Yanomami Warfare

A POLITICAL HISTORY

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Explaining Yanomami Warfare:

Alternatives and Implications

In this book I have attempted to compile what is reported about Yanomami history and explain its main events by reference to a coherent structure of theoretical propositions. One overarching proposition is that the Yanomami's practice of war—along with such political matters as long-distance migrations, the splitting of population blocs during those moves, and the interrelated domains of trade, intermarriage, and political alliance—is primarily determined by local articulation with agents and aspects of European expansion.

As the Yanomami entered known history in the middle of the eighteenth century, they were keeping to the high country, surrounded on all sides by hunters of humans to feed the colonial slave markets. With the formerly complex societies of the region obliterated, and with no travel route passing through their homelands, the Yanomami remained less exposed to the ravages of Western contact than were peoples of lower and more accessible terrain. After mid-century, episodic expansion into the region by a semblance of colonial government mitigated the slave hunting, although in places it continued for at least another century. But now there also were peaceful contacts and opportunities to trade, and through them, to obtain steel tools.

After the mid-1700s, the Western presence waxed and waned. It collapsed in the decades around the turn of the nineteenth century, returned at low levels during the century's middle decades, and then exploded during the rubber boom. The collapse of the boom by 1920, followed

by the global economic depression, led to a spasm of Western retraction that is often mistaken for absolute, unbroken isolation. The initial penetrations by the most recent wave of Westerners began at different times in different places, but in the 1950s contact picked up all around and began to spread out from new points in the highlands. At some times in some places, massive contact and sociocultural disruption set in, and acculturation proceeded toward "peasantization" or cultural extinction.

The theoretical model developed in part I of this book is intended to apply to Yanomami situations beginning at the time when outside raiding for captives ended and trading began, and ending with the onset of acculturative transformation. The effort to substantiate the model's validity with historical information in parts II and III has been lengthy and detailed, so a summary of the model is probably now in order.

It begins with the significance of steel. Steel tools are crucial means of production that, along with other Western manufactures, have been avidly sought by many, if not all, Yanomami. The problem is one of supply: Western manufactures have been scarce and unequally distributed. Improving access to them has involved three options, each with its own drawbacks. First, Yanomami can relocate their villages closer to source points, which may require the considerable effort of starting a new garden at long distance and perhaps draw the violent opposition of others. Second, people can move as individuals or families into another village with a better established supply—but they do so at the sufferance of residents who may demand a bride for the privilege. Third, a group can develop trade with a village that has good access, but again, this option may require securing goodwill with brides or other offerings.

The third option brings up the matter of intervillage economic organization. Constrained by the practicalities of travel, trade strongly tends toward stepped transactions along chains of neighboring villages, except where Yanomami have adapted to canoe travel along the broader rivers. Villages with resident or regularly visiting Westerners try to monopolize access to them and thus to their trade goods. Accomplished monopolists assure themselves a supply of goods for their own use and a surplus they can trade to more remote groups. Those who trade out Western manufactures receive considerable benefits in return: indigenous manufactures, wives, labor, and political and military support. A growing dependence on Western products thus creates a potential for sharply exploitative exchange and thus for an often pronounced ambivalence between trade partners.

Trade is the foundation of a unitary alliance relationship that is interwoven with considerations of marriage, political understandings, and status. In a multidimensional alliance of this sort, any part can stand for the whole. Thus, in a severely strained relationship, a trivial slight may be sufficient grounds for a fight. Women, the most valuable "item" of exchange and the capstone of the alliance, are frequently of central concern in times of strain, and men's disputes over them frequently trigger violent clashes.

The political character of an alliance—how people get along—is shaped by two factors: the availability of Western goods, which makes for "generous" or "stingy" trade partners, and the ability to apply force. That ability is determined primarily by four elements: direct support by Western backers, possession of shotguns, numbers of militarily able men, and exceptional fierceness of individuals. The first two correlate with direct access to a source of Western manufactures, but the last two may favor more remote groups. Different combinations of factors produce a range of possibilities and lead to changes over time that can include passage from alliance to war.

Force can be applied within an alliance through duels of varying intensity. In relatively good relationships, carefully managed chest poundings can clear the air of animosities. But in a politically charged atmosphere, pounding matches can escalate to include large numbers of men who menace each other with clubs and axes, or worse. When the violence turns deadly, it can mark the transition from alliance to war. Duels can be precipitated by various affronts because, as I just noted, any one can stand for the whole alliance relationship. The application of force in a duel is a means of affecting the terms of that relationship. The terms may be those of the exchange of women, of the balance, direction, and velocity of trade in Western versus indigenous products, and of relative status. A more violent shift from alliance to war results from a treacherous slaughter of guests at a feast. These comparatively rare events involve complicated political positionings and come about at moments of sharp contradiction in trade interests.

The shift from alliance and trade to war occurs in three general situations: when recent Western expansion into an area leads to a new, marked inequality in possession of Western goods and therefore to unsettled exchange relationships; during times of established but still limited Western presence, when shifts in the positioning of Westerners destabilizes recently developed exchange and alliance patterns; and during periods of Western retraction that creates scarcities of manufactures among more dependent people and an inability by Yanomami suppliers

to live up to expectations to provide Western goods. Stability in the contact situation generally leads to a reestablishment of peaceful, albeit often strained, relations of trade and alliance.

Once a state of war exists, the expectation of danger, combined with informational limits and uncertainties, can push groups toward preemptive raiding. A period of intense raiding and counterraiding may ensue, or raids may be unidirectional. Active wars rarely last longer than two years because one side will move out of range. A frequent tactic is to kill a headman and thus militarily incapacitate his group. The human costs of war are great: death, pain, disfigurement, emotional loss, arduous and dangerous journeys, much labor, and unpleasant living circumstances. For these reasons, people seek to avoid war, and active warfare cannot be prolonged.

Generally, a war is initiated when the aggressors have reason to expect that their costs will remain relatively limited. The benefits that can outweigh such limited costs include an improved supply of Western manufactures and the corollary advantages it brings. In some efforts, supply is improved directly, through plunder. More often, the strategic rationale is to affect intervillage economics by eliminating, preserving, or inserting a middleman in a trade network.

When Western manufactures are in short supply everywhere, or when they are available in abundance from multiple sources—so that in either case there is relative equality in possession of them—no raiding is expected, according to the terms of this model (although other sources of conflict could certainly develop). Raiding is predicted to occur when there is a general scarcity of steel but marked local inequalities in possession. The standard pattern is that more isolated groups raid better connected villages when the latter do not have the political and military advantages provided by a resident, armed Westerner. This direction of raiding is typically reversed as Western outpost villages establish military hegemony; then violence often occurs out beyond the first dependent allies. Other strategic uses of violence are to prevent a group from establishing a new garden in a location that would harm the aggressors' trade interests, and to keep people from more remote groups from attempting to bypass middlemen and approach the sources directly.

The expression of all these posited relationships displays several major variations, as well as seemingly endless local permutations. Across much of the northern range of their territory, Yanomami entered into war or subordinate alliances, or both, with non-Yanomami who had better access to sources of Western goods. The main version of this pattern joined the Sanema with the Yecuana, but Ninam, Yanomamo, and even some Yanomam were drawn to these options as well, and others besides Yecuana sometimes assumed the dominant position. In this pattern, the ethnic disjunction between unequal allies adds a categorical element absent in intra-Yanomami relations, but otherwise the structure of the relationship is quite similar to that of relationships between dominant and dependent Yanomami groups (see Ferguson n.d.*c*).

Farther east, a critical difference in geography gave rise to another major variation. Ninam and later Yanomam lived close to the trade artery of the Uraricoera and the huge trade network to its north. Some local groups became dependent allies of established traders, while others pirated passing trade goods. When this violence forced the few peoples remaining on the rivers to leave, the Yanomami moved in, joined the trade system, and promptly gave up raiding. Farther south, several groups of Yanomam and Yanomamo experienced another distinctive pattern in the decades after the rubber boom, first working for and then raiding and looting *balateros* and other woodsmen.

The year 1950 marks the beginning of the missions and other outposts of resident Westerners. The missions provide a sample of cases for comparison, and information about local intervillage relations is often exceptionally good. As these situations have been described, the Yanomami's efforts to control the sources of Western goods seem uniform, but the political-military relations that developed around the outposts varied considerably. This varying political character was shaped by the quantity of Western goods flowing from source points, by the ability of different groups to apply force, and by the stability or instability of the Western presence.

In all the major pattern variations, however, violence extends outward from the areas where Western goods are first received. The center of contact itself may pass into largely nonviolent, if still antagonistic, political relations, but those who obtain goods through trade, service, or ceding women often find themselves attacked by other Yanomami farther out. When Yanomami subordinated themselves to Yecuana, the "tame" villages were frequently attacked by *bravos* to the south. When Yanomami broke through the Uraricoera into the Guiana trade network, they were raided in turn by the "wild" people of the mountains. When some more southern local groups became wealthy in steel by working for or plundering woodsmen, they were assaulted from behind by others. And around so many of the missions, war was recorded between the outpost monopolists' allies and those farther away—although, dealing

from strength, the mission group's allies were more likely to be the aggressors than the victims. All this fighting is what constitutes the internal warfare of the Yanomami.

The Orinoco-Mavaca area—the main focus of part III—lay at the extreme end of the spectrum of violence, in contrast to the relatively peaceful Mucajaí and Catrimani mission zones and even the nearby Iyewei-teri. Sporadic contacts along the Orinoco from the late 1930s were linked to increasing tensions and outright warfare. Starting in 1950, a major Western "invasion" occurred, but no secure Western support for any group existed during its first years, and warfare reached a high pitch. Over all these years of antagonism and high mortality from disease and war, local society was disrupted and destabilized, making the instrumental use of violence common and lowering the threshold for war.

Nevertheless, stabilization of the Western presence was soon followed by Mahekoto-teri hegemony and a return to peace after the mid-1950s. Subsequent changes in the Western presence in the late 1950s led to new tensions and actual warfare in areas of marked retraction. Around the juncture of the Orinoco and Mavaca rivers, tensions were brought to a boil by the arrival of new Westerners in 1964. But again within a few years, the new situation was accommodated and wars ended. The worst violence then passed outward to more remote areas experiencing new contacts.

Comparing the Orinoco-Mavaca area between 1937 and 1968 to other places and times, its periods of warfare were not necessarily the most protracted and intense periods of violence ever experienced by the Yanomami. The slave raids of the mid-eighteenth century and the rubber boom of the late nineteenth and early twentieth centuries may have produced as much or more bloodshed in some areas. But the wars of the Orinoco-Mavaca area certainly were the worst of any well-reported situation, and we have extensive and detailed information about them. These Yanomami truly were fierce people, but that fierceness was not an expression of Yanomami culture itself.

The form of explanation employed in this book is not like most anthropological theorizing on war. The application of the model is historical: it maps the actual occurrence of war and other major developments against discernible interests in regard to the contact situation. The model interlocks with another approach (Ferguson 1992a) that can be applied only to situations with unusually complete reporting. In the article just cited, I posit that the broad disruptions of Western contact

have fostered reliance on violence in interpersonal affairs and lowered the threshold at which conflict turns to war.

My causal, deterministic approach to history is hardly the current style. It is also true that not every case fits the theoretical expectations and that many individual wars could just as easily be explained by other theories. But when one considers *all* Yanomami wars, clear exceptions to the pattern are few and the evidence in support of the theory is strong. Ultimately, the test of any theory is how well it explains the facts in comparison with other theories. In the following sections, I contrast my approach to the other major theories that have been offered to explain Yanomami warfare.

The Protein Hypothesis

One major line of explanation has come to be known as the protein hypothesis (see Chagnon 1983:81–89; Chagnon and Hames 1979, 1980; Gross 1982; Hames and Vickers 1983:12–18; Harris 1984; Sponsel 1983). Actually, there are several linked hypotheses, which I have discussed individually in two previous publications (Ferguson 1989b, 1989c). Readers interested in the details of the theories and in the pan-Amazonian evidence are referred to those works. In this discussion, I will stick to the main points and the Yanomami situation.

It was Jane Bennett Ross (1971) who initially proposed that Yanomami warfare was an adaptive response to the limited availability of nutritionally necessary game animals. The general hypothesis, however, has been argued most persistently by Marvin Harris (1974, 1977, 1979, 1984). The basic structure of the theory begins with the observation that it is the male's role to provide meat. As Siskind (1973b) suggested on the basis of her work among the Sharanahua, culturally patterned expectations require that men provide meat to women who provide sex. Game is depleted by hunting, and hunting pressure is a function of village size and the length of time spent in one location. As local game availability declines and hunters experience diminishing returns for their effort, male competition over women intensifies, leading to fighting within groups and raiding between them. Thus—the theory goes—game depletion leads to conflicts over women, which lead to war.

The fact that war makes Yanomami groups move apart from each other is interpreted as having two adaptive consequences: the no-man's-land between enemies acts as a kind of game preserve, allowing animal

populations to replenish themselves; and people in flight may pioneer new or long-unused territory where game is more abundant. A related hypothesis was first proposed by Divale (1970) and elaborated by Divale and Harris (1976): by fostering a male supremacist ideology, war among the Yanomami leads to a devaluation of females, a preference for male babies, and a pattern of female infanticide. Divale and Harris suggest that female infanticide, so induced by war, is a primary means of population regulation in band and village societies.[1]

In one earlier paper (Ferguson 1989b), I examined the first half of this theory—that a limited game supply leads to warfare. After reviewing data from all over Amazonia, I concluded that although there is much evidence to support aspects of the model, there are also reasons why it is inadequate as a general explanation of Amazonian war. Diminishing returns for hunting effort do beset larger, settled villages, and decreasing hunting success is accompanied by a variety of tensions along gender lines. But the typical result is that people move, not that they go to war.

Regarding the Yanomamo specifically, the question of game availability and protein consumption in the Orinoco-Mavaca area has been bitterly contested, without anyone's presenting definitive evidence (see Ferguson 1992a:205). No one disputes that by the mid-1970s, game was almost entirely depleted in the areas most exposed to hunting by mission groups, but it is not clear when diminishing returns and decreasing supply initially set in. I believe they began within a very few years after any Western outpost anchored a local Yanomamo group in one place. Chagnon and Lizot may disagree with me about this timing, but we are all in accord that game depletion is not an expectable result of subsistence practices of Yanomami who are not stuck to a Westerner.

Good's fieldwork (1989, 1991) among a less settled group demonstrates that unanchored Yanomami regularly move around in hunts and between gardens. Helena Valero's two narratives (Biocca 1971; Valero 1984) provide abundant confirmation. Moves respond to a variety of considerations, game availability being a significant one. These movements seem to prevent depletion and ensure a generally adequate supply of meat, without recourse to war. Good (1989:135–40) also describes how a decline in meat sharing in a larger village led to decreased solidarity and tendencies toward fission, without suggesting that a fissioning is predictably followed by war.

Reviewing the case material compiled in this book, there are instances where indications of local game depletion are reported before

a turn to war. Certainly the most significant case is the conflict between Fusiwe's people and Bisaasi-teri over the Shihota location (chapter 11), in which quarrels about meat distribution and animosity over local game depletion are reported. In Chagnon's reconstruction of this conflict, fighting over women is also involved. This one important and unusually well-described case thus demonstrates the full theoretical sequence: (1) problems with game supply, (2) followed by fighting over women, (3) followed by war. But the Shihota case is the only one I know of in which the full sequence is indicated, and even this case of conflict, when considered in full, is more thoroughly and securely explained by considerations of access to Western goods.

In my view, game depletion in areas around Western outposts is an important factor leading to diminished reciprocity and encouraging the instrumental use of force, thus helping to lower the threshold for war. Conflicts related to game and garden produce can also provide the trigger for war between two groups who already have a strained relationship—as can insults, suspicions of witchcraft, or disputes over women. Game depletion can be a contributing factor, but only that, in the occurrence of war. It does not explain why actual warfare breaks out in certain times and places.

In another paper (Ferguson 1989c), I considered the second half of the protein hypothesis—its posited adaptive effects—again using data from all around Amazonia. Although buffer zones and population relocations do result from war, it is not clear that these provide major adaptive advantages that could not also be attained without war. Moreover, war may force populations to concentrate for defensive purposes, and this nucleation may itself lead to less efficient exploitation of game resources.

All these observations appear to be applicable to Yanomami warfare. It has been documented repeatedly that wars end when one group moves away from another. It seems only common sense that sometimes those in flight will enjoy better hunting in their new location, and that a no-man's-land between blood enemies will witness an increase in local game. But there is no evidence that either of these considerations plays a crucial role in the Yanomami's adaptation to their natural environment. Moreover, long-distance movements into new territory, although often spurred by the "push" of war, are much more often motivated by the "pull" of sources of Western goods. And when people have to put up with a diminished game supply in order to preserve access to Western goods, they learn to cope. In sum, there is only a very tenuous

relationship between game depletion, war, and movements that increase game availability.

Regarding the postulated adaptive effect of increased female infanticide, the issue is more complicated. Divale (1970:175) attributes his formulation of the model to a reading of Chagnon, and Chagnon's own descriptions (1967:139–41, 1972b:273–74, 1973:134, 1975:96) repeatedly state that there is a circular relationship among warfare, preference for male children, female infanticide, a scarcity of adult women, conflict over women, and war.[2] What Divale and Harris (1976) do is relate the competition over women to game scarcity and extend the argument to postulate consequences of population regulation.[3]

In my previous paper (Ferguson 1989c:254–255), I argued that evidence, though far from conclusive, does support an association of female infanticide with times of active warfare among the Yanomami—although perhaps not the exact association postulated by Divale and Harris. On the other hand, the evidence does not indicate that such female infanticide leads to stabilization of population, which, as discussed in chapter 4, has been growing rapidly in recent years. My current research found additional support for the existence of preferential female infanticide and its association with times of war. Eguillor García (1984:50–51) documents that the practice does exist in the Orinoco-Mavaca area. Without reference to time period, she reports that of 482 live births, 15 males and 24 females were killed.

Early and Peters (1990:133–37), working around the Mucajaí mission, produced the most comprehensive demographic study done for any Yanomami group. Although they criticize what they see as an exaggerated emphasis on female infanticide, their own analysis tends to support Divale and Harris's position. First, they confirm that "preferential female infanticide is practiced precisely to hasten another pregnancy and birth, hopefully a male" (1990:136–37). This is in keeping with the Divale-Harris proposition that war encourages female infanticide by elevating the preference for male children. Second, their data show that women make up only 22.6 percent of the cohort born between 1914 and 1943 (1990:20–21), a period in which this group was involved in several wars (see chapter 7). The cause of this imbalance is not known, but it is consistent with Divale and Harris's expectations. Third, the study population appears to have been shrinking prior to contact in 1958, a fact clearly linked to the extreme scarcity of women (Early and Peters 1990:19–23, 140), which in turn supports the posited population control function.

Overall, there is a great deal of evidence that the integrated propositions that make up the "protein hypothesis" deal with genuine relationships. Where it is weakest, however, is precisely as an explanation of the occurrence of warfare.

An Eye for an Eye?

Despite their disagreements on many topics, Lizot (1991:68–70) and Chagnon (1988:986) coincide in emphasizing revenge as an explanation for Yanomami warfare.[4] Lizot's explanation invokes Mauss and Lévi-Strauss; it claims that Yanomami fight in the spirit of reciprocity, giving blow for blow. I do not question that Yanomami see war this way. It is part of my model that material interests are converted into moral terms, and always, "they started it."[5] But how can reciprocity explain the change from peace to war and war to peace? How can it explain spatial and temporal variation in the history of conflicts?[6] Lizot's response seems to be that it does not have to, because war and peace are fundamentally the same thing—merely alternative modalities of the fundamental, underlying reality of the spirit of reciprocity. I remain unconvinced that war and peace are essentially the same.[7]

Chagnon (1979a:87, 1988:985–87) avoids these more obvious problems because he argues that wars start over other issues—mainly women—and are (only) continued for revenge. His larger theoretical point is that seeking revenge may confer a reproductive advantage on an individual or a group of kin because a reputation for ferocity may deter others from attacking or attempting to take advantage of them.

Revenge is frequently cited as a cause of "primitive warfare," but its value as an explanation is questionable (Ferguson 1984b:39–40, 1988c:ii–iii). I distinguish revenge—a desire to strike back at someone who has wronged you—from retaliation—a counterstrike against an enemy intended to deter future attacks. In some, but not all, political circumstances, retaliation is a sensible tactic. When a group is retaliating, or even when it is initiating new hostilities, it will use the rhetoric of vengeance to rationalize action and mobilize support (Ferguson 1992a:223–25). Along with other moral themes, especially those of witchcraft and bravery or cowardice, revenge can be used to persuade others to adopt a course of action. I believe, however, that Black-Michaud's (1975:50) generalization—"whatever the rules relative to passive solidarity, the taking of vengeance always tends everywhere to be the affair

of the close agnates of the victim rather than the group as a whole"—applies even within the highly interrelated local groups of the Yanomami.

Except where many different members of a local group have similar losses to avenge—such as after the Shamatari's killing of as many as 15 Bisaasi-teri men (chapter 11)—a blood debt alone will generally not lead to war. In a great many cases reported in this book, perhaps even a majority, a killing is not followed by any counterraid within the next few years. Chagnon probably would not restrict the time frame that narrowly: "Vengeance motivation persists for many years" (Chagnon 1988:986). The only case he cites to substantiate that claim, however, is the 1975 raid by Bisaasi-teri on Patanowa-teri, supposedly intended to avenge the 1965 killing of the Monou-teri headman. This is the case discussed in chapter 14, where it was pointed out that Chagnon had originally stated that the blood debt went in the opposite direction—that it was the Patanowa-teri who needed vengeance. Thus the actual behavior is the reverse of what was specifically predicted in earlier publications.

Moreover, I found numerous incidents described in the literature in which a claimed need for revenge was obviously manipulated. The demand for vengeance can be trumped up, as when the Monou-teri needed to avenge the Patanowa-teri's repossession of five of their own women (chapter 13). It can be forgotten and then suddenly remembered when politically convenient, as it was by Chagnon's friend Rerebawa regarding the Shamatari (chapter 14). It can even be fabricated out of whole cloth, as happened during recent raids by mission Yanomami on the Yanomamo of the Siapa region (chapter 14).

Chagnon makes the general point that physical revenge may be sought for an imputed sorcery killing, so that "even in the absence of active military contest, Yanomamo groups constantly generate mutual hostility" (1972b:259). A sorcery accusation, of course, is virtually a "strain gauge" (Marwick 1970) for the tenor of relations between groups, a point Chagnon (1977:118) makes himself.

In my view, the Yanomami control revenge; they are not controlled by it. For people other than the victim's very closest kin, revenge is a real but highly malleable motivating factor. There are frequently many dormant reasons for seeking revenge, and if none exists, some can be made up. They come to the fore in conflict situations because that is one way to frame materially self-interested actions in moral terms. Vengeance is "good to think" and good to persuade.[8] But a focus on vengeance will not elucidate why wars happen. Nor do I think that those who take revenge are doing themselves a reproductive favor. That point, however, will be considered in a later discussion.

Fighting over Women

There is no question that Yanomami men on many occasions fight over women; there is no doubt that such fights sometimes lead to war. What is in dispute is the theoretical significance of this fighting and, in particular, its utility in explaining war. Chagnon, of course, is most closely associated with the position that Yanomami go to war over women (e.g., 1968:123, 1992b:112–14). In his doctoral thesis, after noting a shortage of sexually available women resulting from female infanticide, polygyny, and postpartum sex taboos, he offers the hypothesis: "The frequency of intra-village fighting and intervillage warfare co-varies with the frequency with which males may legitimately satisfy sexual desires" (Chagnon 1966:2).

From the middle 1970s onward, Chagnon's position became encompassed within his evolving sociobiological perspective, in which Yanomami fighting was seen as a form of sexual selection—part of a broader process of competition for mates aimed at maximizing inclusive fitness (Chagnon 1979a:87–89, 1983:86, 1987:29, 1990b). Chagnon acknowledges that men will compete and fight over material resources when they are scarce, but he argues that when resources are abundant, men will compete and fight over women and reproductive success. It is the latter situation, he maintains, that applies to the Yanomami (1979a: 87, 103–104, 1979b:375–77, 401, 1980:123, 1981:507, 1990b:82).

In a previous paper (Ferguson 1988b:151–52), I questioned whether there is evidence to support Chagnon's (1980:123) proposed inverse relationship between material resource scarcity and (1) polygyny and (2) fighting over women, either among the Yanomami or elsewhere. In the discussion to follow, I will consider first some other questions related to the connection between war and fighting over women, and then the issue of whether aggressive Yanomami men actually do increase their reproductive success.

It is my impression that the anthropological and general publics misunderstand Chagnon's position. Judging from countless conversations, I believe most people think Chagnon's argument is that competition between men is the cause of raiding to capture women. But Chagnon (1977:123) asserts that raids are *not* initiated in order to capture women, although once raiding begins, that possibility is an additional incentive. Rather, Chagnon's central theory (1979a, 1982:305, 1988:986) appears to be that competition over women within a local group leads to fissioning, and fissioning frequently leads to war between segments.

Certainly such sequences of events do occur. But recently fissioned villages go to war only in some cases, and local groups often divide without any hint of a major fight over women. Thus Chagnon's most detailed theoretical discussion can apply only to some fraction of reported wars. To that number could reasonably be added cases where intermarriage between villages is followed by a fight over women that leads to war. But most such conflicts concerning marriage do not result in war, and I suggest they might more appropriately be considered as the operation of law. Even judging from the most inclusive number of cases, for many Yanomami wars we have no indication of any conflict over women antecedent to the outbreak of violence.

In a recent summary exposition, Chagnon's expectations about behavior are framed very broadly: "I am simply arguing that conflicts of reproductive interests occur commonly in band and tribal societies and that these often lead . . . to intergroup conflicts that we traditionally consider to be warfare" (1990b:82). Whether conflicts over women should necessarily be considered "conflicts of reproductive interests" will be discussed below. But if I understand Chagnon's position correctly, all he predicts about behavior is that in the multiple and complicated interpersonal and intergroup relations that precede the outbreak of war, it is common to find reports of a fight between men over women. As an empirical generalization, this prediction cannot be disputed. But it still applies only to some cases of war, and even in those cases it is typically only one of several antecedent grievances.

Consider the two cases Chagnon picked to illustrate his position. One (Chagnon 1990b:96–97, 104) involves a disputed reclassification of potential marriage partners in 1960, which eventually led to a village's fissioning. (The reclassification is apparently the incident described in Chagnon 1977:87, but which fissioning it refers to is not clear.) After fissioning, the two divisions had other, similar quarrels, and a rape in 1986 led them to the brink of war. But instead of going to war, they dissipated their hostility in a club fight. Chagnon (1990b:97) concludes that "*if* a 'war' develops" (my emphasis) then it would be "misleading to argue that reproductive striving is irrelevant to understanding the development of that war." The other illustrative case (Chagnon 1990b: 98–101) is the incident that took place around 1979 when the old Bisaasi-teri headman, Kaobawa, was pelted by Tayari-teri children, an insult that was followed by war (chapter 14).

In his discussion, Chagnon (1990b:101) states that neither of these two conflicts can be attributed to one isolated provocation—that they

are "continuations of smouldering antagonisms that originate in a multitude of previous acts," including seductions, male competition over females, insults, status testing, and a desire for revenge. He concludes that "it is relatively easy to relate all of these variables to reproductive striving" because, he claims, a village that fails to respond aggressively to any slight will be victimized and lose women.

Neither of the two cases Chagnon chose to exemplify his theory provides any support for a posited connection between fighting over women and war. In the first, a disputed marriage reclassification a quarter-century earlier is said not to be "irrelevant" to a war that could have happened (but didn't) in 1986. In the second case, a war did erupt after a series of insults, but none of the sources describing this war refers to any conflict over women having been involved. As Lizot, who was there, points out (1989:28): "The element 'competition to obtain women' is totally absent in the initiation of the hostilities and the development of the crisis."

Thus the operative point in Chagnon's exposition becomes that it is "easy" to relate any and all fighting to "reproductive striving," because aggressive behavior itself is believed to have a reproductive payoff. I will consider that issue in a moment. For the current issue—whether Yanomami warfare can be explained with reference to fighting over women —the two cases Chagnon has selected in fact make opposite points: disputes over women can occur without leading to war, and wars can occur without any triggering dispute over women.

My own position regarding men fighting over women has several components. In an earlier comparative study (Ferguson 1988b:148–52), I noted that regardless of what causes war, the Yanomami stand out among Amazonian societies for the political prominence of their fighting over women, and I suggested several underlying features that may be responsible for that distinction: the unusually limited basis for female cooperation in an economy reliant on plantains rather than bitter manioc; the existence of strong fraternal interest groups in some areas; and the ideological reinforcement associated with unusually intensive warfare. In my article on "warrification" of the Orinoco-Mavaca Yanomami (Ferguson 1992a), I identified additional factors: the atypical number of marriages between villages, which make women more significant as political symbols and leave them more subject to abuse; and the high number of deaths from disease and war, which in an otherwise disturbed environment encourage the instrumental use of force to decide marriage arrangements. (Ramos [1979c:186] emphasizes that

the picture of male dominance presented by Chagnon does not apply to Yanomami in other areas.)

In this book I have been concerned with marriage primarily as the capstone of a total relationship between groups. The tone of that relationship is determined in part by each group's ability to apply force, but also and more fundamentally by the distribution of Western goods. From my perspective, it is the total relationship that is at issue in any fight. In a good relationship, many things can be overlooked. In a bad one, any dispute can trigger violence. The detonator may be food, status, or sorcery, but fights will frequently be "over women" because exchanged women are the ultimate medium of alliance.

Striving for Reproduction

A further difference between my position and Chagnon's is that I do not accept the presumption expressed in his sociobiological writings that conflict over women is in itself evidence that male behavior is motivated by "reproductive striving." Men may fight over women for reasons other than maximization of inclusive fitness. Yanomami women may be part of a contract between groups who have definite expectations about trade and political support. Women are laborers, and their value may increase when they have the option of working for missionaries. They are sex partners—a role quite distinct from that of being a mother. Obviously, sex leads to reproduction, but what Chagnon is (or at least was) arguing is that reproductive success in itself is a goal with a direct impact on individual behavior.

The proposition that people deliberately act in ways that maximize their inclusive fitness is the most distinctive and debatable point in human sociobiology. Chagnon (1987:29) holds that "Yanomamo males are tracking their environment with their own fitness interests at stake," and they "manipulate and adapt to [this environment] in striving for reproductive success and maximal inclusive fitness." In earlier work (Chagnon 1979a:128; Chagnon and Bugos 1979:223), Chagnon held this motivation to be unconscious but strongly determinative—comparable to the effect of gravity on a falling rock or to planetary motion. In recent theoretical statements (1988:985, 1990b:79, 81), he argues that there are two kinds of resources people strive for, two kinds of human effort, two kinds of competition: somatic and reproductive.

Thus there is no ambiguity in the hypothesis Chagnon has advanced

for over a decade: a desire to maximize inclusive fitness is itself a major factor shaping Yanomami behavioral decisions. Yet in a rejoinder to me (Ferguson 1989d), Chagnon (1989b:567–68) introduces an entirely different proposition: "I maintain that it is useful and legitimate to investigate the possibility that material gain might possibly be turned into reproductive benefits . . . that is, I am interested in ultimate (reproductive) consequences, not just proximate (immediate material gain) consequences." This distinction between ultimate and proximate goals is very different from distinguishing between alternative goals, and it gives rise to a completely different set of theoretical expectations and understandings.

I find nothing to argue about in the proposition that individuals tend to their material self-interest and that in evolutionary perspective, such self-interest has a generally favorable impact on reproductive success.[9] But it remains very much in question whether aggressive behavior itself can be seen as a reproductive strategy (see Moore 1990), even among the Yanomami.

The main evidence for aggression's having a reproductive payoff appears in Chagnon's (1988) controversial article in *Science*. In that piece, Chagnon asserts that *unokai*, men who have undergone the purification ritual for killers, have considerably more wives and children than do other men. His position was immediately challenged by Albert (1989:638, 1990a:559–60) and Lizot (1989:33), who claim that the status of *unokai* is not an accurate marker of men who have killed. Chagnon (1990a:49–50) replies that the way he collected the data, it is. I leave this argument to those with the necessary linguistic competence and field experience.

My own dispute with Chagnon (Ferguson 1989d) has been about whether his statistical data show that *unokai*, as Chagnon defines them, really do have greater reproductive success (and see Albert 1990a:560–61). These are the only published data relevant to his claim that taking revenge is adaptive because those who do so are less likely to be attacked, and to his more sweeping claim that a demonstrated willingness to fight contributes to reproductive success by deterring the aggression of others.

The way in which Chagnon has characterized his findings in recent publications is not, I believe, what his statistics show. Chagnon (1990b:95, and see 1992a:205, 1992b:239–40) claims his research demonstrates that *unokai* have more than three times as many children as non-*unokai of the same age*. His data, in contrast, show that this 308-percent

difference in number of children is derived from the total sample, *not* broken down by age. This is a significant distinction. His table, divided into four age categories, makes clear that "success" in both killing and reproduction is associated with age: younger men are less likely to have killed or had children. As I have pointed out previously (Ferguson 1989d:564), in the two older age categories—which include 86 percent of all *unokai*—the number of children reported for *unokai* men shrinks to 140 percent and 167 percent of the number reported for non-*unokai* men. Differences of such magnitude could still represent a substantial reproductive payoff for "killers," but closer examination calls that inference into question.

In my previous critique (1989d), I raised three questions about Chagnon's data and inferences. One was that these apparent differences in *unokai*'s reproductive success might represent a spurious correlation. As Chagnon (1988:988) notes, all the headmen in the sample are in the *unokai* category. An exceptional tendency toward polygyny by political leaders, with or without war, has been an axiom of Amazonian ethnography for at least half a century.[10]

This is the only one of my three points that Chagnon seriously addresses in his rejoinder (1989b:566). Reanalyzing the statistics with headmen factored out, he reports that there remains a statistical relationship at the 0.05 level of significance in all but one age category. He concludes: "I would not care to argue, given these data, that living *unokais* among the Yanomamo have *fewer* offspring than non-*unokais*." This assertion of a statistically significant relationship between *unokai* status and marital and reproductive success is a far cry from the claim that *unokai,* "compared to same-age non-*unokai,* have over twice as many wives and over three times as many children" (Chagnon 1990b: 95). But if Chagnon's reanalysis supports my point that the inclusion of headmen skewed the results, it still does not establish that aggressive men have even a marginal statistical advantage in reproduction, because of other problems with the data.

My second objection (Ferguson 1989d:564) is that some of the apparent correlation of *unokai* status with higher numbers of wives and children may be a result of covariation with age *within* the four age categories. A 40-year-old man, for example, is more likely to be *unokai* and to have more children than a 31-year-old man. Chagnon's (1989b:568–69) response is that precise estimates of age are impossible, and so lumping people into categories is necessary.[11] Perhaps so, but that does not address the problem. Given that the correlation is drastically reduced by

factoring out headmen, this additional source of potential bias raises the question whether any relationship, even a statistical one, really exists.

The third problem is the most serious. For reasons Chagnon does not explain, his data on reproductive success (1988:989) do not include "living children whose fathers are dead." I question the impact of participation in a killing on the likelihood of being killed (Ferguson 1989d: 564): Does the average *unokai* live and breed longer than the average non-*unokai*? After compiling the case material presented in this book, I emphasize this question even more (and see Albert 1990a:560–61).

Most of the men identified as war leaders were killed in war, including Ruwahiwe of the Konabuma-teri, Fusiwe of the Wanitima-teri, Rashawe of the Bisaasi-teri, Riokowe of the Iwahikoroba-teri, Kohawe of the Shitari, and Damowa of the Monou-teri. People were expecting Helena Valero's second husband, Akawe, to be killed before he fled to the world of the *nape* (Valero 1984:471). Moreover, at least one of Riokowe's children was killed by his enemies, and Valero had to flee to prevent the same from happening to Fusiwe's children. Only Kaobawa's rival, Paruriwa, seems to have prospered after leading several raids, and he did so by securing the support of the Salesians and obtaining a shotgun. All this evidence suggests that unusually active leaders in violence could lose so many reproductive years that it would diminish their lifetime reproductive success.

In his response, Chagnon (1989b:566) acknowledges that this is an important issue and adds one new item of information and inference. He claims that one headman—Moawa of the Mishimishimabowei-teri, now dead from unknown causes—had killed an extraordinary number of people (21 or 22) and left no living children. He concludes: "Being *excessively* prone to lethal violence may not be an effective route to high reproductive success, but, statistically, men who engage in it with some moderation seem to do better reproductively than men who do not engage in it at all." In short, adding in deceased men and their offspring could lower the *unokai*'s measured reproductive advantage; it is certainly within the realm of possibility that *unokai* men would be found to have fewer offspring than non-*unokai*.

Chagnon (1989b:566) states that he now has the data to address this question, collected during fieldwork that he carried out after completing the *Science* article, and "as my schedule permits, I will publish them." He reassures us that "while I have not completed the analysis of these new data, my impressions of how they are shaping up give me little reason to believe that my initial suspicions [that *unokai* are not

at greater risk of violent death] are wrong" (1989b:566). At the time of this writing, over four years have passed and the new data have not yet appeared in any publication with which I am familiar. When they do, it may be possible to begin to answer the question of whether killing another person has the effect of increasing the lifetime reproductive success of Yanomami men. That question cannot be addressed with the information provided so far. At present, there simply are no data that substantiate the claim that aggressive behavior is associated with reproductive success among the Yanomami.[12]

A Demographic Pump?

In recent fieldwork in the highlands of the Siapa region, Chagnon (1992a:82–86) found that in contrast to the warlike peoples of the Orinoco-Mavaca area, the Siapa people were gentle and sedate. They also differed in several of the correlates of war, having smaller villages, less elaborate alliances and so less feasting, and fewer marriages based on abduction or coercion. When there was coercion, highland women went to lowland men.

To explain this contrast, Chagnon invokes ecological differences in altitude, terrain, and the fordability of local rivers. The lower lands near the Orinoco are said to be richer in "game animals, plants for food, construction and manufactures, and well-drained easily cultivated lands for gardens" (Chagnon 1992a:83). In the mountainous areas, it is much more difficult and costly just to keep alive. On the other hand, the wider Orinoco and lower Mavaca and Siapa rivers are difficult to cross, so Yanomamo generally avoided settling there until the advent of the missionaries. "These low, flat areas in regions where the rivers are small and easily crossed are the regions that Kaobawa's people—and many other groups—appear to have preferred as settlement locations for the past 150 or so years; and these areas are dotted with hundreds of long-since abandoned gardens—more than 500 of them" (Chagnon 1992a:83).

According to Chagnon, it is competition for prime resource land that explains the political tone of the lowlands.

> Groups that live in the lowlands have to be large and bellicose in order to control the large, desirable, and wide-open ecological niche they live in. They seem to keep their neighbors at a comfortable distance by adopting an extremely bellicose strategy that entails frequent raiding and chronic attempts to either abduct women from their neighbors or coerce weaker neighbors into ceding more women than they ultimately "repay" via marriage alliance agreements. To be effective at this, they must maximize village size. (Chagnon 1992a:87)

The problem, as he explains it, is that for other reasons, villages tend to fission after reaching a certain size. In the local atmosphere of violent competition, the new, smaller groups have three options: they can live close together to maintain an advantage in numbers; they can move into more marginal highland zones; or they can pioneer into vacant lowlands. The second option is the most relevant for Chagnon's recent observations. He posits a repeated pattern in which groups are pushed out of optimum areas such as the Shanishani drainage, longtime home of the Patanowa-teri. The Shanishani area is said to have functioned as a kind of "demographic pump" (Chagnon 1992a:88), spewing groups westward and southward through the Siapa region.

This argument represents a surprising turn to cultural ecology after Chagnon's years of sociobiological theorizing. It does not invoke reproductive competition, and although it contains no suggestion that war may be related to differential access to Western goods, Chagnon's new approach does include many points that are convergent with positions I have argued in this book.

Chagnon (1992a:89) acknowledges the "possible" role of sources of Western goods in pulling Yanomamo southward across the Siapa River. He acknowledges (1992a:86) an inequality in relations between people in areas of greater exposure to Westerners and those in the more isolated hills—an inequality that involves a unidirectional ceding of women. He acknowledges (1992a:209–10) that the most isolated Yanomamo appear to be the most peaceable. He describes (1992a:220) a recent wave of wars that pitted mission groups against "wild" groups. And he posits a "demographic pump" (1992a:88) that sounds very much like an idea I proposed in an earlier paper (Ferguson 1989c:255), following Steward and Lathrap—the idea of a prehistoric "population pump" whereby war pushes people from prime lowland resource zones into mountains and other marginal areas.

There is, however, one seemingly insurmountable problem in applying a model of pre-Columbian population movements to the historical Yanomami. Whether or not the Yanomami of the past 150 years "preferred" to live in low, flat areas, where they *actually* lived until recently was not in the low flatlands but in the Parima and Siapa highlands.

Chagnon's proposed pump, pushing Yanomami from lowlands into highlands, may fit certain local situations during the twentieth century, but as a general process, population movements have been from higher to lower ground.

Another evidentiary problem confronts the suggestion that people in the lowlands are warlike while those in the highlands are peaceable. That frequently is the case because lowland groups are usually more exposed to Westerners, and the highlanders are more isolated. But we have seen numerous instances where groups with greater exposure to sources of Western goods are pushed farther into low country by attacks from more isolated groups from higher elevations. On several occasions, war has broken out between highland groups after a Western presence was established there; and several lowland groups, under facilitating circumstances, are reported to have been quite peaceable. Thus the lowland/highland dichotomy is less adequate as an explanation of war than are circumstances of access to Westerners.

Emics and Agency

The point of this book has been to understand why war happens. Not war as a quality of a species, not war as an abstract cultural pattern, but war as actual practice: war in which real people die at particular places and times. My explanation is a highly deterministic model applied to observed behavior. But real wars are not carried out by models. What about the real people? Have they no say in what happens? And if my model is right, why has no Yanomami ever offered it as an explanation of why he fights?

Several years ago (Ferguson 1984b:38–42), I called attention to the many problems involved in eliciting emic statements about the motivations that lead to war, and I advocated instead an approach that infers motive from action—an etic, behavioral approach. But the question of the Yanomami's stated reasons for war deserves a direct answer.

One answer might be that the Yanomami do not want the *nape* to know why they fight. It is well documented that they are skilled at misleading outsiders—witness their concealing deadly raids from missionaries who loaned them shotguns, or their countless tales of ferocious killer Waikas upstream, or their five months of comedy at Chagnon's expense over the fake genealogies. All Westerners among the Yanomami experience their nearly overwhelming demands for manufactured goods; those demands cannot be concealed. But how would missionaries and other well-meaning Westerners react if their local friends explained

that people were being killed over them? Revenge, fights over women, and witchcraft, on the other hand, are all acceptable as local culture—something the stranger is there to study or reform.

Although deliberate manipulation of outsiders may be one reason why Yanomami do not offer conflict over Western goods as an explanation of their fighting, I would not push this too far. Helena Valero's two narratives provide an insider's perspective on some wars and show that the Yanomamo do not talk in terms of my model even among themselves. Indeed, if one read those two accounts and nothing else, one would probably see Napoleon Chagnon's views on war as more accurate than mine. Valero's husband Fusiwe—unusually polygamous, highly aggressive, ready to respond violently to perceived insult—is virtually the Chagnonian ideal. But his behavior does conform to the patterns I have argued and documented, and there are indications that his real motives for war remained unspoken.

Fusiwe began wars without any reported reference to differential access to Western manufactures. He justified the killing of Ruwahiwe as revenge for sorcery. But after the slaughter, his kinsman Repowe accused Fusiwe, in the logic of the concrete, of killing the Shamatari because he was left out of the trading of machetes for dogs. Later, when Bisaasiteri provocations and his youngest wife's jibes goaded him into starting a war, Fusiwe lied about his intentions to his own people, while Valero and others openly speculated that he was pursuing some hidden agenda.

But the most revealing incident in Valero's narrative occurs with her second husband. Fearing for his own safety and wanting the largesse of the *nape,* Akawe goaded other men with accusations of cowardice, inciting them to take revenge on the Shitari. It was a sham—a ruse to give himself and Valero an opportunity to flee to the whites. The only reason Valero learned of the deception was because she was part of the plot.

The position advocated here is that material interests are converted into moral points for public discourse. Everyone knows the existing circumstances and what may be at stake. But basic interests, even matters of life and death, can only become realities in a social world. Everything else in life is piled on top of those basic interests: family, politics, status, and, above all, a value system. To behave as a pure "economic man," openly and rationally weighing costs and benefits to decide on war, oblivious to this highly textured social reality, would be, in a word, crazy.

Public discussion invokes collective values to persuade, to put pressure on those with mixed feelings, to make someone's self-interest seem an expression of moral principle. In the process, unadorned material

interests are put into an idiom of normative behavior—an essential step toward any course of social action but especially one in which a person may be called upon to kill a relative or former friend. But any political discourse, however well argued, will persuade others to accept the risks of war only if the message is consistent with the listeners' self-interests.

I strongly suspect—without any means of proving it—that proponents of moral arguments believe in the morality themselves, at least to some degree. Perhaps this is how culture works in general. There are comparatively few people, I believe, who think of themselves or their actions as deliberately "bad," no matter how reprehensible others may find them. Morality, well cooked, provides the means for translating "need" or "want" into "right." Because actions in war often grossly violate established norms of behavior, the pressure to rationalize must be great. Combine these points with the idea that when a total relationship is at issue, any component can stand for the whole, and the dialogues recounted by Valero—along with the explanations given to anthropologists—make perfect sense.[13]

Is there any role for agency in understanding Yanomami warfare? Yes, at two levels. If by agency one refers to individual human beings making choices that shape the course of history, rather than simply acting out the forces that work through them, this study allows some specification of its scope. Again, Helena Valero provides the necessary details.

Valero tells us that Fusiwe personally, though not alone, initiated three violent conflicts: the early failed raid against the Shamatari, the slaughter of Ruwahiwe's party, and the war against the Bisaasi-teri in which he died. Fusiwe was able to do this because of a combination of situation and structure. The situation was the rising antagonism associated with the times. As danger increases, others listen more closely to the man with the aggressive personality—the war leader. The structure was his position as headman of the Wanitima-teri. He had supporters to lead into war. The equally aggressive but junior Akawe, in contrast, had the situation but not the structure. He traveled from group to group, a bow for hire by anyone who would feed him or offer him a wife.

What difference did Fusiwe make? In my view, there was a strong probability of war involving the Namowei at this time, just as war was breaking out all over on the other side of the Orinoco. But the enemy and alliance pattern could have gone very differently. Around 1948, Fusiwe, virtually on his own, started the war between the eastern Namowei and the Bisaasi-teri at Shihota. Had the more peaceable Repowe prevailed, the eastern Namowei might have reestablished peace, perhaps by ceding

women to the Bisaasi-teri as the Patanowa-teri did later. Maybe then the Namowei as a group would have found themselves going to war against others, such as the Hasupuwe-teri. The underlying structure of antagonisms made some warfare highly likely, but it remained for political leaders to actualize the possibilities.

Agency can also be identified on another level: that of indigenous people's actively shaping their collective history. The Yanomami are not passively molded by contact with the outside world. They have aggressively pursued their own interests. They have shaped a political milieu in response to the intrusive Western presence, which itself is largely beyond their control. Indeed, one could say that along the banks of the Orinoco, it was the Yanomamo who made contact with the outside world, rather than the reverse. That the result of Yanomami maneuvering is often quite terrible for themselves is no surprise, given the circumstances they face and their fallibility as human beings.

Some Broader Implications

In this final section, I will attempt to apply more broadly some of the themes developed in this book, starting with the issue of how anthropologists approach their subject matter. Although it is fashionable to decry scientific approaches and causal theories in the study of culture, and although cultural materialism is derided in particular, the view of Yanomami politics developed here grew out of an application of the cultural materialist principle of infrastructural determinism (see Ferguson n.d.*b*). That theory directed me toward steel tools—items the culture of anthropology has somehow relegated to insignificance. And although this study might be perceived as similar to other recent challenges to established ethnographic portrayals, it is no exercise in reflexive contemplation of our own discourse. It is, rather, a reaffirmation of the value of the comparative method.

One of the biggest problems facing anthropology today is a lack of strong theory, comparatively applied. The protein hypothesis—although I disagree with it as an explanation of war—has been a tremendous stimulus for research, and our understanding of Amazonian ecology is much richer because of it. A criticism of this book, I feel sure, will be that the author did no fieldwork among the Yanomami. But as the persevering reader will now appreciate, a huge amount of material is already out there, all based on firsthand experience. What has been done with

it? Helena Valero's narratives are incomparable ethnographic sources, but no one has made serious use of them. Anthropology is filling warehouses with information. Can we not use this accumulating knowledge to generate better theory?

My hope is that the theory developed in this study could be part of a more comprehensive understanding of war. The model I develop and apply to Yanomami warfare is an application of a more general approach, tailored to the particulars of the case. There are a vast range of "warrifying" situations in tribal zones (Ferguson 1990b; Ferguson and Whitehead 1992a), and my present model would require major modification if it were applied to any but the most similar of them. With other applications of the general approach, it would be possible to better understand major parameters and permutations in war situations. It might even be possible to work toward a general understanding of social conflict, spanning the spectrum from war to witchcraft accusations and on to revitalizations, ethnic violence, class conflict, and revolution. A distant ideal to be sure, but any progress toward it would be commendable in a world facing a constantly changing and seemingly worsening panorama of violence.

Another issue arises from the different superlatives that can be applied to the Yanomami as a case study. Although I have stressed the role of Western contact, it is nevertheless true that the Yanomami have been more isolated from Westerners longer than any other large group of Native American people. They stand as the end point—the outer limit—of the post-Columbian, New World tribal zone. For others with more history of exposure to Westerners, the impact of Western presence should be even greater than that described here.

The Yanomami have also acquired in popular and some anthropological literature a reputation for being the most warlike people on earth. This study shows how misleading such statements can be. Some Yanomami in some places and times have been extremely warlike, but most Yanomami in most places and times have been peaceable. And the violent periods, I have argued, are caused by circumstances introduced by intrusive Westerners. Considering the Yanomami's "most remote" status, analysts should proceed with caution in alleging that any observed warfare among nonstate peoples anywhere is a purely indigenous pattern.[14]

For another superlative, the Yanomami are one of the "simplest" societies for which warfare has been well described. Contrary to some recent thinking, I see no contradiction between historical explanation

and evolutionary comparison. My teacher Morton Fried (1967, 1968) developed his evolutionary theory in connection with an investigation of the postcontact generation of tribes. Evolutionary comparison, as I understand it, does not presume any people to be pristine, unchanged survivors of the Stone Age. Rather, it uses ethnographic description to explore the implications of organizational or other features associated with different levels of societal scale and complexity.

The Yanomami show us war at its smallest and with the least amount of political structuring piled on top. The political process of Yanomami warfare—all its discussion, deception, alliance building, military recruitment, and so on—is probably our clearest window on how war is carried out in any situation where people live in small, shifting groups without any basis for fixed or authoritative leadership. Although it is my position both for the Yanomami and in general (Ferguson 1989b:197, 1993) that war is an infrequent occurrence among small, relatively mobile groups who have not been destabilized by a tribal zone, to whatever extent war was waged by such groups in the distant past, it was probably waged something like this.

Yet at the same time, Yanomami fighting, when we look beyond the paint and feathers, seems not unlike war as practiced the world over. A contemporary military analyst, made appreciative of the practicalities of Yanomami existence, would have no problem understanding the logic and practice of their warfare. Thus the Yanomami as a case argue against the idea, framed by Turney-High (1971) but accepted by many anthropologists, that "primitive" and "civilized" war constitute two qualitatively distinct categories. On the other hand, Yanomami warfare is very different in that its small scale allows it to be studied in its full social context, within which major permutations can be compared. That goal is far beyond the reach of even the most massive research projects directed at modern warfare.

I have argued (Ferguson 1984b:1–2) that it is the possibility of developing a more complete theoretical picture that constitutes anthropology's greatest potential contribution to understanding the human problem of war. In that spirit, several inferences with contemporary relevance may be drawn from the Yanomami case. First, war is not a natural state of affairs for human societies. It is not the normal condition, leaving peace the state that needs to be explained. Yanomami do not go easily into war or stay there long, although their proneness to war does vary with local history. It took several years for the Bisaasiteri and eastern Namowei to go from peace to war.

Second, war is not self-perpetuating. The costs are too high. But war can be self-reinforcing. In combination with other pressures toward violence, war itself lowers the threshold for war and puts into positions of greater influence men who are prone to use military force. From a regional systems perspective, the introduction of war may select out the possibility of nonviolent resolution of antagonisms (see Ferguson 1990a: 29, 1993). In sum, opting for war makes future war more likely, but something else is always involved.

Third, there is nothing in any of the accounts of war between Yanomami communities to suggest the "tribal loyalties" so frequently invoked by pundits trying to explain this or that conflict in the modern world. Indeed, the in-group amity/out-group enmity often posited by ethologists and sociobiologists seems here a most fickle sentiment, when amity and enmity regularly shift within the same social universe. The Yecuana-Yanomami relationship exemplifies how cultural differences that coincide with fundamental material antagonisms can provide definition to existing hostility, but only that. The cultural differences are not the cause of the conflicts.

Fourth, a "negative image of the other," sometimes suggested as that dimension of war on which anthropology can shed some light, is itself merely an expression of conflict, not its cause. Thus the eastern Namowei, already pushed to the brink of war, decided that the Bisaasi-teri were not true Namowei after all, and that is why they caused so much trouble. But this recategorization was to explain what was already happening. Any attempt to understand why war occurs that takes negative images of the enemy as its main focus is putting the cart before the horse.

Fifth, war is bad business. I argue that war is initiated because those who decide on war think they will be better off fighting than not. Yet it does not always work out that way for the decision makers. When the total costs for everyone involved are tallied up and compared with possible outcomes of conflicting interests in the absence of war, war's net effect is destructive, any possible adaptive benefits notwithstanding (see Ferguson 1989c:258).

Sixth, understanding war requires close scrutiny of those who make political decisions and what their interests may be in a given situation of war or peace. This requires an understanding of the dynamics of political process, which in turn requires attention to levels of evolutionary complexity. In relatively egalitarian societies like the Yanomami, almost every man can make up his own mind whether to fight, based on his own evaluation of circumstances. At the other end of the evolutionary scale, in states, decision-making ability is very unequal: men can be compelled to go to war, and interests in any political situation will vary according to a person's position within the structure of stratification.

Seventh, and finally, when studying decision makers, it should be remembered that they often lie. They conceal their true motives. They skillfully employ commonly held values to make their favored course of action appear to be a moral imperative in the interest of everyone in their society. But perhaps more insidious than plain manipulation is that decision makers may very well come to believe their own self-serving justifications. Critical examination of any moral claim by any political leader justifying war is always in order.

Chapter 15

1. All this theorizing relied heavily on Chagnon's first portrayals of the Yanomami and their wars, including the prominence of conflicts over women. Thus Chagnon (e.g., 1983:86, 1992a:95) misrepresents the protein hypothesis by claiming it denies that Yanomami fight over women. This assertion is contradicted by numerous clear statements of his opponents' theory (see Ferguson 1989b:180). The debate between advocates of the protein hypothesis and Chagnon is not about whether Yanomami fight over women, but about what theory best explains that fighting.

2. After his turn to sociobiology, Chagnon (Chagnon, Flinn, and Melancon 1979:308–309) dropped all reference to this circular relationship, as well as his previous claims that sex ratio at birth could not be accurately determined. He began to argue instead that Yanomami had a live-birth ratio of about 129 males to 100 females. After considering various reasons for doubting this proposal, I concluded (Ferguson 1989c:254) that "unless strong new evidence is presented to support the skewed-ratio-at-birth hypothesis, it can be rejected."

In his recent work, Chagnon (1992a:93) claims that he stopped publishing about female infanticide after 1985 because some Venezuelan politicians wanted to use his findings to prosecute Yanomami for murder. That claim does not accurately reflect the development of this debate, as just described. It does, however, confirm that even Chagnon has dropped the sociobiological skewed-ratio-at-birth hypothesis. While I certainly sympathize with his ethical dilemma, his silence now obviously does not help resolve the scientific questions.

3. Chagnon (1992a:96) again misrepresents his opponents' position when he asserts that they claim "a woman will kill her own newborn to make the

jungle more productive in monkey protein for future members of the group." Regarding the motivation for female infanticide, advocates of the protein hypothesis merely took Chagnon at his word.

4. It is worth noting that for all their differences, Lizot and Chagnon (1974: 77, 1977:163) agree on another fundamental—and empirically unsupportable—assumption: that warfare "is endemic *among all primitive peoples*" (Lizot 1979:151, emphasis in original).

5. But I do not agree at all with Lizot's (1991:62) dismissal of the often outrageous demands for Western goods that permeate trade dialogues as being nothing more than figures of speech to remind the other of the necessity to exchange.

6. In another discussion, Lizot (1989:31–32) makes several points with which I am in complete agreement: that a variety of motives go into any war, that the final incident that leads to a war may be merely the detonator in a relationship gone bad, and that understanding war requires close examination of village histories and intervillage relations. What Lizot does not offer is any key to explaining major variation in those histories and relations, or why they sometimes get to the point of violent detonation.

7. In a footnote to this discussion, Lizot (1991:72) levels several criticisms at a manuscript version of my paper "A Savage Encounter" (Ferguson 1992a). One is that I approached my study with a theory already elaborated. In a broad sense, that is true, although the general theory was critically reevaluated and specified through examination of Yanomami case material. But it is difficult to understand how Lizot can mean this as a criticism when his own approach is a straightforward application of the ideas of Mauss and Lévi-Strauss. Lizot also accuses me of selective presentation of data and ignoring contrary information, although he offers no specifics. I do not accept the criticism in regard to that article, and it certainly cannot be said of this book, which applies my theory to every single case of Yanomami warfare that I could find.

8. The idea that "tribal" peoples make war for revenge seems to be part of the broader conceptual divide implicit in our dichotomies between simple versus complex, nonstate versus state, and, especially, primitive versus civilized war (see Turney-High 1971). It would sound ludicrous to suggest that World War II was a continuation of World War I for the sake of revenge, but if the scale were reduced to the Yanomami level, that characterization probably would be applied. The role of revenge in the conflicts of nonstate people may actually be analogous to the "sense of history" in modern societies and equally subject to political manipulation.

9. Chagnon (1989b:567) challenges me to clarify whether I believe that "humans are designed by natural selection to make choices that generally increase their material benefits." I do (see Ferguson 1984b:37–38). But no one could possibly doubt that humans are motivated to maintain the resources and safety needed to survive, whereas it is entirely hypothetical that they are motivated by an unconscious reproductive striving. My skepticism is directed to the proposal that this hypothetical motive would confer sufficient additional reproductive advantage to be maintained by natural selection in our biogram against the relentless drift of mutation.

10. Chagnon (1989b:568) claims that I "seriously misrepresent" the facts when I say (Ferguson 1989d:564), "It is a commonplace in Amazonian ethnography, at least since Levi Strauss' (1944) famous article, that headmen have more wives and more children." He counters (Chagnon 1989b:568), "If that knowledge is widespread, it cannot be based on very much empirical evidence." I will concede half the point: ethnographers have been more interested in leaders' plural wives than the number of their offspring. But on the matter of leaders' polygyny, I repeat, the fact is so routinely reported that it is merely a commonplace. Clastres (1987:32) for instance, comments that the ethnographic literature for lowland South America documents that "nearly all these societies, whatever their type of socio-political unity and demographic size, recognize polygamy; and almost all of them recognize it as the usually exclusive privilege of the chief."

11. Chagnon (1989b:566) states that I assume that "offspring production by men is a simple function of aging, that is, that all men produce the same number of offspring if they live to be the same age." This is another of Chagnon's straw men. What I do say on the point is that "as a young man matures, he is . . . more likely to have more children," an elementary observation that is obvious in Chagnon's own data.

12. Chagnon attributes to me other positions that I do not hold. He claims that I dismiss his theory as wrong simply because it is sociobiological (Chagnon 1990a:49). Of course that is an invalid way to argue, although Chagnon makes just that kind of judgment against anthropologists who have not adopted new biological models (1990b:78, 1992a:93; Chagnon and Hames 1980:347). He asserts (1990b:89) that I assume all human populations are approaching their carrying capacity. My stated position (Ferguson 1990a:32) in that same volume is the opposite: "It is certainly not inevitable that human populations expand until they are stopped by scarcity of some crucial resource." Chagnon (1992a: 91–92) claims that materialist models in general focus on the level of the group and ignore individual-level strategizing. As I discuss elsewhere (Ferguson 1984b: 35–38), that criticism was valid up to the mid-1970s, but no longer. Certainly my theory in this volume is much more concerned with individual strategizing than with the essentially group-selection model Chagnon has most recently presented.

Chagnon claims that during a conference we both attended at the School of American Research in 1986, I made "a statement *to the following effect:* 'I don't understand why "you sociobiologists" keep bringing in reproduction. After all, if you have enough to eat, reproduction is more-or-less automatic' " (Chagnon 1989b:567, my emphasis). An endnote (1989b:569) adds: "His comments were both tape-recorded and heard by the some dozen or so other participants in the symposium." Shortly thereafter we are told: "The assumption 'when people have enough to eat reproduction is more-or-less automatic' is a serious defect in his approach" (1989b:567). As presented, that is a silly statement. I could not remember or imagine saying it, so I obtained the tapes of the discussions of our two papers in order to check. I found no such statement. What I did ask Chagnon to explain was what the calculation of inclusive fitness added to an understanding of behavior, compared to the material variables I stress, and that is the way the seminar chairperson describes this debate (McCauley 1990:2–6).

13. What I am suggesting is that Yanomami do something similar to what

anthropologists do. Anthropologists frequently acknowledge that an intense demand for Western goods characterizes their immediate environs, but they leave that aside in the ethnography to concentrate on "more interesting" cultural norms. Yanomami too direct their attention to more elevated cultural levels.

14. Knauft (1993:1186) takes exception to this position and cites interior New Guinea as an area in which ethnographic documentation reveals warfare "not appreciably influenced by state societies." As Whitehead and I note (Ferguson and Whitehead 1992a:6), "highland New Guinea . . . seems to offer some of the best material for relatively pristine warfare," yet even there, exogenous influences may be ignored at some peril.

For instance, Salisbury (1962), one of the earliest anthropological fieldworkers in the highlands (in 1952) and the author most concerned with the impact of steel tools, notes attacks aimed at plundering goods introduced by explorers in 1933 (1962:114). More significantly, during World War II, when outsiders were largely absent from the Siane area but steel tools were filtering in through exchange networks, "three large wars occurred in the central Pira Valley, in which villages were burned and clans exiled," compared with only four burnings on a smaller scale during the previous 25 years. He adds in a footnote: "The same trend has been remarked for other areas following first contact with Europeans, not merely in Highland New Guinea but throughout the Pacific. Wars on the scale seen in 1938–45 would have rapidly devastated the whole Siane area" (Salisbury 1962:118–19).

The year 1933 is not the earliest date that the external world impinged on life in the New Guinea highlands (see Feil 1987). The point here is not to disregard or minimize local sources of conflict, but to argue that they should be considered in relation to possible stresses associated with state expansion. Clearly, cases will exist in which such stresses are insignificant or even nonexistent, but that conclusion cannot be *assumed* to be true, as it typically has been.