; Oberg 1973:191; Wagley 1983:135-136; and see Kang et al. 1979:207-208; Lapointe 1970:107; Reichel-Dolmatoff 1971:145). So whatever population regulation can be achieved by these methods, it seems that it can be achieved in the absence of war.

In sum, the proposition that war has negative consequences for population trends is supported; but the proposition that these consequences maintain a population/resource balance is not. This leaves us with the question that inspired so much of early ecological studies: how is it that human populations have avoided the Malthusian dilemma? Given the potential rate of human population growth, why is that human groups typically do not crash into famine? One answer, perhaps, may be found in a previously unappreciated demographic consequence of war.

#### A POPULATION PUMP AND SINKS

One of the first broad patterns to become apparent in the nascent ethnology of Amazonia was based on the distribution of languages and culture traits. To Steward (1948, 1949) and others, the distributions indicate that the culture bearers had spread along the major rivers and up the tributaries, with prior inhabitants pushed or restricted to areas of marginal subsistence productivity. The ecological limitations of these marginal areas, especially in terms of agriculture, forced people to adapt with technologically and structurally simple societies (Steward 1948:883; Steward and Faron 1959:26-27, 288-290, 374-381).

Lathrap (1970:186-190; 1973) based on Steward's conclusions, posits a directionality to the movements, with population growth in the agricultural heartland of the central Amazon spilling outward, progressively pushing smaller and weaker groups further upstream or off the floodplain and into the forest. Lathrap (1973:94) emphasizes the "wreckage" of complex societies entailed by this process, arguing that all of Steward's "marginals" were "degraded descendants of peoples who at one time maintained an advanced form of tropical forest culture" (Lathrap 1973:87).<sup>3</sup>

The Steward-Lathrap hypothesis, as I see it, is that population growth in areas of high productivity ultimately stimulates warfare, which pumps population to progressively less productive areas, and people pushed into those areas undergo a forced devolution to simpler, band forms of organization. This seems plausible, especially if qualified to note that: (1) there may have been more than one center of population growth, and (2) forced displacement may have replaced peaceful migration from the more productive areas rather

late, only after all the prime land fell within the territorial claims of incipient chiefdoms. However, 40 years after Steward, there is still little direct evidence on questions of who moved where in ancient times, and if these moves were accompanied by violence (Roosevelt 1987:161-162; cf. Migliazza 1982). On the other hand, recent advances in archaeological research, and particularly in archaeological osteology (Roosevelt 1987:162-163), indicate that relevant evidence may be forthcoming. Given the possibility of imminent data, I feel warranted in offering a speculative extension of the Steward-Lathrap hypothesis.

Neither Steward or Lathrap, to my knowledge, discuss the likely population effects of all this. My suggestion is that: (1) forced population displacement, and the breakdown of complex forms of social organization, would be accompanied by major population losses; and (2) that the end points of these displacements, the areas of marginal productivity, may act as demographic sinks, with long-term population decline. Taken together, these two effects are theoretically capable of explaining relative population stability in Amazonia as a whole.

There are several reasons to expect that the displacements and organizational breakdown would be accompanied by major population losses. Major losses are expectable in the actual displacements. This could begin with major military defeats, trigger secondary wars as the displaced people try to carve out a new territory, and continue with prolonged raids and counter-raids between victors and vanquished. Besides direct combat deaths, there are the circumstances of flight. The few descriptions available show that life in the forest on the run is often grueling and precarious (Goldman 1963:163; Golob 1982:127, 166-177; Henry 1964:3-7; Wagley 1983:40-43; and see Chagnon et al. 1979:305). Friendless refugees would find it very difficult to establish viable gardens, especially in the extensive regions which lack native stone, and could be forced to rely on hunting and gathering. And however affluent hunters and gatherers may be elsewhere in the world, in Amazonia, this can be a relatively difficult and uncertain way to keep alive (Henry 1964; Hurtado and Hill 1987; Kloos 1977; Milton 1984; cf. Hill and Hawkes 1983).

Then there is the process of sociopolitical decay posited by Lathrap. From a purely technoenvironmental perspective, this is conceivable without necessary loss of life, provided there was ample room to spread out. But the process must be considered in the seemingly inevitable social consequences of a sudden, war-provoked collapse of an existing social order: breakdown and transformation of family and kin relations, disruption of economic systems, intense political infighting, anomie, even internal war. How much stress will be felt and for how long would depend on local ecology and the circumstances of displacement. Altogether, the ability to adjust to interior ecology would be far less for refugees of war than if such a move occurred as peaceful expansion from the rivers.

Once a displaced group had adjusted to a point of reasonable stability, they might still suffer gradual population decline. To the degree they were circumscribed, or in direct competition over possessible things like secondary growth forest areas, there would likely be warfare, with all the growth reducing effects already described. More mobile groups probably would not make war, but they would have other problems. People pushed into truly marginal areas face a non-stop struggle for existence, and a probable long-



term population decline. Other environments may appear adequate, but suffer from some less obvious or periodic liability, which may take newcomers as a terrible surprise. For people living more or less by the day, one bad year can wipe out the population growth of many good years.

All of the above hazards must be considered in light of the discussion by Wobst (1974) which suggests both upper and lower limits to "maximum band" size. Above the upper limit the cluster divides, below the lower limit a downward spiral may ensue. There may be a relatively narrow range between these limits in given circumstances. A major population loss could push a population cluster below the minimum self-sustaining level, and so lead to even greater losses. A parallel process could occur for local groups, with the number of men dropping below the minimum needed to defend the group and/or bring in routinely sufficient supplies of meat, although such unfortunates would probably join other local groups at the first opportunity.

Ethnographic evidence that peoples on the peripheries of human occupation of Amazonia suffered long-term population decline is limited (see Hurtado and Hill 1987; Isaac 1977; Stearman 1984). It cannot be otherwise. At present we sometimes cannot say with certainty even if given groups were exclusively hunters and gatherers in the recent past (Jackson 1983:150-155; Milton 1984:17-19), so most estimates of more subtle things such as population trends are little more than guesses. It also is impossible to factor out the impact of Western contact on demographics. Still, it is suggestive that areas at the end of regular refugee routes usually see a process of coalition and assimilation of small cultural groupings into a new local culture (Jackson 1983:99; Riviere 1969:27-28; Villas Boas and Villas Boas 1973:30, 35, 37) which is what one might expect if refugee groups were small and declining in numbers. Particularly noteworthy are the "Maku," hunters and gatherers of the Northwest Amazon who actually encompass at least three language groups and who sometimes incorporate themselves into local sedentary cultures (Jackson 1983:148-149).

The Yanomamo are another relevant case. Their prehistory is debated (Harris 1974:100-102; Layrisse et al. 1962; Lizot 1977:499-500; Smole 1976:8-18; Spielman et al. 1974; Wilbert 1966:237-238). But one thing seems reasonably certain: that for the thousand years or more that they occupied the area in or around the Parima highlands, their population certainly did not increase over time at anything close to its present rate, which leads to doubling in a century or so. Despite all the warfare and the new epidemic diseases, recent living conditions of the Yanomamo have been more conducive to population growth than past conditions deep in the highlands.

Definitive answers to questions about population trends in the interior may be forthcoming only when archaeological and linguistic research allows the construction of more complete tribal phylogenies. The pump-and-sinks idea is presented as a possibility for investigation. It is consistent with known facts, and it can explain long-term, over-all population stability in Amazonia: any spurt of growth in the center would just accelerate passage through the system, and be negated by population losses (a) in the process of displacement itself, and (b) population decline in the inhospitable end points of the displacement sequence.

The idea that war forces people from densely settled areas into less hospitable population sinks has been argued, in varying formulations, for the

New Guinea Highlands and Highland Fringe (Lowman 1980; Meggitt 1972; Morren 1977; 1984; Stanhope 1970). It fits patterns of ecological variation and war on the Northwest Coast (Ferguson 1984b). It may be applicable to other areas where human populations encounter a range of environments, the most favored supporting large social groups and population growth, the least favored supporting only small groups, and leading to population decline.

### CONCLUSIONS

In regard to the distribution of population to resources: (1) village relocation to more productive areas sometimes is prompted by war, but also occurs without it, (2) game preserves may be a consequence of war, but these areas are of questionable ecological significance, (3) population nucleation forced by war limits ability to exploit available natural resources. On balance, it seems that war produces a less favorable distribution of population to resources than would occur in its absence.

In regard to restriction of population growth: (1) casualties of war are demographically significant, both in sheer numbers, and the decreased natality and increased mortality these deaths will cause, (2) female infanticide is somehow associated with war in patrilocal societies, although there is some indication that here too these actions can occur in the absence of war. Neither consideration, however, leads to a homeostatic balance with resources, and in the type case of the Yanomamo, population continues to grow at a rapid pace.

I have posited a third kind of demographic consequence of war: that war drives a pattern in which population is pumped out of areas of large settlements and population growth, losing great numbers in the forced displacements, and ending up in sinks where unfavorable conditions lead to population decline. This pattern is theoretically capable of explaining long-term population stability over a large area like Amazonia.

Taking all this together, should we say that war is ecologically adaptive? Are people better able to adjust to their natural environment than they could without war? Against this idea, I have presented evidence and argument that where war seems to have beneficial consequences, these can also occur without war, and/or are of dubious ecological importance. Moreover, it seems reasonably certain that the presence of warfare means that far fewer people will be able to live in an area than could do so without war.

It could be argued that the last point is proof of war's adaptive benefit, that war prevents a population from growing until it permanently degrades the environmental base. True, that is a possibility. Other possibilities are that in the absence of war, population growth would be dealt with by deliberate intensification of growth restricting techniques, by productive intensification and political evolution, or by natural disasters--periodic famines or epidemics are arguably no more of a curse than chronic warfare.

But speculation on what would happen if everything were different is pointless. Assessment of available evidence indicates that people are not in a better relationship to their natural environment because of war. War may produce a fit between population and resources, but it is a bad fit. Life is worse for war.

## NOTES

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2. If there is a connection between war and female infanticide in patrilocal societies such as the Yanomamo, it may have less to do with an ideology of male supremacy than with the practical exigencies of the situation. When in danger of being attacked, the need to space births is more acute. Mothers take young children with them when they go into the forest (Chagnon 1977:83-84), and it is much more difficult to escape raiders if one must carry more than one child (Biocca 1971:25, 35, 221-222). The destruction of more female infants may be because they can be replaced by capture, while males cannot. Boys are trained to be aggressive and to recognize enemies. They may actually go on a raid as young as age 12 (Biocca 1971:55; Chagnon 1977:84, 132; Lizot 1985:74). To capture a male child would bring a potential enemy into the village, and in practice the Yanomamo will not accept boy captives (Barker 1959:153; Biocca 1971:34-35, 271; Chagnon 1977:122; Lizot 1985:2). Females do not represent the same kind of threat, and of course the capture of girls and women is a strong motivation in Yanomamo warfare.
3. Lathrap (1973:94) specifically excludes Ge and Bororo from the scope of his model. I concur. The Central Brazilian exceptionalism that pervades Amazonian ethnography applies here as well. The cerrado environment seems unusually productive for hunting (Werner 1983). It supported vigorous and complex cultures, able to resist invaders and even to displace nearby horticulturalists (Balee 1984:245, 249; Bamberger 1971; Gross 1979; Steward and Faron 1959:377, 379).

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