2

CHAGNON AND TIERNEY IN THEIR OWN WORDS

In moving deeper into the controversy, we will start with the key figures' own words to learn what they did (and did not) say before we turn to what others suggest they said. Since the material on Neel is limited—we have covered most of it already and will discuss the rest in chapter 6—this chapter focuses on Chagnon's and Tierney's work. I start with Chagnon.

NAPOLEON CHAGNON

Chagnon's description of his first day of fieldwork has captivated millions of students over the past thirty-five years. Here are selected passages from his chapter "Doing Fieldwork among the Yanomamö":

> My first day in the field illustrated to me what my teachers meant when they spoke of "culture shock." I had traveled in a small, aluminum rowboat propelled by a large outboard motor for two and a half days. This took me from the Territorial capital, a small town on the Orinoco River, deep into Yanomamö country. . . .
>
> We arrived at the village, Bisaasi-teri, about 2:00 PM and docked the boat along the muddy bank. . . . It was hot and muggy, and my clothing was soaked with perspiration. It clung uncomfortably to my body, as it did thereafter for the remainder of the work. The small, biting gnats were out in astronomical numbers, for it was the beginning of the dry season. My face and hands were swollen from the venom of their numerous stings. . . .
>
> The entrance to the village was covered over with bush and dry palm leaves. We pushed them aside to expose the low opening to the village. The excitement of meeting my first Indians was almost unbearable as I duck-waddled through the low passage into the village clearing.
>
> I looked up and gasped when I saw a dozen burly, naked, filthy, hideous men staring at us down the shafts of their drawn arrows! Immense wads of green tobacco were stuck between their lower teeth and lips making them look even more hideous, and strands of dark-green slime dripped or hung from their noses. We arrived at the village while the men were blowing a hallucinogenic drug up their noses. One of the side effects of the drug is a runny nose. The mucus is always saturated with the green powder and the Indians usually let it run freely from their nostrils. My next discovery was that there were a dozen or so vicious dogs snapping

> at my legs, circling me as if I were going to be their next meal. I just stood there holding my notebook, helpless and pathetic. Then the stench of the decaying vegetation and filth struck me and I almost got sick. I was horrified. What sort of a welcome was this for the person who came here to live with you and learn your way of life, to become friends with you? . . .
>
> We arrived just after a serious fight. Seven women had been abducted the day before by a neighboring group, and the local men and their guests had just that morning recovered five of them in a brutal club fight that nearly ended in a shooting war. The abductors, angry because they lost five of the seven captives, vowed to raid the Bisaasi-teri. When we arrived and entered the village unexpectedly, the Indians feared that we were the raiders. On several occasions during the next two hours the men in the village jumped to their feet, armed themselves, and waited nervously for the noise outside the village to be identified. . . .
>
> I pondered the wisdom of having decided to spend a year and a half with this tribe before I had even seen what they were like. I am not ashamed to admit, either, that had there been a diplomatic way out, I would have ended my fieldwork then and there. I did not look forward to the next day when I would be left alone with the Indians; I did not speak a word of their language and they were decidedly different from what I had imagined them to be. The whole situation was depressing, and I wondered why I ever decided to switch from civil engineering to anthropology. (1968:4–6)

As previously noted, Chagnon is very forthcoming about his experiences in the field. It is something I admire in his writing. There is less of the rosy glow common to most ethnographies and more of the real problems anthropologists face in struggling to do research in a difficult situation.

Chagnon's description of how he handled his food supply has become a classic within the discipline: "Food sharing is important to the Yanomamö in the display of friendship. 'I am hungry,' is almost a form of greeting with them. I could not possibly have brought enough food with me to feed the entire village, yet they seemed not to understand this." "I found peanut butter and crackers a very nourishing food, and a simple one to prepare on trips. . . . More importantly, it was one of the few foods the Indians would let me eat in relative peace. It looked too much like animal feces to excite their appetites. I once referred to the peanut butter as the dung of cattle. They found this quite repugnant." Chagnon goes on to describe another occasion: "I was eating a can of frankfurters and growing very weary of the demands of one of my guests for a share in my meal. When he asked me what I was eating, I replied: 'Beef.' He then asked, 'What part of the animal are you eating?' to which I replied, 'Guess!' He stopped asking for a share" (1968:7).

Chagnon also openly discusses how he gathered genealogical information for his and Neel's research, despite Yanomamö's sometimes strenuous opposition to the project:

> There was a very frustrating problem. . . . I could not have deliberately picked a more difficult group to work with in this regard: They have very stringent name

> taboos. They attempt to name people in such a way that when the person dies and they can no longer use his name, the loss of the word in the language is not inconvenient. . . . The taboo is maintained even for the living: One mark of prestige is the courtesy others show you by not using your name. The sanctions behind the taboo seem to be an unusual combination of fear and respect. . . . As I became more proficient in the language and more skilled at detecting lies, my informants became better at lying. One of them in particular was so cunning and persuasive that I was shocked to discover that he had been inventing his information. . . . He would look around to make sure nobody was listening outside my hut, enjoin me to never mention the name again, act very nervous and spooky, and then grab me by the head to whisper the name very softly into my ear." (1968:10–12)

To find out the needed genealogical information against Yanomamö wishes, Chagnon says that "I began taking advantage of local arguments and animosities in selecting my informants. . . . I began traveling to other villages to check the genealogies, picking villages that were on strained terms with the people about whom I wanted information. I would then return to base camp and check with local informants the accuracy of the new information. . . . Despite . . . precautions, I occasionally hit a name that put the informant into a rage, such as that of a dead brother or sister that other informants had not reported. . . . These were always unpleasant experiences, and occasionally dangerous ones, depending on the temperament of the informant" (1968:12).

A positive result of *Yanomamö: The Fierce People* selling so many copies and staying in print for so many years is that Chagnon has had an opportunity to update and revise his book four times; the second edition was published in 1977, the third in 1983, the fourth in 1992, and the fifth in 1997. By studying the changes Chagnon made as he progressed from one edition to another we can gain important insights into Chagnon's motivations as an anthropologist and as an author. Let me highlight some of the themes that come through in examining these changes.

First, Chagnon is concerned with presenting an ever-deeper understanding of the Yanomamö as he learns more about them. He went from nineteen months of fieldwork among the Yanomamö in 1968 (the first edition) to sixty-three months in 1997 (the fifth edition). He is able to infuse the chapters on social organization and political organization with increasingly sophisticated analyses of village dynamics, alliance making, and village movements through time. He writes, in the third, fourth, and fifth editions, that his fieldwork includes an important lesson for anthropologists: "It is in some cases impossible to understand a society's 'social organization' by studying only one . . . community . . . for each community is bound up in and responds to the political ties of neighboring groups" (1997:1). In his first forty-two months of fieldwork, Chagnon was able to visit more villages (sixty) than any other anthropologist who worked among the Yanomamö has been able to do in a comparable period of

time. The implication here is that Chagnon, because of his peripatetic fieldwork style, is able to analyze the Yanomamö in a way few others can.

This focus on visiting so many Yanomamö villages—anthropologists tend to stay put in one village rather than moving around—relates to what we might perceive, in Chagnon's research, as a sense of haste. Implicit in this style of fieldwork is a concern for studying the Yanomamö before they are overwhelmed and transformed by outside forces (see 1977:xi): "The 'first contact' with a primitive society is a phenomenon that is less and less likely to happen, for the world is shrinking and 'unknown' tribes or villages are now very rare. In fact, our generation is probably the last that will have the opportunity to know what it is like to make contact" (1992a:31). In a dramatic fashion that adds excitement to the book, Chagnon describes certain of his "first-contact" experiences with Yanomamö.

Second, Chagnon is intent on addressing criticisms of his work, especially what others view as his overstatement of Yanomamö violence. In the second edition, he emphasizes that most waking hours of Yanomamö are taken up with something besides warfare and that warfare varies from region to region. Still, he asserts, "a meaningful description of Yanomamö . . . warfare necessarily requires the presentation of facts . . . many of us would prefer not to consider. Infanticide, personal ferocity, club fights, and raids . . . have to be described and explained, no matter how unpleasant they might appear to us" (1977:163). The fourth edition discusses reasons other anthropologists working with the Yanomamö report less violence than Chagnon does. He suggests that lowland Yanomamö, particularly in the Shanishani drainage area, are more belligerent as an ecological strategy for safeguarding their large, desirable garden plots. In the highlands, where there is less competition for land, there is less conflict. Chagnon drops the subtitle of the book, "The Fierce People," in the fourth edition in response to criticism of it. As he explains: (a) "Fierce" often comes across in Spanish and Portuguese translations as conveying negative, animal-like overtones. (b) Some colleagues objected to the subtitle and, as a result, refused to assign his book in their classes. And (c) certain colleagues suggested the Brazilian government was using the "fierce" description to justify oppressive policies against the Yanomamö (see 1992a:xii).

A third theme is Chagnon's concern with maintaining the *Yanomamö*'s popularity with student audiences. As he noted in the second edition, "I decided [in writing the book] that I would let my own experiences as a student be my guide as an author, for I wanted to communicate with students of anthropology as with my professional colleagues. I remember . . . how much I enjoyed reading monographs that were sprinkled with real people, that described real events, and that had some sweat and tears, some smells and sentiments mingled with the words" (1977:xi). True to his word, in the third edition he added a case study entitled "The Killing of Ruwähiwä." The fifth edition added another case study, "Alliance with the Mishimishimaböwei-teri." (Chagnon notes that he added the latter case study partly to fit with a new interactive CD-

ROM he helped produce on the topic. By 1997 such interactive materials were becoming increasingly popular in classes and a major selling point for texts.) Clearly critical to the publishing success of Chagnon's *Yanomamö* are a number of vivid ethnographic films relating to his fieldwork. One, *The Feast*, won first prize in every film competition in which it was entered. The films convey a realistic sense of Yanomamö life, building upon and, in turn, enhancing points Chagnon develops in his text. (The second and later editions list these films in case readers wish to view them.)

A fourth theme is Chagnon's increasing concern for the Yanomamö themselves. In the second edition, he includes a chapter entitled "The Beginning of Western Acculturation" that discusses the outside changes that were beginning to engulf the Yanomamö. The chapter gives his account of the 1968 epidemic that devastated the Yanomamö (note that this account differs slightly from Neel's account, which I presented in chapter 1).

> In 1967, while participating with my medical colleagues in a biomedical study of selected Yanomamö villages, we collected blood samples that clearly showed how vulnerable and isolated the Yanomamö were: They had not yet been exposed to measles. Thus, in 1968, when we returned again to extend this study, we brought 3,000 measles vaccines with us to initiate an inoculation program in the areas we visited. Unfortunately, the very week we arrived an epidemic of measles broke out at a number of mission posts and began spreading to the more remote villages as the frightened Yanomamö tried to flee from the dreaded epidemic. We worked frantically for the next month trying to vaccinate a barrier around the epidemic, ultimately succeeding after visiting many villages. . . . Still, a large number of Yanomamö died in the epidemic in some regions—villages that were remote and difficult to reach." (1977:146)

In the fourth edition, Chagnon affirms his stance as a "committed advocate of not only Yanomamö cultural survival and human rights but also native rights and conservation issues all over the globe" (1992a:ix). "It was very difficult for me to write the final chapter," Chagnon notes in the preface. This was partly because of the destructive effects of the Brazilian gold rush and partly "having to do with the negative effects of being too frank about describing some of the politics that interfere with doing anthropological field research, effects that might compromise my effectiveness as an advocate of the Yanomamö, their rights, and their cultural survival at a time when these issues hang in the balance. There is much opposition in both Venezuela and Brazil to anthropologists who want to work among the Yanomamö, and my efforts will be most effective if I am able to return and learn about their new problems and try to develop ways to solve them" (1992a:xii–xiii).

In saying this, Chagnon nonetheless takes a position that upsets many activists working in the Amazon. He criticizes Davi Kopenawa, the most prominent Yanomamö activist in Brazil, calling him a spokesperson "for his [non-Yanomamö] mentors." "Everything I know about Davi Kopenawa is positive and I am convinced he is a sincere and honest man," Chagnon writes in the fourth edition, but

> my concern is that he is being put into a difficult position. . . . For one thing, . . . he cannot possibly speak for Venezuelan Yanomamö. . . . There is also the danger that if Yanomamö "leaders" can be easily created by interested outside parties, every interested group will create and promote their own leader in order to advance their own special interests. In 1990 the Brazilian mining interests paraded their own Yanomamö leader . . . who advocated *their* rights just as strongly as Davi Kopenawä advocates the policies of his mentors. . . . I am astonished at how manipulative the various "outsiders" are in establishing and grooming the candidates whose political positions seem to reflect those of their mentors as much as anything else." (1992a:233–34)

Chagnon concludes the chapter with a position statement:

> My anthropological career has now come full gamut. I started out as just another anthropologist, a scientist, attempting to document and explain a different culture as best I could. By repetitively returning and becoming more and more intimately associated with people like Kaobawä and Rerebawä [two of his informants], I became "involved" in their culture and now want to make sure that they and their children are given a fair shake in the inevitable changes that are occurring. I can do so only by becoming, as they say, involved—by becoming more active and becoming an advocate of their rights and their chances to have a decent future, one that does not condemn them to becoming inferior members of the lowest possible rung of the socioeconomic ladder—bums and beggars in Puerto Ayacucho, alcoholics and prostitutes in the ghettos of Caracas. The rest of my useful career will be dedicated to this. (1992a:244–46)

A final theme that comes through in the five editions is that despite some hard times, Chagnon enjoyed fieldwork among the Yanomamö, and they, in turn, came to appreciate him. In the second edition he writes, "Suffice it to say that the danger [he faced] contrasted with and intensified the pleasure of my happier experiences . . . and the enormous amount of valuable new information I collected" (1977:154). In the fifth edition he observes, "Most of the yet-living Yanomamö men who threatened to or tried to kill me in the past are now friends of mine—and we even joke, albeit gingerly, about those long-ago situations. . . . The Yanomamö have come to know, accept, respect, and consider me as a welcome friend because I have treated them fairly, have not taken sides in their quarrels or wars, provided them with medicines, treated their sick, and regularly brought them the material things I knew they desired and needed" (1997:257).

Even with the changes Chagnon makes as he writes and rewrites *Yanomamö* through time, he continues his basic adaptive theme of Yanomami cultural adjustments not only to their physical environment but to their social and political environments. In the fifth edition, for example, he talks about how new technologies are allowing him to develop a "more sophisticated interpretation of Yanomamö cultural and economic adaptation to their political and geographical environment" (1997:xii)—the same theme espoused in the first edition.

An extension of this adaptive theme can be seen in the famous (and controversial) article "Life Histories, Blood Revenge, and Warfare in a Tribal Population," published in *Science* in February 1988. He writes, "In this article I show how several forms of violence in a tribal society are interrelated and describe my theory of violent conflict among primitive peoples in which homicide, blood revenge, and warfare are manifestations of individual conflicts of interest over material and reproductive resources [i.e., women]" (1988:985).

The article's abstract reads: "A theory of tribal violence is presented showing how homicide, revenge, kinship obligations, and warfare are linked and why reproductive variables must be included in explanations of tribal violence and warfare. Studies of the Yanomamö Indians of Amazonas during the past 23 years show that 44 percent of males estimated to be 25 or older have participated in the killing of someone, that approximately 30 percent of adult male deaths are due to violence, and that nearly 70 percent of all adults over an estimated 40 years of age have lost a close genetic relative due to violence" (1988:985). Chagnon is reiterating what he perceives as the violent nature of Yanomamö society. It was the next point that stirred up a hornets' nest: "Demographic data indicate that men who have killed have more wives and offspring than men who have not killed." Killers are more successful biological reproducers than non-killers. Violence, he is saying, trumps nonviolence in evolutionary terms. As readers will see, others challenge Chagnon's claims. But there is no doubting the provocativeness of his article. It stirred up much debate.

In concluding this section, I would note a contrast in Chagnon's treatment of two topics that are repeatedly referred to in the controversy. As we have seen, Chagnon openly discusses the Yanomami taboo against naming deceased relatives as well as the problems he encountered and how he sought to circumvent them. But he does not discuss the Yanomamö concern with the blood of deceased relatives. He notes that Yanomamö feel the deceased's body should be cremated at death. (Relatives may eat some of the deceased's ashes.) But he does not discuss how the Yanomamö feel about having body fluids, such as blood, preserved after an individual's death, especially in a faraway country.

PATRICK TIERNEY

Tierney makes a number of accusations in *Darkness in El Dorado* against a number of people, but the ones that have most been taken note of—perhaps because they have received the most publicity—are the ones against Neel and Chagnon. Because Tierney organizes his book chronologically, the accusations, especially against Chagnon, are woven into a number of chapters and do not unfold systematically. Still, as one chapter builds on another, Tierney's case against both individuals become clear. If we cut and paste a little to bring related points in different chapters together, we might highlight two accusations against Neel and the seven accusations against Chagnon.

Tierney's accusations against Neel are far more serious and—in my reading of the media reports—sparked the most attention. They are also the most controversial.

First, Tierney accuses Neel of making the deadly 1968 measles epidemic worse, rather than better, through his actions. Essentially, Tierney accuses Neel of aiding and abetting the deaths of Yanomami as part of a larger, vaguely defined project to explore Yanomami susceptibility to measles. "It is difficult to imagine a group at higher risk to a live measles virus [vaccine] than the Yanomami," Tierney states in discussing the vaccine Neel used to inoculate Yanomami against measles in 1968 (2000:60).

> Yanomami at the Ocamo mission received the Edmonston B [vaccine] without the recommended gamma globulin coverage [meant to lessen adverse reaction to the shot], which doubled the risk of reaction [to the vaccine]. . . .
>
> There was no doubt . . . that a full measles rash and fevers first appeared among the Ocamo Yanomami within a week of the Indians' vaccination. Prior to the Yanomami's severe vaccine reactions . . . no one had seen the disease's telltale lesions. (2000:60, 67)
>
> Chagnon and Neel described an effort to "get ahead" of the measles epidemic by vaccinating a ring around it. As I have reconstructed it, the 1968 outbreak had a single trunk, starting at the Ocamo mission and moving up the Orinoco with the vaccinators. (69)
>
> Clearly he [Neel] and his doctors distributed medicine and cared for some of the sick they encountered. But his choice of vaccine [the Edmonston B] suggested he wanted new data [on genetic questions of selective adaptation] and his impatience with Venezuelan authorities meant that he had no backup from government doctors when crisis occurred.
>
> Moreover, Neel barely slowed his pace of blood-collecting or filming, both of which required massive payments of trade goods, a reckless policy during an epidemic [because the infected people would, in trading goods to other villages, spread the disease]. . . . The scientists kept moving on and the epidemic moved on with them. (82)

In the prepublication galleys, Tierney suggested that Neel's use of the Edmonston B vaccine itself might have caused some cases of measles. In the version that was published, Tierney backs away from explicitly asserting that: "It is unclear whether the Edmonston B became transmissible or not. That was the question that perplexed the expedition." (Apparently the possibility that the vaccine might cause measles was raised in one of the expedition's radio transmissions that Tierney examined.) He adds: "The chaos and deaths that followed vaccination . . . can be explained in terms of the extraordinary high vaccine reactions, coupled with simultaneous exposure to malaria and bronchopneumonia" (2000:81).

Tierney then goes on to suggest that Neel's excitement at the measles epidemic "was understandable. Witnessing measles as it infected an aboriginal group was a once-in-a-lifetime event. It seems to have been the only time in recent history when scientists were present at such an outbreak. And Neel was on hand with a documentary filmmaker to capture the scenes. . . . It was as if the sound and video had been suddenly added to the sixteenth-century Spanish chronicles" (2000:72). Of all of the accusations presented here, this first one—regarding Neel's role in the epidemic—is the one most often rejected by those familiar with the controversy.

Tierney's second accusation is that Neel could have done more than he did to help the Yanomami during the epidemic. When push came to shove—in terms of making choices between treating Yanomami and pursuing personal research goals—Neel, while trying to act humanely, emphasized his research.

> Neel's expedition, with its two doctors and a nurse and 250 doses of vaccine, passed through Platanal, invited all of the Mahekoto-teri to a filming even, but failed to vaccinate them, as it promised Venezuelan authorities. It is difficult to understand that decision today, knowing that 25 percent of the Mahekoto-teri, about thirty individuals, died of measles. . . . [Neel's] expedition had been in the field for almost a month and . . . the scientists were exhausted, sick, and increasingly disgruntled. Most of them were in the jungle for the first time, and each had a demanding research agenda. Their scientific hopes were all pinned on reaching the remote village of Patanowä-teri. It was hard to turn back to care for sick Indians, especially when the scientists, like the missionaries, were still not sure what was going on (Tierney 2000:78).

Some scholars who have examined the evidence, especially those critical of Chagnon, tend to accept this second accusation against Neel.

Tierney offers an intriguing perspective on the need for Yanomami genealogies and the collection of blood samples by Neel. "Students of [Chagnon's] *The Fierce People*, have gotten only the vaguest inkling about why the agency that manufactured atomic bombs spent large sums studying the Yanomami" (2000:37). The reason was "the AEC [the Atomic Energy Commission] wanted thousands of Yanomami blood samples, together with their corresponding genealogies, to determine mutation rates in a completely 'uncontaminated' population" (2000:43). This meant that Chagnon, in collecting data for Neel, had to travel far more than the anthropological norm, moving from village to village both to collect the necessary genealogical data and to prepare villagers for the collection of their blood by Neel's team.

This brings us to Tierney's accusations against Chagnon. First, Tierney accuses Chagnon of misrepresenting key dynamics of Yanomami society, especially its level of violence. He points out that "the Yanomami have a low level of homicide by world standards of tribal culture and a very low level by Amazonian standards. Compared to other tribes, they are fearful of outsiders" (2000:13). He quotes a former student of Chagnon, Ken Good: " 'In my opinion, the Fierce People is the biggest misnomer in the history of anthropology' " (2000:131). Tierney adds, "Chagnon's other students would also report much lower levels of violence than their mentor found" (2000:131). Tierney goes on to suggest that Chagnon's focus on violence played into the hands of Brazilian gold miners intent on disrupting plans for a large Yanomami land reserve in Brazil. (Mining would be illegal within such a reserve.) According to Tierney, the Brazilian military chief of staff, General Bayna Denis, justified drastically reducing the size of this reserve "by explaining that the Yanomami were too violent and had to be separated [into several small reserves] in order to be civilized" (2000:160). "María Manuela Carneiro da Cunha [a past president of the Brazilian Anthropological Association] accused Chagnon of doing violence to the Yanomami's chances of survival through his theories of violence" (2000:160).

Second, Tierney accuses Chagnon of stimulating, through his gift giving, the very warfare Chagnon suggests was prominent among the Yanomami. Tierney writes: "Within three months of Chagnon's sole arrival on the scene three different wars had broken out, all between groups who had been at peace for some time and all of whom wanted a claim on Chagnon's steel goods." He quotes Brian Ferguson, who has written a book on Yanomami warfare: " 'Chagnon becomes an active political agent in the Yanomami area. . . . He's very much involved in the fighting and the wars. Chagnon becomes a central figure in determining battles over trade goods and machetes' " (2000:30). "Whatever else can be said about Yanomami warfare," Tierney continues, "it is not 'chronic.' . . . All the violence among Chagnon's subjects can be spelled out in two stark spikes, both corresponding to outside intrusion" (2000:34). Tierney adds that the "deadliest war ever recorded among the Yanomami" occurred between villages allied with SUYAO (United Yanomami Communities of the Upper Orinoco, a Yanomami trade cooperative) and villages allied with Chagnon and Brewer-Carías as part of their FUNDAFACI (Foundation to Aid Peasant and Indigenous Families) project (the incident is described later in this section). "The outbreak of the wars occurred at around the same time as Chagnon's entry into Yanomami territory, in the early summer of 1990" (2000:227–28).

Third, Tierney accuses Chagnon and others of staging films on the Yanomami and portraying them as real events. Regarding the award-winning film *The Feast*, he asserts that Chagnon and the filmmaker Timothy Asch drew two Yanomami groups together—when they were not necessarily inclined to meet—and plied them with trade goods so as to act out the film's scenes. "Chagnon saw himself as recording 'specific events,' " Tierney writes. "The Yanomami recall his staging them" (2000:84). "The Yanomami understood that Chagnon wanted scenes of violence" (102). The Yanomami were afraid of cameras, Tierney notes: "The Yanomami believe cameras kill. . . . Cameras are like sci-fi ray guns, whose energy envelops and steals its target's spiritual essence" (83–84). The problem wasn't the staging but the fact that the staging was never revealed in the film. The whole context suggested the films were live footage of real events when, in fact, this was not the case. Left undisclosed is how the Yanomami felt about images of themselves being caught on film.*

Fourth, Tierney accuses Chagnon of falsifying data in Chagnon's famous *Science* article: "In the *American Ethnologist*, Jacques Lizot accused Chagnon of having created villages whose demographics were unlike any known communities and whose exact location was 'impossible to determine.'" He observes that while Chagnon's "charts on fertile killers looked good on paper, there was no way to confirm or refute them. Not only were the 'killers' anonymous, so were the twelve villages they came from" (2000:164). Through independent research, Tierney claims to have rechecked Chagnon's analysis and finds the data far more ambiguous than Chagnon acknowledges: "Minute manipulations in each age category could easily skewer all the results. . . . The spectacular superiority of killers for the entire study depended on a big bachelor herd under age 25 whose members were both peaceful and infertile" (176). He also notes that Chagnon's thesis differs from the recollections of Helena Valero, who lived among the Yanomami for fifty years: "This divergence began with motives and dates, but, most crucially, it included the actual number of victims and their specified killers" (247).

Fifth, Tierney asserts that Chagnon acted unethically in collecting Yanomami genealogies. Not only did Chagnon go against the Yanomami name taboo in collecting people's names but he used techniques that antagonized Yanomami informants. He gathered data by relying on children and marginal individuals as well as by playing individuals and villages off against one another. "His divide-and-conquer information gathering exacerbated individual animosities, sparking mutual accusations of betrayal." Tierney makes reference to "the ugly scenes" Chagnon "witnessed and created" (2000:33). Tierney writes: "Although it might appear that these were simply the antics of an ego out of control, there was a logic to Chagnon's anthropological methods. He had . . . to get the Yanomami to divulge their tribal secrets" (48).

*Tierney also discusses the staging of the well-known NOVA/BBC special on the Yanomami, *Warriors of the Amazon*. Andy Jillings, the director of the documentary, noted in a telephone interview with Tierney: "I was looking for a group that was fairly unacculturated and that was at war and suing for peace. So Jacques [Lizot] and I went out to another group that was at war, but they were not home much of the time. I wanted an unacculturated group because you can't make a film about the Yanomami if they're wearing Black Sabbath T-Shirts. We spoke to the more remote group but, basically, we were hijacked because the Karohi people said, 'Why don't you have the feast here?' They saw all our trade goods and they didn't want them going to the other group. The feast of reconciliation [between two warring groups] was a set-up. We might have facilitated it. But they wanted it" (2000:220). The film's highlight was the cremation ritual of a recently deceased woman. Rather than nursing the woman back to health, the film crew recorded her dying. Mike Dawson, who had lived among the Yanomami for more than twenty-five years, told Tierney, "With a little bit of help, they [the mother and her newborn infant] could have pulled through. The film crew interfered in every aspect of their [Yanomami] lives. Let's be real. They're giving them machetes, cooking pots, but they can't give a dying woman aspirin to bring her fever down?" (2000:217)

Sixth, Tierney indicates that Chagnon misrepresented his first-contact experiences: "It is a remarkable fact and a remarkable theft. Every single place . . . and every single village . . . that Chagnon has touted as his discovery, was intimately known and visited by Helena Valero [before him]" (2000:246).

Finally, Tierney asserts that Chagnon violated Venezuelan law in what came to be known as "the FUNDAFACI affair." Chagnon allied himself with Charles Brewer-Carías, an entrepreneur with a reputation for mining remote regions of Venezuela, as well as with Cecilia Matos, the mistress of the then president Pérez. The three hatched a plan to set up, under their control, a vast Yanomami "nature reserve" roughly the size of Maine (2000:9; on page 188 Tierney states the area involved was the size of Connecticut). Tierney notes that the reserve "would have given him [Chagnon] unprecedented power, but it required overthrowing the legal structure already established in Yanomami territory" (10). Tierney suggests that by 1990, opposition to Chagnon's research among both Venezuelan academics and the Yanomami had increased, and, as a result, Chagnon was finding it ever harder to continue his periodic trips to the field. "With Matos and Brewer . . . for allies, Chagnon devised a . . . bold . . . plan to permanently circumvent all the institutions that controlled the Yanomami Reserve. The three . . . would simply create their own, private reserve, a Yanomami park. At the same time, . . . they began a fierce press campaign against the Salesian" missionaries who opposed the plan (186). "According to . . . Venezuela's assistant attorney general for indigenous affairs, the various trips by Brewer and Chagnon, which cost millions of dollars in government transportation costs, 'were illegal because there is no evidence they even submitted their plans to the DAI [Indian Agency] for approval'" (191).

Concluding this section, I would add two points regarding *Darkness in El Dorado*. The first concerns Tierney's view of Neel's role in the measles epidemic. I talked to Tierney two times about it when he visited Hawaii. (He was invited by a group of Hawaiian activists.) Both conversations progressed in much the same way. I would indicate that I viewed the measles accusation regarding Neel as without empirical support. He would respond by expressing regret about including this accusation because it was the part of the book reviewers had most vociferously attacked. He perceived, correctly I believe, that it distracted from other more extensively discussed issues—especially his accusations against Chagnon. When I suggested that he delete the accusation regarding Neel from later editions of his book or admit that he might have been mistaken in his analysis, he always backtracked. Perhaps there might be a grain of truth, he would suggest, in what he had written. I take this to mean that Tierney, in his heart of hearts, wants to believe the controversial accusation against Neel is true, although few others do. He realizes the assertion has created serious problems and has cast a shadow of doubt over his whole work. This he regrets. Still, there is something inside him, I believe, that resists his letting go of the accusation.

Second, Tierney never accuses Neel or Chagnon of committing genocide. As we have seen, Tierney makes a number of serious accusations against each of them, but he never refers to genocide. My research suggests that it was Chagnon who first brought up the accusation of genocide. In 1989 Chagnon responded to a published letter from the Brazilian anthopologist Carneiro da Cunha (which, while critical of Chagnon's behavior, never refers to genocide): "The suggestion . . . that I am encouraging or promoting genocide is gratuitous and insulting. It is also libelous" (1989b:24). I perceive in Chagnon's response a way of discrediting his attackers by overstating their case. ("See what they accuse me of? What type of people would make such a patently false statement?")

We need not get drawn into the theatrics involved on either side of the argument. It is far better to stick with Chagnon's and Tierney's positions as they themselves represent them. These are provocative enough.

3

HOW THE CONTROVERSY HAS PLAYED OUT WITHIN AMERICAN ANTHROPOLOGY

EARLY RUMBLINGS

The Yanomami controversy had been brewing for years before the publication of Tierney's *Darkness in El Dorado* in 2000. Most anthropologists did not take much notice. Still, elements of the controversy were there if one cared to look.

Elements of the controversy could be seen in 1988 when Maria Manuela Carneiro da Cunha, the past president of the Brazilian Anthropological Association (ABA), wrote to the American Anthropological Association's (AAA's) Committee on Ethics regarding Napoleon Chagnon. The committee never addressed her concerns, but her letter was eventually published in the *Anthropology Newsletter*. Carneiro da Cunha wrote: "The recent appearance in the Brazilian press of two articles on the Yanomami Indians based on Napoleon Chagnon's latest paper on Yanomami 'violence' [the article in *Science*] . . . has prompted us to call your attention to the extremely serious consequences that such publicity can have for the land rights and survival of the Yanomami in Brazil." (She is referring to the ways in which Chagnon's work had gotten entangled in the politics surrounding the establishment of a Yanomami reserve.) After challenging Chagnon's claims regarding the high rate of Yanomami violence in detail, she concludes: "The Brazilian Anthropological Association (ABA) feels that it is fundamental *to insist* on the need to bring to the awareness of North American anthropologists the political consequences of the academic images they build about the peoples they study. The case of the Yanomami in Brazil, who have been suffering a brutal process of land expropriation which is justified in discriminatory images based on dubious scientific conclusions, are in this respect a particularly grave and revealing case. . . . We urge the AAA to take the necessary steps to call to the attention of the North American anthropological community the ethical and moral repercussions of their writings for critical situations such as this" (Carneiro da Cunha 1989:3).

Chagnon was invited by the editor to reply to Carneiro da Cunha's letter. Chagnon responded by concurring with Carneiro da Cunha regarding the "senseless, inaccurate and irresponsible portrayal of the Yanomamö" by members of the press. But he went on to offer a detailed rebuttal of her accusations against him, concluding that "despite the disclaimer by the AAA that it does not 'endorse' the position of either the ABA or me, this exchange has some serious implications for ethnographic reporting by U.S. researchers working in other countries. The AAA's policy of 'reciprocity' (guaranteed publication) to sister AA organizations might be opening the door to an avalanche of complaints that, like this one, are rather more political, not to mention libelous, than they are professional, scientific or ethical. I am astonished that the AAA has accepted for publication in the *AN* an accusation against one of its members, without considering its possible accuracy, that he is (1) falsifying and manipulating data, (2) doing so with a 'fidelity' that fosters genocidal practices and (3) implies he is describing the people among whom he has worked in racist terms" (1989b:24).

There was more to the exchange, though this only came out later. The *Anthropology Newsletter* subsequently published a letter by a Chagnon supporter (Machalek) but refused to publish a letter by a supporter of Carneiro da Cunha (Albert). The reason was never made clear.

Elements of the controversy could also be seen in 1994 in the aftermath of the massacre at Haximu of sixteen Brazilian Yanomami by gold miners. (Initial accounts in the *New York Times* placed the count at twenty, then seventy-three, before it was revised down to the now accepted figure of sixteen.) On the Venezuelan side of the border, a controversy erupted regarding who was authorized to investigate the actions of gold miners against the Yanomami. Two investigative teams were formed. The initial investigative team included Chagnon and Charles Brewer-Carías. When various Venezuelans protested this team's membership, a second investigative team was formed. By chance, the two teams met near the massacre site. According to Tierney, Judge Aguilera (the head of the second team) ordered Chagnon (from the first team) "to cease and desist [in his investigation] or face arrest. . . . Chagnon was escorted to Caracas by Colonel Márquez, who took his notes and urged him to leave the country immediately, which, in fact, Chagnon did" (Tierney 2000:200). Behind this conflict lay a broader one. According to Salamone: "Principal among [the] . . . concerns [involved] is control of research in the Orinoco region of Venezuela. The issue, in many people's views, is whether Chagnon or the Salesian [missionaries] should control research in the sector" (1996:4; cf. Chagnon 1977:150).

Chagnon made his criticisms of the Salesians public following his expulsion. In a *New York Times* op-ed piece, he wrote, "The Salesian policies include attracting remote Indian groups to their missions, where they die of disease at four times the rate found in remote villages. While the Salesians claim they no longer attract converts by offering shotguns, that was their policy until 1991. Over the past five years there has been a rash of shotgun killings. Yanomami from the missions raid distant, defenseless villages, often traveling in power boats borrowed from the Salesians. They kill the men with guns, abduct the women and gang-rape them. . . . The Salesians have done little to stop this practice. It is likely that many more Yanomamö die from mission policies than at the hands of *garimpeiros* [gold miners]" (1993a:12).

Chagnon elaborated on these accusations in the *Times Literary Supplement*: "So far the Brazilians have sponsored and conducted a far more effective, professional investigation than the Venezuelans. And for this embarrassment the Venezuelan government must thank the Salesian missionaries, as well as their own reluctance to defend their nation's secular legal right to pursue justice in the face of the opposition and intimidation of the Catholic Church. Clearly, the Salesians are attempting to preserve their virtual monopoly of political authority in Venezuela's Amazonas" (1993b:11).

The Salesians responded with an attack of their own. The *Chronicle of Higher Education* observed:

> This year and last [1993–94], documents attacking Mr. Chagnon's scholarship have been sent, some anonymously, to many anthropology departments in the United States, as well as to the National Science Foundation. The documents included newspaper articles critical of him and Mr. Brewer Carías. Some of the anonymous mailings were postmarked in New Rochelle, N.Y., where the Salesians have their U.S. headquarters. Mr. Chagnon says the Salesians are orchestrating a smear campaign against him. Father Cappelletti acknowledges sending some of the materials, but not anonymously. One item Father Cappelletti did send was an English translation of a posting to a computer bulletin board in which Mr. Lizot [the French anthropologist referred to in chapter 1] derides Mr. Chagnon personally and professionally. 'Everyone is sick and tired of the maniac,' Mr. Lizot wrote. (Monaghan 1994)

Seeking resolution of the conflict, Salamone organized a session at the American Anthropological Association Annual Meeting, which he describes: "On December 2, 1994 an extraordinary event took place. . . . Napoleon Chagnon . . . met with Father Jose Bortoli, a Salesian missionary to the Yanomami on the Orinoco River for 20 years" (1997:1). The transcript of the session (published in Salamone 1996) makes evident that the two parties were trying their best to set aside their differences. It all seemed to be working—that is until Terry Turner, a critic of Chagnon, made the following statement during the question period:

> "Professor Chagnon has recently said in print in the American Anthropological Association newsletter that I [Terry Turner] have forfeited all credibility as an anthropologist because I have referred to Davi Kopinawa [*sic*] as a genuine Yanomami leader, where he is only a mouthpiece for NGO's. It's not only a matter of this being false, it's a matter of this undermining the most effective spokesman for Yanomami interests. . . . To undermine him in such an untruthful way, without knowing him and obviously without taking the trouble to analyze the text of his speeches . . . directly damages the interest of the Yanomami. And I submit that this is in apparent contradiction to the ethical dictates of this association" (Salamone 1996:49–50).

When asked if he wanted to reply, Chagnon responded: "You're goddamn right I'd like to. I came here in a spirit of conciliation with an interest in advocating the rights for the Yanomami and I'm going to ignore all of Professor Turner's comments, which I think are out of place in the spirit of what we're attempting to accomplish in this meeting today" (Salamone 1996:50). The transcript stops at this point, but people who were at the session indicated that the confrontation between the two pretty much ended at this point as well. Other people then asked other questions and the ensuing discussion moved off in another direction. No one took up Turner's point regarding whether Chagnon had possibly violated the American Anthropological Association's code of ethics.

The following year, Brian Ferguson published a book entitled *Yanomami Warfare: A Political History*. In the book Ferguson develops a general theory of warfare focusing on the Yanomami as a case study. He asserts that "the existence and variation of actual Yanomami warfare in historical context is explainable largely by reference to changing circumstances of Western contact, which, contrary to established opinion, has been important to the Yanomami for centuries" (1995:xii). He continues: the events of conflict discussed in his book "display a pattern . . . [of] actors . . . [employing] force instrumentally [i.e., using violence] in order to enhance their access to and control over Western goods" (306). Ferguson concludes that "the wars and other conflicts of the middle 1960s—those made famous in *Yanomamö: The Fierce People*—are directly connected to changes in Western presence . . . including the arrival of Chagnon himself" (278).

Reviewing Ferguson's book for the *American Anthropologist*, Chagnon writes: "Ferguson comes uncomfortably close to claiming that my presence among the Yanomamö, especially between 1964 and 1970, 'caused' the wars I described, a politically correct and increasingly popular theme in some of the anonymous hate mail denouncing me that has been put into circulation since 1993 and is occasionally claimed in print by some writers" (1996:670). "It is difficult to avoid the conclusion," Chagnon continues, "that much of contemporary cultural anthropology, even the kind of 'scientific' anthropology that Ferguson claims he is doing, is an enterprise that promotes politically correct fairy tales intended to repudiate and denigrate colleagues while solemnly claiming that it is good academic behavior. These activities are now preventing anthropologists from doing fieldwork in many places, including the Yanomamö region" (672).

A PAINFUL CONTRADICTION

Many anthropologists might have missed the 1989 exchange between Carneiro da Cunha and Chagnon. After all, there were thirty-two pages in that issue of the *Anthropology Newsletter*. And many might have missed the session organized at the 1994 AAA Annual Meeting by Salamone. There were over five hundred sessions, workshops, and meetings that year at the gathering. Likewise, there were hundreds of anthropology books published in 1995 along with Ferguson's, and Chagnon's review was one of over fifty in the issue in which it appeared.

But one would find it hard to explain how most anthropologists missed the

critical contradiction regarding Chagnon's work that faced the discipline for more than three decades. Without doubt, Chagnon's ethnography has been fantastically successful in terms of sales. No one knows exactly how many copies have been sold. George Spindler, coeditor of the Case Studies in Cultural Anthropology series that published *Yanomamö*, indicated that original sales (sales directly from the publisher) probably numbered around one million. But the book has been sold and resold on the used book market as well. That total is impossible to ascertain, but Spindler suspected that one might well add another one to two million in sales. Sales of the book thus total perhaps three million. (Tierney, citing a quote attributed to Chagnon that appeared in a Brazilian magazine, puts the number between three and four million (2000:8, 331n4). These are phenomenal figures, unmatched by any other anthropological account in the past forty years. "Best-selling" ethnographies sell around forty thousand copies, and most ethnographies usually sell between one and three thousand copies.

Part of the book's success clearly can be attributed to the films, produced in collaboration with Timothy Asch, that complement the book. In their introduction to *Yanomamö*'s third edition, George and Louise Spindler point to the films: "In our extended experience as instructors of introductory anthropology . . . the combination of a challenging, exciting case study and well-executed ethnographic films is unbeatable" (1983:vii). Chagnon's writing style has been important as well. Leslie Sponsel observes: "It is very well written, sprinkled with personal anecdotes and candid reflections, dangerous and heroic adventures, cultural surprise and shock, tragedy and humor, and sex and violence" (1998:101). "We recommend *Yanomamö: the Fierce People*," the Spindlers state, "as one of the most instructive and compelling writings available in anthropology" (1983:viii).

There is only one problem. Chagnon writes against the grain of accepted ethical practice in the discipline. What he describes in detail to millions of readers are just the sorts of practices anthropologists claim they do *not* practice. Let me quote from two introductory textbooks as a way of conveying how anthropologists generally describe their discipline to students. Here is Haviland's popular *Cultural Anthropology* describing an anthropologist's obligations to the people he or she studies: "Because fieldwork requires a relationship of trust between fieldworker and informants, the anthropologist's first responsibility clearly is to his or her informants and their people. Everything possible must be done to protect their physical, social, and psychological welfare and to honor their dignity and privacy. In other words, *do no harm*" (2002:26). In Nanda and Warms's *Cultural Anthropology*, it is described this way: "Anthropologists are always required to reflect on the possible effects of their research on those they study. Three main ethical principles that must guide the field-worker are obtaining the informed consent of the people to be studied, protecting them from risk, and respecting their privacy and dignity" (2002:63).

The American Anthropological Association's "Statement of Ethics" (adopted in 1971 and amended in 1986) reads, under "Relations with those studied": "In research, anthropologists' paramount responsibility is to those they study. When there is a conflict of interest, these individuals must come first. Anthropologists must do everything in their power to protect the physical, social, and psychological welfare and to honor the dignity and privacy of those studied" (AAA 1971/1986). The 1998 "Code of Ethics of the American Anthropological Association" reaffirms this position: "Anthropological researchers must do everything in their power to ensure that their research does not harm the safety, dignity, or privacy of the people with whom they work, conduct research, or perform other professional activities" (AAA 1998:III, A.2).

Note the contrast between these statements and the way Chagnon described his efforts to circumvent the Yanomami name taboo in his genealogical research: "If the informants became angry when I mentioned the new names I acquired from the unfriendly group, I was almost certain that the information was accurate. . . . When I finally spoke the name of the dead woman, [the informant] flew out of his chair, raised his arm to strike me, and shouted: 'You son-of-a-bitch! If you ever say that name again, I'll kill you'" (1968:12–13). In *Studying the Yanomamö*, Chagnon elaborated further: "[Because] I could not expect to easily get the true names of the residents from the residents themselves . . . I had to resort to . . . tactics such as 'bribing' children when their elders were not around, or capitalizing on animosities between individuals, or photographing the people and taking the photos to other villages for identification. . . . There is . . . no better way to get an accurate, reliable start on a genealogy than to collect it from the [person's] enemies" (1974:91, 95).

Chagnon also discussed Yanomamö's reactions to his presence in various villages: "There was great danger, for as my personal relationship with Möawä developed, it grew more tense, and in the end he almost killed me with his ax. . . . I recall vividly the long trek through the gloomy forest to contact Börösöwä's village, and how Börösöwä and his brothers tried to do me in while I slept. . . . And beyond this village lay Tananowä's. . . . I turned back from that trip when Rerebawä told that Tananowä, whom I had never met, vowed to kill me if I ever came to his village, for he concluded that I was practicing harmful magic against him. He, along with some of my Patanowä-teri friends, had made an effigy of me . . . and ceremoniously shot it full of arrows" (1977:153–54). In *Studying the Yanomamö*, Chagnon writes: "My study of the Shamatari groups began with threats to my life and ended that way" (1974:166).

Chagnon's relationships with several informants, in other words, tended at times toward the confrontational—especially in his early years of research. He dedicated himself to collecting data many Yanomami did not want him to have.

James Clifford, in discussing the fieldwork of French anthropologist Marcel Griaule, points out that there are alternative fieldwork styles to the standard Anglo-American model of sympathetic rapport characterized by close relationships and respect. Marcel Griaule emphasized "a recurring conflict of interests [in fieldwork], an agonistic drama, resulting in mutual respect, complicity in a productive balance of power" (Clifford 1983:140). This was Chagnon's style.

Readers need to realize that invading people's privacy and violating their taboos also falls within the bounds of earlier American fieldwork practices. Here is how Eliza McFeely describes the fieldwork of Matilda Stevenson and Frank Cushing among the Zuni of the American Southwest in the 1880s: "In any number of . . . instances, Stevenson bullied her way into ceremonial chambers where she was not welcome; by her own account, she rode roughshod over Zuni guides to make them take her to shrines they wished to keep secret from her. . . . [Cushing characterized his uninvited move into the house of the Pueblo's civil leader] as impetuous and aggressive, casting himself as a hero who was willing to defy common courtesy and potentially hostile hosts in the pursuit of science" (2001:57, 89). But in terms of current American and British standards—as expressed in introductory texts *and* the American Anthropological Association's code of ethics—Chagnon's style of research is anomalous.

It is useful in this context to contrast Chagnon's behavior with that of E. E. Evans-Pritchard under very trying circumstances. During his initial fieldwork among the Nuer of Sudan, Evans-Pritchard found that "the local Nuer would not lend a hand to assist me in anything and they only visited me to ask for tobacco, expressing displeasure when it was denied them. When I shot game to feed myself . . . they took the animals and ate them in the bush, answering my remonstrances with the rejoinder that since the beasts had been killed on their land they had a right to them. . . . When I entered a cattle camp it was not only as a stranger but as an enemy, and they [the Nuer] seldom tried to conceal their disgust at my presence, refusing to answer my greetings and even turning away when I addressed them" (1940:10–11). As for data collection, "After a while the people were prepared to visit me in my tent, to smoke my tobacco, and even to joke and make small talk, but they were unwilling either to receive me in their windscreens [homes] or to discuss serious matters. Questions about customs were blocked." After offering an example of how informants circumvented his questions, he continues, "I defy the most patient ethnologist to make headway against this kind of opposition. One is just driven crazy by it" (1940:12–13).

Yet Evans-Pritchard did not turn to Chagnon's confrontational style. Instead he focused on a few select locales where he could directly observe the Nuer. "As I could not use the easier and shorter method of working through regular informants I had to fall back on direct observation of, and participation in, the everyday life of the people. From the door of my tent I could see what was happening in the camp or village and every moment was spent in Nuer company" (1940:15). Chagnon writes in the preface to the third edition of *Yanomamö* that he visited some sixty villages during his first forty-two months in the field (1983:ix). Given the difficulties he faced in traveling to and dealing with informants in a host of diverse locales, it is—in my opinion—an impressive effort. But why do it? Especially when he notes that "it takes months to establish rapport with individuals in a new group and to discover who the good informants are" (1974:94).

In reading through Chagnon's field exploits, one is led to repeatedly ask, why rush from place to place, generating antagonism here, having people threaten you there, and often being uncertain who is exactly telling you accurate information? Evans-Pritchard was able to get around the problem of recalcitrant informants by staying put in one place for a period and observing everyday life. Chagnon tended to keep moving.

Chagnon explains his mobility in the following terms: "It became increasingly clear that each Yanomamö village was a 'recent' colony or splinter group of some larger village, and a fascinating set of patterns—and problems—began to emerge. . . . The simple discovery of the pattern had a marked influence on my fieldwork: it meant that I would have to travel to many villages in order to document the genealogical aspects of the pattern" (1983:30).

But that is not the only explanation. Through mentions here and there one can piece together another story: Chagnon had to collect the genealogical data needed by Neel to make sense of Neel's massive blood sampling. Chagnon was forced by the terms of his funding through Neel to keep on the go—handing out goods (e.g., 1974:183, 186), collecting genealogies, and then, rather than making a particular village his home, moving on to another village. Rarely does Chagnon provide details of Neel's project. The main reference occurs in a footnote that appears in the second and later editions. The primary description of Chagnon's relation to Neel's blood sampling project by Chagnon comes from *Studying the Yanomamö*. "One of my tasks is to provide my colleagues with minimal genealogies for use in family studies of inherited genes. Since the genealogies are necessary, I am often in the position of having to select my informants from among total strangers and accept what they say" (1974:92). Occasionally in reading Chagnon one detects a frustration with his having to follow a schedule not his own: "I had advised my medical colleagues that to complete *my* [Chagnon's italics] study, I had to have four months of additional research among the Shamatari unencumbered by rigorous airplane schedules and the urgency to get perishable blood samples to point X at time Y" (1974:180).

I have spent some space trying to provide a sense of Chagnon's fieldwork as it comes through from his various accounts. A question that faces us as a discipline is why so few anthropology teachers of introductory classes objected to a fieldwork style that runs counter to what most of them espouse in principle.

In addressing this question, I would note that a sympathetic reading of Chagnon's texts suggest that he himself realized something was amiss in this style of fieldwork. He is at pains in several places to downplay his conflicts with the Yanomamö. In the second edition, for example, he notes: "The reciprocal and generally good-natured mischief with which the Yanomamö and I treated each other during my first 15-month stay among them gradually evolved into a much warmer and more intimate relationship as I returned to live among them nearly every year since I wrote the first edition of *Yanomamö: the Fierce People*" (1977:xii). And resonating with the more general style of American anthropology today, he writes: "The great privilege I have had in my life was to have met people like Kaobawä, Rerebawä, and Dedeheiwä and to learn from them something about the quality of their way of life" (1977:196).

The book has proved so popular in part because of the way Chagnon portrayed himself. He was Indiana Jones before Indiana Jones. Susan Sontag writes of "The Anthropologist as Hero," in which she refers to the way anthropologists use difference to challenge, to cast doubt on our accepted assumptions and habits (1966). But Chagnon represented a different anthropologist as hero. He was the adventurer who overcame a host of physical and social obstacles to return home with "the goods." He domesticated the exotic, the dangerous, in the name of Western science. Observe how he describes his work: "I have nearly been killed by the Yanomamö several times. . . . I knew, in those cases, that it was risky to go to some of the places where this was a possibility, but I was willing to take those known risks" (1992a:238). After mentioning various people who sought to kill him during his fieldwork, he continues: "Suffice it to say that the danger contrasted with and intensified the pleasure of my happier experiences . . . and the enormous amount of valuable new information I collected, . . . information that will contribute to a greater understanding of population dynamics and political processes . . . [and] the role of warfare in the history of our species" (1977:153–54).

Chagnon was able to beat the Yanomamö at their own game: "I soon learned that I had to become very much like the Yanomamö to get along with them on their terms: sly, aggressive, and intimidating" (1968:9). "I developed a very effective means for recovering almost all [of my] . . . stolen items. I would simply ask a child who took the item and then take that person's hammock when he was not around, giving a spirited lecture to the others as I marched away in a faked rage with the thief's hammock" (1968:10).

For American audiences attuned to violence on television and in newspapers, there was more than enough to excite the most jaded of readers. Here was pure adventure. George and Louise Spindler note in their editorial remarks to the first edition that the Yanomamö have "a high capacity for rage, a quick flash point, and a willingness to use violence to obtain one's ends. . . . To the ethnographer it is frightening, frustrating, disgusting, exciting, and rewarding" (1968:vii-viii). "The thing that impressed me most," Chagnon states in the first edition and repeats in later editions, "was the importance of aggression in their culture. I had the opportunity to witness a good many incidents that expressed individual vindictiveness on the one hand and collective bellicosity on the other" (1968:2–3).

And if violence were not enough, there were also provocative statements regarding male-female relations like the following: "Most fighting within the village stems from sexual affairs or failure to deliver a promised woman—or out-and-out seizure of a married woman by some other man" (1983:7). And: "Once raiding has begun between two villages . . . the raiders all hope to acquire women if the circumstances are such that they can flee without being discovered" (1968:123). Of his 1988 *Science* article regarding the relation of violence to reproductive success, Chagnon writes in the fourth edition, "*Unokais* (men who have killed) are more successful at obtaining wives and, as a consequence, have more offspring than men their own age who are not *unokais*" (1992a:205).

It was all there—adventure, violence, and sex à la American—recorded in the name of science. Chagnon's work resonated with large audiences of students in ways that most ethnographies never come close to managing.

Chagnon might well perceive his accounts as simply "telling it like it is." But without additional information that adds greater humanity to the Yanomamö, readers are left with a sense of what is termed *orientalism*—a playing up of Yanomamö differences in ways that enhance our own power and status at their expense. This is an attitude almost all anthropologists criticize. Remember his first meeting with Yanomami (quoted in chapter 2): "I looked up and gasped when I saw a dozen burly, naked, filthy, hideous men staring at us down the shafts of their drawn arrows!" (1968:5). The description appears in all five editions of his book and is widely anthologized. It reinforces Western images of Amazonian Indians as "primitive" and "savage" compared to us.

To summarize, there is a puzzling contradiction between the espoused aims of anthropology and the overwhelming success of Chagnon's book. I can only conclude that many anthropology teachers and students, caught up in the excitement of Chagnon's work, forgot anthropology's abstract pronouncements regarding appropriate styles of fieldwork and writing. They went for adventure, violence, sex, and, of course, the films.

What Tierney's *Darkness in El Dorado* did was to expose this contradiction to the whole world. No wonder Tierney's book made a lot of anthropologists mad. Whatever Tierney's mistakes—and there clearly are mistakes—he pointed out a contradiction anthropologists had grown comfortable with. There was something almost inevitable about Tierney's exposé. The contradiction was too obvious not to be commented upon eventually. But it took an outsider—a journalist—aided and abetted by the media to make anthropologists take note. Many anthropologists seemed willing to ignore the whole problem.

THE AMERICAN ANTHROPOLOGICAL ASSOCIATION'S AMBIVALENT RESPONSE

As discussed in the previous section, the discipline—viewing it as a collective group for the moment—knew about the problems surrounding Chagnon's fieldwork years before the publication of Tierney's book. But the American Anthropological Association resisted investigating them. It responded mostly with a cascade of nice-sounding abstractions followed by little concrete action. The leaders of the association took steps in the wake of the media storm generated by *Darkness in El Dorado* that at first continued this pattern.

While Tierney's book was still in prepublication galleys, Terry Turner and Les Sponsel wrote a confidential e-mail memo to the president (Louise Lamphere) and president-elect (Don Brenneis) of the AAA as well as to the chair of the Committee for Human Rights (Barbara Johnston). At the behest of Johnston, Turner writes, "we agreed to send a second version to the Chair of the Ethics Committee and the Presidents of the . . . Societies of Latin American Anthropology and Latino and Latina Anthropology" (Turner 2000b:2). Somehow, one of these e-mails was passed on to someone else who, in turn, forwarded it on to others. The process snowballed and within perhaps forty-eight hours the memo had circled the world. Within another forty-eight hours, most of the discipline knew about it.

Turner states in a September 28, 2000, letter to Dr. Samuel Katz that "the sole purpose of the memo was to describe . . . [Tierney's] allegations, in order to warn the leaders of the association of the nature of the allegations that were about to be published" (Turner 2000b). The Turner-Sponsel memo begins: "We write to inform you [i.e., the leaders of the AAA] of an impending scandal that will affect the American Anthropological profession as a whole in the eyes of the public and arouse intense indignation and calls for action among members of the Association." In elaborating on these accusations, the gap between Tierney's assertions and what Turner and Sponsel accepted of them got lost. Turner and Sponsel referred to Tierney's "convincing evidence" and to his "well-documented account." They also sought to catch the AAA's attention with a few provocative turns of phrase. For example, they refer to Tierney's account as a "nightmarish story—a real anthropological heart of darkness beyond the imagining of even Josef Conrad." (One might suspect that they felt frustrated, given the years the issue had been ignored, and wanted to ensure that the AAA leadership understood the importance of Tierney's accusations.) Turner and Sponsel were certainly right about one thing: as they suggested, Tierney's accusations became seen "by the public, as well as most anthropologists, as putting the whole discipline on trial" (Turner and Sponsel 2000).

Turner and Sponsel were both well versed in the controversy surrounding Chagnon's fieldwork. Both had talked to Tierney about it. It is understandable, then, that Turner would write that "Tierney's accounts of . . . [Chagnon's] activities checked out with what we knew, although Tierney provided much new data." According to Turner, Tierney kept the accusations about Neel "under authorial wraps for as long as possible" (Turner 2000b). Turner and Sponsel found out about them only when they read the final galleys of Tierney's book in August 2000, just before the book's publication. Turner and Sponsel assumed that if the accusations they knew about were correct, then the new ones about Neel—which they were not familiar with—probably should be taken seriously.

It turns out they were too hasty in making that assumption. As Turner explains, once the "confidential" memo had been sent to the AAA leadership, he and Sponsel turned to investigating Tierney's specific accusations against Neel.

> After sending the memo, we set out to check for ourselves on the most sensational (and to us, the most unfamiliar) of Tierney's allegations (that the vaccination campaign, through the vaccine it used, had actually started the measles epidemic). Experts we consulted confirmed that the consensus of medical opinion was that a vaccine could not cause contagious cases of the disease against which it immunizes. This appeared to contradict the possibility that Dr. Neel could have caused

> the epidemic through the vaccinations, either deliberately or accidentally. . . . Both Sponsel and I have made a point, in our contacts with journalists and the media, of repudiating irresponsible media reports of "genocide," or any intention to cause death as part of an experimental plan, by Dr. Neel or anyone else connected with the expedition. (Turner 2000b)

But it was too late. Given the discipline's past resistance to addressing the controversy surrounding Chagnon, one might have predicted what transpired next. Rather than engaging with the substance of Turner and Sponsel's message—that negative publicity was about to hit the discipline—some sought to shoot the messengers. For them, Turner and Sponsel's memo became the scandal.

Instead of confronting the breadth of issues raised by Tierney and the media, many anthropologists focused on Tierney's accusations regarding Neel and on the Turner-Sponsel memo. As previously noted, focusing on Neel had a particular advantage for those who wanted to continue sidestepping the role of anthropologists in all this. Neel was a geneticist, and soon after the book's publication most experts realized that the accusation that Neel helped facilitate the spread measles was false. Focusing on Neel allowed anthropologists to downplay the role of the discipline in the whole affair.

Still, the American Anthropological Association clearly heard Turner and Sponsel's message regarding the approaching whirlwind of negative publicity. The first recorded AAA response, entitled "Statement on Allegations Made in the Book *Darkness in El Dorado*" reads in part: "The American Anthropological Association is aware of the publication of the book *Darkness in El Dorado* by Patrick Tierney. The book makes serious allegations. . . . If proven true they would constitute a serious violation of Yanomami human rights and our Code of Ethics. . . . The Association is anticipating conducting an open forum during our Annual Meeting to provide an opportunity for our members to review and discuss the issues and allegations raised in the book" (AAA n.d.).

The issue of having an open forum is discussed further in another statement from the American Anthropological Association dated October 19, 2000, and entitled "Questions and Answers."

> Q: Why is the AAA holding an open forum regarding the allegations?
> A: . . . As a scientific and professional organization we are committed to a fair and impartial discussion of the issues raised by the book. . . .
> Q: How does the AAA respond to the accusations that the forum is one-sided?
> A: These charges are absolutely false. We are holding an open forum at our Annual Meeting in November designed to include both sides of this controversy, as well as impartial experts in the field, so that the allegations and issues which they raise can be fairly debated and discussed among our members. (AAA 2000a)

Before the open forum, the Executive Board decided to "establish a Special Ad Hoc Task Force of seven members, six of which will be appointed by the AAA President from among the members of the Committee on Ethics and the Committee for Human Rights, chaired by AAA Past President James Peacock, and charged . . . to examine assertions and allegations contained in Darkness in El Dorado as well as others related to the controversy" (AAA 2000c). The basic conclusion of the Ad Hoc Task Force, as reported by the Executive Board, was that "it finds many of the allegations made in the Tierney book to have such serious implications for anthropologists and for the Yanomami that they are deserving of further attention from the AAA" (AAA 2000c). The Ad Hoc Task Force, in other words, reiterated the basic point of the Turner-Sponsel memo. But there was a critical difference. The AAA labeled this report confidential. And when the AAA said confidential, it meant confidential. No copy of the report has ever been made public. Nor, for that matter, has the full membership of the Ad Hoc Task Force been made public.

An open forum was held on November 16 at the annual meeting. Was the open forum balanced? Did it, as claimed, "include both sides of this controversy, as well as impartial experts in the field?" If this occurred, then the majority of the members present missed it. This is how the forum was perceived by one person there:

> I thought at first that so many panelists meant that Tierney and Chagnon's sides were each to be heard. Not. Tierney was isolated and visibly distanced at one end of the elongated panel table. . . . [Napoleon Chagnon] was represented by Dr. Irons, seated to the left. That led me to expect that the three women sitting to the right of the podium must be taking Tierney's perspective. Wrong. One after another, each panelist rose to excoriate Tierney over mistakes they claimed he had made, over his determination to "prevent" scientific medical research to aid remote indigenous people, and all kinds of other positions I had never heard or read that he had taken. . . . They . . . seemed to merge rumor and published text together into an intertextual morass which amounted more to diatribe than to critique (Curran and Takata 2000).

The writer wasn't alone in feeling that the session was slanted against Tierney. Reporters at the open forum had a similar impression. Geri Smith wrote in *Business Week*: "Tierney underwent a four-hour grilling at the November AAA . . . special symposium called to discuss his book" (2000:24). John Noble Wilford of the *New York Times* reported "Mr. Tierney bore the brunt of attack when appearing on a panel on Thursday and at a news conference afterward" (2000:24).

What happened? Not only were the panelists stacked against Tierney but they mostly focused on the accusations surrounding Neel—accusations that no one involved in the controversy besides Tierney still clung to. Only Irons—Chagnon's chosen defender at the session—spoke at any length regarding Chagnon. If there were significant critiques of Chagnon or Neel at the session by speakers other than Tierney, then the press, and many at the meeting, including myself, missed them.

One might well have assumed from the Thursday night open forum that Tierney's key arguments had been thoroughly refuted. In fact, of course, only the argument regarding Neel helping to facilitate the spread of measles had really been criticized, and that had been refuted weeks before. To those versed in the controversy, it looked like beating a dead horse. From the open forum, one would have thought that Chagnon had played only a minor role in the book, that almost *all* of Tierney's accusations centered on Neel.

The next night, the AAA allowed an open mike session on the controversy. Instead of a stage-managed panel with presentations slanted in a particular direction, individuals were free to line up and offer three-minute statements. Miller, in the *Chronicle of Higher Education*, summarizes what happened: "Although no one offered a four-square endorsement of Mr. Tierney's facts or conclusions, many of the 20 or so speakers took the microphone to fault Mr. Chagnon in particular and anthropologists in general for questionable conduct in the field" (2000a).

The AAA Executive Board, at its meeting on February 3 and 4, 2001, established an El Dorado Task Force based on the recommendations of the private Ad Hoc Task Force report. Louise Lamphere, the AAA president, described the purpose of the task force in the *Anthropology Newsletter*: "The Board designated the work of the task force as an inquiry, not an investigation. We are not the American Bar Association; we do not license our members, nor do we have a process in place by which we can impose sanctions. Our concern is with the book Patrick Tierney has written and the allegations he makes. The Task Force will gather evidence from a broad variety of sources: AAA members, the book's author and key anthropologists mentioned in the book. . . . The Task Force . . . will gather information in a fair and open manner and will carefully consider evidence that either substantiates Tierney's allegations or casts doubt on them" (2001:59).

The Executive Board's report for February 3 and 4, 2001, states:

> Members of the Task Force were appointed by the AAA President. The Chair, Jane H. Hill (Arizona) is a linguistic anthropologist specializing in American Indian languages, and former President of AAA. Fernando Coronil (Michigan) is a cultural anthropologist specializing in the Venezuelan state. Janet Chernela (Florida International University) is a cultural anthropologist specializing in Amazonian indigenous societies. Trudy Turner (Wisconsin-Milwaukee) is a biological anthropologist specializing in genetics of non-human primates and in ethics. Joe Watkins (Bureau of Indian Affairs) is an archaeologist specializing in relations between Indians and archaeologists and in the involvement of Indian people in archaeology and anthropology. Watkins is Chair of the AAA Ethics Committee. (2001c)

I want to deal with the question of why President Lamphere chose these five people, since a major critique of the Task Force is that it did not interview at length many of the key anthropologists mentioned in the book (or even Tierney). There was no open discussion regarding the selection. And only Janet Chernela had, in any real sense, experience with the Amazon region; she had some interaction with a Brazilian NGO working with the Yanomami and had studied an unrelated Tukanoan group some distance from the Yanomami. Fernando Coronil, a citizen of Venezuela, had extensive expertise on Venezuelan politics but little on the Yanomami. Joe Watkins, a Choctaw Indian, works on the archaeology of the southern Great Plains and relations between Native Americans and archaeologists. Trudy Turner specializes in the life history of vervet Monkeys in Africa as well as genetic diversity and ethics. Jane Hill works on Native American languages of the Uto-Aztecan family (spoken in Mexico and the United States).

In other words, no one on the original Task Force had extensive field experience with the Yanomami. In the summer of 2000, under what she refers to as pressure from the Chagnon camp for a more balanced Task Force, Lamphere added a sixth member, Ray Hames. A student of Chagnon, Hames has conducted extensive fieldwork among the Ye'kwana and Yanomami Indians of Venezuela.

One might recognize that the membership of the Task Force represents all four of anthropology's subfields. Affirming the value of subfield integration has been a continuing theme of the AAA in recent years as specialization has pushed different subfields in different directions and threatened the unity of the AAA (see Borofsky 2002). Viewed in structural-functionalist terms, in this time of stress the AAA leadership sought to reaffirm disciplinary solidarity. However, it is not readily apparent that either archaeological or linguistic issues were central to the controversy.

There is another, more political, way to look at the Task Force's composition. One needs to be careful, though: students do not necessarily follow the opinions of their teachers in lockstep. But readers should be aware of the relationships that exist. Coronil was a student of Terry Turner, who has been a critic of Chagnon. Trudy Turner held a postdoctoral fellowship in 1981–82 in the Department of Human Genetics, University of Michigan. Though she claims never to have had close contact with Neel, who headed the department the year Turner began her fellowship, she has proved to be a strong defender of Neel. Hames, as previously noted, was a student of Chagnon. Chernela was chair-elect of the AAA's Committee for Human Rights at the time, and Joe Watkins was chair of the AAA's Committee on Ethics. (Only in the final report do we learn that both Watkins and Chernela were members of the Ad Hoc Task Force Committee.) Hill, an honored past president who was not seen as affiliated with any particular camp, wrote the first piece on the controversy published in the *Anthropology News*: "Is it possible to turn this public-relations disaster not only into a 'teachable moment' inside the profession but into an unforeseen opportunity to get out the good word about anthropology and anthropologists?" (2000:5).

Aside from trying to respond to the concerns of Chagnon's supporters with the selection of Hames (to balance the selection of Coronil, perceived by supporters of Chagnon to be in the opposite camp), Lamphere downplays the politics of her selections. She conveys in personal conversations a sense of wanting to get on with the task with a reasonable set of people who would represent a fair sampling of the constituencies involved. Still, many involved in the controversy found the selections problematic. Why were more experts on the Yanomami not brought in, for example? Hames's selection upset many. In fairness to Hames, it should be noted that he did not want to be on the Task Force. Lamphere had asked two other behavioral ecologists (with little experience with the Yanomami), and both had turned her down. Hames had recommended John Peters (a participant in this book's part 2 discussion) because of his in-depth experience with the Yanomami. But Lamphere rejected Peters. Given this context, Hames felt, despite his reservations, that he should help, since the Task Force obviously needed someone with knowledge of the Yanomami.

By mid-2001, the Task Force had begun seriously going about the business of collecting information and framing a preliminary report. An understanding of how it proceeded in this process is critical. Following established academic style, different Task Force members took on different assignments. They specialized in areas of particular interest. Trudy Turner, for example, was assigned the accusations surrounding Neel; Fernando Coronil, the accusations surrounding Chagnon's work with FUNDAFACI (the Foundation to Aid Peasant and Indigenous Families, which sought to set up a private Yanomami reserve in Venezuela). Ray Hames examined Chagnon's involvement in Yanomami warfare.

We need to note four problems with the process. First, the report indicates that each of these people took positions that might have been expected of them given their backgrounds. The side taking was not blatant. Much detailed data and many citations were mustered to support the varying perspectives. But there were few surprises. No one collected piles of information and then took a totally new position based on that material. At best, there was a slight softening of positions, an offering of subtleties and complexities to go with the perspectives that outsiders to the Task Force assumed specific individuals would take.

Second, there was little systematic investigation of topics from divergent perspectives. Coronil and Hames, for example, did not both study FUNDAFACI but turned their attention to different topics. As a result, members had to rely mostly on the information a particular person collected if they wished to challenge that person's conclusions. They had no independent, confirming source to assess another member's analysis.

To make matters worse, there were no public hearings where scholars more familiar with the data than those on the Task Force could challenge the position statements being drawn up. It was all done hush-hush, mostly in private with only the occasional leak.

Third, we come back to the Task Force's composition. In my opinion, having Ray Hames on the Task Force was a sound idea. He was thoroughly familiar with the controversy. But why not have other experts similarly versed in these matters on the Task Force as well? Why, for example, was John Peters rejected? The critical weakness of the Task Force, I would suggest, is that there was no engagement between experts deeply versed in the subject—as occurs in part 2 of this book. It was mostly well-intentioned people holding to positions that, some would suggest, were formulated well before the members ever met as a Task Force.

Fourth, the Task Force's preliminary report obscured who wrote what. It was presented as a consensus of the collective Task Force, though it was later discovered that two Task Force members had not even read it. The *Chronicle of Higher Education* provides the best account of what unfolded when the preliminary report was publicly presented at the AAA Annual Meeting in November 2001: "Two of the six members of the panel that is studying the controversy said they have not endorsed the report, and one asked that it be withdrawn. . . . [Mr. Coronil] urged his colleagues to refashion the report as a series of working papers credited to the individuals who had done research on each issue. 'As far as I'm concerned, the report was not discussed,' he concluded, to . . . [a] round of sustained applause" (Miller 2001). As for the preliminary report itself, it "essentially exonerated the late James V. Neel . . . of Mr. Tierney's charges that he had exacerbated a deadly measles epidemic in 1968 and withheld treatment from sick Yanomami in order to further a research experiment. . . . But Mr. Tierney had spent several chapters describing the alleged transgressions of Mr. Chagnon. In its investigation of these charges, the committee has so far cleared Mr. Chagnon of a few of the most serious charges, criticized him for a few relatively minor lapses in judgment, and left other allegations unaddressed" (Miller 2001). Critics of the Task Force cried whitewash.

The uproar that followed the preliminary report brought about two positive outcomes: First, at its next meeting, in February 2002, the Task Force decided to openly acknowledge who wrote which sections of the report. An author's positioning was no longer obscured by the Task Force supposedly speaking with a collective voice. (At this point, they clearly did not.) Second, and, more critically, the Task Force decided to open up the preliminary report for public comment by way of the Web. People were encouraged to voice their opinions—in a place where all could see them—regarding the strengths and weaknesses of the preliminary report.

This decision transformed the debate. The chief antagonists on both sides had, in many ways, stopped listening—that is, honestly listening—to one another. In their rebuttals they would acknowledge some detail in the other's position and then reframe the issues in terms advantageous to themselves. Most of the time they talked past one another when they talked to each other at all.

To the surprise of many, over 170 comments were put up on the Web site between March 1 and April 19. One hundred nineteen students weighed in with one or more assessments of the report (compared with 36 professors). These students' statements helped transform the debate. The responses made clear that a lot of people were discussing the Task Force's report in very public ways. Because the student comments could not be precisely pigeonholed into this or that camp, they drew Task Force members into focusing on the common public good rather than on placating this or that constituency.

The involvement of a large number of students clearly shook things up. To my knowledge, nothing like this had ever occurred in the history of the discipline. A long dormant and often de-emphasized part of the association was making its opinions felt. It was "student power" in action. No one on the Task Force that I talked to felt that such an outpouring from students could be dismissed—in sharp contrast to members' reactions to positions taken by key figures on one or the other side of the debate. More was involved here than just principle. Anthropologists and journalists from around the world were also reading these comments, which were a matter of public record. Who wanted to be caught ignoring such a massive public outpouring?

While the students' positions varied widely, they tended to be more critical of Chagnon than the Task Force was. Several astutely critiqued the Task Force itself. (One suggested there should be a new task force to write a report on the errors of the current one.)

As a result of the Web postings, Ray Hames, who had always been ambivalent about being on the Task Force, resigned. In his resignation letter he says, "My association with Chagnon presents the appearance of bias. Consequently, I feel it is in the best interest of the American Anthropological Association that I resign from the Task Force. . . . The goal of the Task Force is to produce an accurate and unbiased appraisal of ethical research practices by anthropologists among the Yanomamö. Any false perception that this goal was not met can only harm our association and vitiate the findings of the Task Force" (2002). It was an honest assessment—especially given the lack of effort to balance his perspectives with those of other Yanomami experts holding different views.

Another result of the student outpouring was that members of the Task Force at their next meeting (in April 2002) started to reach across their differences and explore the possibility of developing a real consensus on certain issues—particularly relating to Chagnon, who all along, with a strong set of supporters, was the most problematic figure to investigate. People began to carefully listen to one another and seek out shared points of agreement. Ideally they would have brought Yanomami experts as well as a host of Yanomami into their discussion (or at least used a speakerphone to collectively ask the Yanomami questions in Roraima, Brazil, for example). Still, as a result of the student outpouring, Task Force members turned toward more seriously addressing the problems Tierney had raised regarding Chagnon than many critics thought possible.

Chagnon deserves better than death by a thousand small cuts. He should not have to contend with unsubstantiated innuendo. He deserves a fair chance to address the accusations against him in open court where others, too, can see what he is being accused of and why. Because Chagnon has refused to participate in such discussions, part 2 of this book constitutes the most open, balanced discussion we are likely to have on this matter in the foreseeable future. It is not perfect. But, more so than in the Task Force's final report (see chapter 11), it gives readers the information to draw their own conclusions regarding the controversy's central issues. It is *not* done for them by a special task force meeting in private.

4

BROADER ISSUES AT STAKE IN THE CONTROVERSY

POWER DIFFERENTIALS IN THE ANTHROPOLOGICAL ENDEAVOR

Different anthropologists define cultural anthropology in slightly different ways. Kroeber, in his classic 1948 introductory text, *Anthropology*, observes that cultural anthropology "sometimes . . . seems preoccupied with ancient and savage and exotic and extinct peoples. The cause is a desire to understand better all civilizations" (1948:4). Felix Keesing, in 1958, writes that "the cultural anthropologist looks at human behavior comparatively" (1958:v). His son Roger, almost twenty years later, suggests that cultural anthropology is "concerned with the study of human customs: that is, the comparative study of cultures and societies . . . especially what used to be called 'primitive' peoples" (1976:3). Kottak says that "cultural anthropologists study society and culture, describing, analyzing, and explaining social and cultural similarities and differences" (1997:5–6). If one does a little bit of editing here and there, adjusting this phrasing, adapting that, the definitions clearly overlap.

But more interesting than the fact that the definitions overlap is what they all leave out. Since its disciplinary beginnings, cultural anthropology has tended to be the study of less powerful groups by scholars from more powerful groups. Whether you phrase it as the First World studying the Third, "us" studying "them," or the richer studying the poorer, there is almost always a power differential involved. Those with more power are usually studying those with less.

Anthropologists do not return empty-handed from their research. They return with knowledge that they then systematically circulate to others in the form of publications and lectures. In most cases, this knowledge circulation enhances their careers. Few anthropologists make thousands of dollars from their publications and lectures. (Chagnon is a rare exception in this regard.) But most anthropologists make hundreds of thousands of dollars over their careers, and those careers are enhanced by their publications. The publications constitute critical stepping-stones for professional advancement.

The less powerful give something of value to the more powerful who are studying them. Anthropologists—out of respect, kindness, guilt, or a combination of all three—tend to provide a host of compensating gifts. But rarely, if ever, do these gifts add up to the monetary value anthropologists earn as they advance through their academic careers based on visiting and writing about the less powerful.

This is not to say the power differential goes unnoticed. It is widely perceived by all the parties involved. This dynamic gets expressed in the writings of indigenous activists. One such activist, Hereniko, asks: "'Do outsiders have the right to speak for and about Pacific Islanders? . . . Westerners seem to think they have the right to express opinions (sometimes labeled truths) about cultures that are not their own in such a way that they appear to know it from the inside out. . . . The least that outsiders can do . . . is to invite indigenous Pacific Islanders, whenever possible, to share the space with them, either as copresenters or as discussants or respondents. Not to do so is to perpetuate unequal power relations between colonizer and colonized'" (quoted in Borofsky 2000:86). Prins notes that "'the image made in Accra to commemorate the achievement of political independence by Ghana shows the fleeing agents of colonialism. Along with the [administrative] District Officer is the anthropologist, clutching under his arm a copy of Fortes and Evans-Pritchard's *African Political Systems*'" (quoted in Kuper and Kuper 1985:870).

Some anthropologists acknowledge the problem in their writings. Lévi-Strauss observes, "It is an historical fact that anthropology was born and developed in the shadow of colonialism" (1994:425). Asad says, "It is not a matter of dispute that social anthropology emerged as a distinctive discipline at the beginning of the colonial era, that it became a flourishing academic profession towards its close, or that throughout this period its efforts were devoted to a description and analysis—carried out by Europeans, for a European audience—of non-European societies dominated by European power" (1973:14–15). Anthropology is, he continues "rooted in an unequal power encounter . . . that gives the West access to cultural and historical information about the societies it has progressively dominated" (16–17).

We should be cautious here. The broad outline is clear, but there are shades of gray that also need to be taken into account. Clifford notes that while colonial domination framed most anthropological accounts of times past, anthropologists "adopted a range of liberal positions within it. Seldom 'colonists' in any direct instrumental sense, ethnographers accepted certain constraints while, in varying degrees, questioning them" (1983:142).

What concerns me here is how anthropologists, once they acknowledge this power differential, tend to respond to it. Many offer various forms of appreciation to informants: gifts, money, and/or help. A decent percentage of anthropologists, moreover, continue contact with informants long after they, the anthropologists, have left the field. Interestingly, pre–World War II *American Anthropologists* published obituaries of key informants. This suggests that many informants held honorable, publicly acknowledged places within the discipline during this period.

But at a broader level, the abstract formulations anthropologists offer for addressing this power differential, while frequently sounding nice, tend to perpetuate the power structures. Let me illustrate my point with the anthropological injunction to "do no harm." The injunction draws power from the Hippocratic dictum "As to disease make a habit of two things—to help, or at least, to do no harm" (*Epidemics* I. II). The 1998 Anthropological Association statement on ethics asserts that "anthropological researchers must do everything in their power to ensure that their research does not harm the safety, dignity, or privacy of the people with whom they work, conduct research, or perform other professional activities (AAA 1998).

But when things are falling apart politically and economically in a society, is doing no harm a reasonable standard to follow? There is self-absorption in the "do no harm" framing: the injunction implies that we—the outsiders, the westerners, the powerful—are the major source of other people's troubles. If we leave others alone, everything should be fine. In the case discussed below, the troubles of the Ik people in Uganda did not stem from actions by the West but from specific actions by the Ugandan government.

What does "do no harm" mean when informants have been suffering—perhaps for decades—before you arrive? Do you help lessen the pain, the problems? Or do you simply sidestep the pains, believing that since you did not cause them, they are not your problem?

The Ik offer a good illustration of the issues involved. Bordering on starvation, the Ik were falling apart as a society when Colin Turnbull studied them. The back cover of the 1987 paperback edition of Turnbull's book explains: "In *The Mountain People*, Colin M. Turnbull . . . describes the dehumanization of the Ik, African tribesmen who in less than three generations have deteriorated from being once-prosperous hunters to scattered bands of hostile, starving people whose only goal is individual survival. . . . Drought and starvation have made them a strange, heartless people, . . . their days occupied with constant competition and the search for food."

How does one respond to a situation such as this? Appiah ponders the question why "the former general secretary of Racial Unity [i.e., Turnbull] had done so little to intervene? Why had he not handed over more of his own rations? Taken more children to the clinic in his Land Rover? Gone to the government authorities and told them that they needed to allow the Ik back into their hunting grounds or give them more food?" (2000:58).

Turnbull took a group-dictated letter to government authorities at Moroto regarding the Ik's plight. "I delivered the letter and a report of my own, without much conviction that either would carry any weight" (1987:109). And when they apparently did not, he went off to the capital, Kampala, to stock up with fresh supplies for himself. That was it: no insistence, no pleading, no seeking to bring pressure on local authorities from those higher up, no public exposé with the hope of helping the Ik (see also Grinker 2000:166). What Turnbull did in his book, instead, is offer a general reflection on the state of humanity: "Most of us are unlikely to admit readily that we can sink as low as the Ik, but many of us do, and with far less cause. . . . Although the experience was far from pleasant, and involved both physical and mental suffering, I am grateful for it. In spite of it all, . . . the Ik teach us that our much vaunted human values are not inherent in humanity at all, but are associated only with a particular form of survival called society, and that all, even society itself, are luxuries that can be dispensed with" (1987:12, 294; see also Grinker 2000:156, 163).

Keeping the issue at an abstract level—doing no harm, reflecting on what the Ik teach us about ourselves—means the power differential is never addressed. The anthropologist remains an observer of other people's suffering and, in Turnbull's case, deaths. This standard allows anthropologists to claim the high road of morality—they have not caused ill by their presence—while letting the sufferings of the status quo prevail.

I want to emphasize that there is no simple answer to resolving the power differentials embedded in the ethnographic endeavor. It is not from want of caring that the problem remains the uninvited guest in most anthropological publications and most anthropological meetings. Most anthropologists care about helping those who so caringly helped them.

But what constitutes help? One might share one's income with one's informants. But would they do the same if the positions were reversed? And is money the answer—a framing of the field relationship in terms of capitalistic exchange? Or is some kind of continued caring more sensible: a partaking of each other's proffered gifts through time?

In his *Theory of Justice* and *Justice as Fairness*, the late political philosopher John Rawls offers a framework for finding our way through the complexities. Rawls asserts: "The fair terms of social cooperation are to be given by an agreement entered into by those engaged with it." Given people may not "agree on any moral authority, say a sacred text or a religious institution or tradition. . . . What better alternative is there than an agreement between . . . [people] themselves reached under conditions that are [perceived as] fair to all?" (2001:15). Rawls is saying that concerns over compensation need to be resolved by the parties themselves. Given that the two parties often come from different backgrounds and likely possess differing values, they need to find points of common reference if they are to build a mutually satisfying relationship.

Rawls emphasizes that these discussions need to be more than negotiated exchanges. They need to involve a concern for a shared sense of justice. He assumes that the parties—with their different perspectives—are positioned behind "a veil of ignorance" where "they do not know how the various alternatives [they are discussing] will affect their own particular case and they are obliged to evaluate [the] principles of who will get what solely on the basis of general considerations" (1971:136–37). In other words, both sides must establish the terms of their relationship with each other not knowing which side they ultimately will be on—the one they bargained for, or the other. "No one knows his place in society, his class position or social status . . . and the like" (137).

Rawls's point, adapted to the ethnographic endeavor, means establishing a just sense of engagement based on shared discussions. True, the anthropologist, having a clearer sense of the value gained from his fieldwork in relation to the rewards returned to informants, is likely to be at an advantage in such discussions. But following Rawls we might ask what would be a fair agreement for the anthropologist if he found himself on the other side of the relationship, if he were the informant? Start with the possibility that the tables could be reversed, Rawls is suggesting, and seek a just solution based on that.

There is the question of continued ties. Is it reasonable to simply grab what one can, strew gifts here and there, and then vanish? Or is the ethnographic endeavor—perhaps started in the field-worker's youth—something that endures through the years, even when the anthropologist does not necessarily visit informants or they him? Might one view the issue also as a matter of knowledge exchange? Informants provide anthropologists with the data (or tools) they need to write thoughtful publications. Might anthropologists, in turn, provide informants with the tools to effectively engage with the injustices, inequities, and diseases they face on an ongoing basis?

What is critical here is that the terms of the negotiation be public: that they be included in the publications themselves. It is important that others who live in the society, who read the publication, or who later visit the locale studied have an opportunity to understand on what terms the anthropologist gathered the information being presented in his or her publications. The power differentials do not disappear here. They are embedded in structures both parties to the ethnographic relationship will likely not change—short of a revolution that neither will likely lead. But the differentials are acknowledged, softened through a negotiated, fair exchange, and made public so others can understand and assess the exchange.

THE PROBLEMATIC WAYS IN WHICH ANTHROPOLOGISTS SEEK TO RESOLVE CONTROVERSIES

When accusations fly back and forth—as they do in this controversy—how do anthropologists make their way through the torrent of words, the thicket of argumentation? Anthropologists generally rely on certain signs of credibility. They assess credibility in the work of others in a number of ways.

First and foremost, anthropologists pay attention to whether the researcher "was there." A researcher is more credible if he or she has lived in a particular locale and interacted with people there. Chagnon uses this technique to make his work more credible. In the first edition of *Yanomamö: The Fierce People*, he writes "I spent a total of twenty-three months in South America of which nineteen were spent among the Yanomamö" (1968:1). And in the fifth edition, he writes: "To date I have spent 63 months among the Yanomamö" (1997:viii). To make sure readers understand that he was seriously at work during this time—because he could conceivably have spent much of his time lounging around taking in the sights—he reinforces his expertise with personal anecdotes, statistics, and photos. In *Studying the*

Yanomamö, Chagnon presents interviews (1974:80–82), detailed genealogies (100, 134), computer printouts (109), photographs (114), and tables (131, 136). All these data convey an important message: Chagnon knows what he is talking about.

Tierney uses the same technique. He includes a host of personal, first-hand experiences with Yanomami to reinforce his critique of Chagnon. He writes, for example, "The real shock came when I visited a village on the Mucajaí River in Brazil, where Chagnon claimed to have discovered a Yanomami group that embodied the tribe's ultimate form of 'treachery.' In reality, these Indians had lived in relative harmony for more than a century. I was amazed to find that Chagnon had even created his own topography—moving a mountain where one did not exist and landing cargo planes where they had never touched down—while quoting people he could never have spoken to in this part of the jungle" (2000:8). Tierney also offers tables full of data (2000:165, 321). Tierney's point is that he too has first-hand knowledge of the Yanomami.

Second, anthropologists give credence to work that presents new research material. For example, Chagnon writes that his field research involved traveling "further and further into uncontacted regions attempting to document political histories of specific villages " (1983:ix). Chagnon, in studying what appear to be previously uncontacted people, gathers new information—information that should allow us to gain further insight into the Yanomami. Tierney claims to have uncovered new information as well: data relating to the specific villages and individuals referred to in Chagnon's famous *Science* article. He suggests that Chagnon overstates the Yanomami's murder and marriage patterns in the article. The dust jacket on Tierney's book asserts: "Tierney, who gained access to dozens of unedited audio tapes of documentaries, provides an astonishing link between the Atomic Energy Commission and . . . [Chagnon's] anthropological forays." In offering new material, anthropologists seem more credible than if they simply restate what others have asserted.

Third, anthropologists look for references to the work of other scholars: generally, the greater the number of sources cited, the more credible the work. By this standard, no one comes close to Tierney. He has more than 1,590 footnotes. He cites more than 250 books, dissertations, and magazine articles; 8 government documents; 13 films and documentaries; 36 unpublished sources; and more than 90 interviews.

A fourth technique for establishing credibility is to build one's new material on accepted knowledge (cf. Shapin 1994). If a new account overlaps with already accepted material, then it tends to be seen as credible to others. This is what occurred when Turner and Sponsel wrote their memo. Turner and Sponsel were familiar with many of Tierney's accusations against Chagnon; they had made similar charges themselves. So why not take Tierney's accusations regarding Neel equally seriously? Tierney seemed a credible researcher—based in the material of his they had read.

A fifth way to establish credibility is to speak from a position of status. People with high status tend to be seen as more credible than those with lower status. A good example is Clifford Geertz. Because he seems to have limited knowledge of the Yanomami or the whole controversy, one might wonder why he should review Tierney's *Darkness in El Dorado* for the *New York Review of Books* and offer his assessment of the controversy. The answer is that he is one of the best-recognized anthropologists in the United States. His aura of credibility extends beyond his areas of expertise.

"Hard charges," Geertz suggests, "demand hard evidence, or, failing that, at least an enormous mass of it." Tierney's effort in this direction, he continues, "is uneven, in many places vague or insubstantial, and in some, it is, as the critics have charged, simply unfair—ideologized second-guessing. But, as the instances accumulate and their implications come home, it all, in some strange way, begins to add up. Whatever caused the measles epidemic . . . a case gets made, however clumsily, that something was seriously amiss in the relation between these confident and determined *soi-disant* 'scientists' with their cameras, their vials, their syringes, and their notebooks and the beset and puzzled, put-upon 'natives' to whom they looked for facts to fill them with" (2001:20).

About placing blame on anthropologists, Geertz writes: "Given all that has happened to the Yanomami over the past half-century, encountering anthropologists . . . surely ranks as historical small change, a very small blip on a very large curve. . . . They have been plagued by a good deal more than measles which, however grave, are a one-time thing" (2001:21). It all sounds, well, authoritative. He seems to be speaking with the confidence of competence.

But should we trust such techniques? They make sense to most anthropologists, but there are flaws and fallacies in each of the techniques that need to be noted.

Let us start with "being there" as a way of establishing credibility. Certainly Chagnon gained expertise through extensive fieldwork. But as will become clear in part 2 of this book, other researchers who have lived longer among the Yanomami—Albert, Good, Lizot, Peters—disagree with Chagnon on certain points. "Being there" works only when no one else comes forward to challenge your account. Tierney has spent less time living among the Yanomami than Chagnon has. But Tierney supplements his observations with the work of the above noted anthropologists. Whom should we then believe?

The second technique is to present new information. But how new is Chagnon's new material? It appears his uncontacted villages had been previously contacted. Citing a host of references, Sponsel suggests that Yanomami communities "have been influenced by Western contact, directly and/or indirectly, for some 250 years. At various times these influences have included slave raiders, rubber tappers, loggers, miners, missionaries, explorers, scientists, the military, border commissions, government censuses, malaria patrols, and so on" (1998:113). And how do we know that Tierney's identifications of the villages Chagnon used in his *Science* article are accurate? Just because Tierney lists certain villages Chagnon visited does not mean these are the villages Chagnon used in his analysis. It may be new information, but is it correct?

The third technique is to gain credibility through extensive citations of others' work. Tierney's effort to do this has come in for extensive criticism. One expert on the Yanomami, Alcida Ramos, says: "*Darkness in El Dorado* has been commended . . . for its solid documentation. Indeed, there is a profusion of end notes, but these require close examination. For instance, to challenge Chagnon's data on polygyny, Tierney chooses a sentence from a Waorani ethnography. . . . To support his description of 'the sad history of the Marash-teri and their struggle with the gold rush,' he cites an article by Bruce Albert written about a Yanomami community well before the gold rush" (2001:275). Tierney has interviewed all the participants in the discussion in part 2 of this book: Albert, Hames, Hill, Martins, Peters, and Turner. At least two of them—Hames and Hill—strongly object to Tierney's summary of their conversations. What then should we infer about Tierney's massive documentation?

Fourth, we noted above that Turner and Sponsel were slow to challenge Tierney's accusations regarding Neel because Tierney's criticisms of Chagnon fit with what they themselves already knew. In the rush to warn leaders of the American Anthropological Association regarding the gathering storm, they perceived time to be of the essence. Once they had a chance to investigate the accusation that Neel played a key role in the spread of the measles epidemic, they found it to be wrong.

Finally, we should be cautious in accepting the proclamations of high-status anthropologists outside their areas of competence. We might wonder why high-status anthropologists should understand the controversy better than others—especially when they make no claims to have steeped themselves in the ethnographic material. I am uncertain what Geertz knows about the Yanomami. But I do know that in a *New York Review of Books* analysis of another controversy—between Obeyesekere and Sahlins regarding Captain Cook—he also positioned himself as the arbiter between squabbling intellectuals. In that controversy, Geertz missed important facts basic to the case (cf. Borofsky 1997). We need to be cautious in assuming that others—whatever their status—know things that reach beyond their areas of expertise.

In seeking to make sense of the Yanomami controversy, I am suggesting that we need to reflect on the ways in which we assess controversies within the discipline. We need to cast a critical eye on how we evaluate credibility because in the very ways we strive to resolve disputes we sometimes perpetuate them.

To summarize, the Yanomami controversy extends beyond the specific accusations made against one or another individual. It also involves issues of power—between anthropologists and those who help them—as well as intellectual competence regarding how anthropologists resolve controversies such as this. We will return to these issues in chapter 6.

5

KEEPING YANOMAMI PERSPECTIVES IN MIND

In dealing with the Yanomami controversy, we must not lose sight of the Yanomami themselves. Throughout the controversy, claims of concern for the Yanomami's welfare have produced a lot of political posturing. But as noted in chapter 1, the Yanomami do not seem to have substantially benefited from the piles of paper this posturing has produced. In talking about the Yanomami, we often seem to be talking about our hopes for ourselves as ethical professionals.

Hearing Yanomami voices and experiencing Yanomami perspectives on the controversy, however, is not easy to do because (1) the Yanomami speak with many voices, not one; (2) some of the events of interest happened decades ago; and (3) the material drawn from interviews is not easily presented to readers.

First, the Yanomami do not necessarily speak with a collective voice but with many voices, many perspectives. Yanomami are well aware of this. When the Yanomami Piri Xiriana visited the AAA's annual meeting in 2002, for example, he refused to act as a spokesperson for Yanomami views with the AAA, even on a matter where there appears to be broad Yanomami concern: the storage of Yanomami blood in the United States. He indicated that the matter was something Yanomami needed to discuss among themselves in their own gatherings. (He suggested the AAA send individuals to the Yanomami who could discuss the problem with them; together, they could decide how to proceed.) He pointedly rejected the association's proposal that the Yanomami send a few representatives to the United States to discuss the problem with AAA representatives. Piri Xiriana's subsequent attempt to foster discussion on the topic has focused on five villages along the upper Mucajaí River. But there are certainly other Yanomami, in both Brazil and Venezuela, who might be consulted. The problem is there is no collective body of Yanomami to represent their views to outsiders. Yanomami organize themselves in a range of groups but never as a whole tribe. It is one of their traits as Yanomami.

A second problem with these interviews is that the Yanomami interviewed are at times discussing experiences that occurred over thirty years ago. Some recall what they experienced as small children, others what they heard particular individuals say. These views have been shaped and reshaped with the passage of time.

One aspect of this, for example, is the perception by some Yanomami of themselves as "fierce." I can well imagine, as we read in chapter 1, certain Yanomami conveying in encounters with Chagnon that they were fierce. This would be a way of affirming their competence as warriors—especially if they are not as violent as non-Yanomami groups (as Tierney suggests) or if there is regional variation in Yanomami violence (as Chagnon suggests). It would be a politically useful ideology for intimidating others and protecting themselves no matter what the actual degree of violence was.

But such an assertion (particularly to outsiders) takes on a different tone during the political struggles to establish a Yanomami land reserve in Brazil in the 1980s. Asserting Yanomami fierceness became a political ploy by certain Brazilian politicians to subvert Yanomami demands for a large reserve. They depicted the Yanomami as too violent for a large reserve; they needed to be broken up into several smaller reserves. Despite considerable opposition, a single large reserve was eventually established in 1992.

Today for Yanomami to publicly affirm their fierceness to outsiders is basically to undermine their political cause. Periodically, one or another prominent individual in Brazil calls for reducing the size of the present reserve.

In searching for a sense of objectivity in this politically charged matter, readers need to remember that most Yanomami specialists view the Yanomami as less violent and warlike than Chagnon suggests. Still, we might wonder to what degree Yanomami self-representations to outsiders have changed with changing political times.

The third problem is that it is difficult to present the interview material to readers. My initial inclination was to offer verbatim transcripts so readers could see how the interviews unfolded word by word. Readers of these interviews indicated that they found the unedited format confusing. Some interviewees seemed to ramble, and the connection between particular points was not always clear. As a result, I have organized the interview material around certain themes. This format lacks the *in situ* sense of how Yanomami express themselves, but it allows readers to readily grasp how Yanomami perspectives on the controversy's central concerns differ from those expressed by anthropologists, the critical point of this chapter. (Readers interested in examining the unedited interviews can turn to the first page of the bibliography to find their locations on the internet.)

What comes through in the interviews is that the Yanomami are concerned about different things than we are. We are primarily concerned about the validity of Tierney's accusations; the Yanomami are more concerned about the blood collected by Neel that is still being preserved in American laboratories.

The excerpted interviews in this chapter come from six different sources. For the discussion in part 2, I asked both Bruce Albert and Lêda Martins—who were in Brazil during part of 2001—to interview several Yanomami in order to gain their perspectives on the issues raised by the controversy and what anthropologists might do to help them. Bruce Albert interviewed Yanomami activist Davi Kopenawa; the interview took place, in the Yanomami language, on April 8 at Boa Vista, the capital of the Brazilian state of Roraima, which is where most Brazilian Yanomami live. Lêda Martins conducted two sets of interviews: The first, on April 19, occurred during a conference on the health of indigenous people near the Brazilian capital of Brasília with five Yanomami (Carlos Krokonautheri, Ivanildo Wawanawetery, Roberto Pirisitheri, Geraldo Parawautheri, and Alexandre Hawarixapopitheri); Davi Kopenawa acted as a translator from Yanomami to Portuguese and, in the process, voiced some of his own opinions. The second interview occurred on May 18 in Boa Vista. This interview, in Portuguese, was with Geraldo Kuesitheri Yanomami and Peri Porapitheri.

Janet Chernela, a member of the AAA's El Dorado Task Force, interviewed Davi Kopenawa twice, once in Boa Vista on June 10, 2000, and once in a Yanomami village in Roraima on June 7, 2001. (Because the first of these interviews occurred before the publication of Tierney's book and is less relevant to the controversy's central concerns, it is not excerpted below.) Chernela also interviewed Julio Wichato on November 24, 2001, at Shakita, a small village by the Mavaca River in the upper Rio Orinoco region of Venezuela, during the first National Conference of Venezuelan Yanomami (called in part because of the controversy). Chernela indicates that she was interested in talking to a Yanomami "who was unbiased and familiar with the professional aspects of blood collection." (Wichato has worked as a nurse for the Venezuelan health ministry for the past eighteen years.) The interview was conducted in Spanish.

Janet Chernela also recorded two formal presentations in the United States by Yanomami relating to the controversy. One was by José Seripino, a Venezuelan Yanomami, at George Washington University on September 7, 2001. The other was by Toto Yanomami, a Brazilian Yanomami, on April 6, 2002, at a conference on the controversy held at Cornell University.

This chapter, then, includes a certain range of opinions. Brazilian and Venezuelan Yanomami are represented. And although Davi Kopenawa dominates the discussions—understandably, perhaps, because he is the Yanomami activist best known by non-Yanomami—other Yanomami have also been interviewed. To my knowledge, these interviews represent the major corpus of material presently available in English on Yanomami reactions to the controversy's central issues. The fact that more interviews are not readily available suggests listening to Yanomami perspectives remains a work in progress.

YANOMAMI BLOOD

Davi Kopenawa makes his concern clear. "My mother gave blood. Now my mother is dead. Her blood is over there [in the United States]. Whatever is of the dead must be destroyed. Our custom is that when the Yanomami die, we destroy everything. To keep it, in a freezer, is not a good thing." Toto Yanomami states: The doctors "collected these things: blood, urine [inaudible], saliva, and feces. I want it to come back to the Yanomami. . . . I want all of it returned. . . . Blood is important in shamanism. . . . All the blood of the Yanomami belongs to [the deity] Omami. . . . Those people have died! . . . Yanomami never take blood to keep. Yanomami don't . . . take blood to study and later keep [it] in the refrigerator. . . . The doctors have already examined this blood; they've already researched this blood. Doctors already took from this blood that which is good—for their children, for the future. . . . So we want to take all of this Yanomami blood that's left over." Ivanildo Wawanawetery makes the same point: "That person who donated blood and who . . . does not live anymore . . . that is an injustice. . . . Who knows . . . how many people have died and even today they have their blood. . . . [It] is in the other country. . . . Someone . . . who gave blood and no longer lives . . . and his blood is still in another country" (2001b).

The Yanomami remember receiving trade goods in exchange for their blood. Wawanawetery reports that the Americans "gave knives, beads, fishing line, and so they convinced the people" to give their blood. Kopenawa states: "The whites said things like this: 'I'm going to give you a machete . . . when you come give blood. . . . I'll give you fishing hooks!' That's why people went to them to give their blood" (2001a).

But there does not appear to be anything approaching informed consent from the Yanomami perspective regarding two critical issues. First, Yanomami claim they were never informed that the blood would be stored past its initial examination in American laboratories. Kopenawa notes (interspersing his views with those of Carlos Krokonautheri in the Martins interview): "The American didn't help to explain . . . 'Look, this blood is going to stay many years.' He didn't say that. . . . The Yanomami were thinking that he would take the blood and then read it and then throw it away. That's what the Yanomami thought. That's why they gave the blood. . . . They thought it was to see some disease, malaria, tuberculosis, flu, or some other disease" (2001c). Kopenawa repeats this point in his interview with Albert: "The white man didn't tell us . . . 'We're going to store your blood in the cold, and even if a long time goes by, even if you die, this blood is going to remain here'—he didn't tell us that! Nothing was said" (2001a).

Second, the Yanomami say they were never apprised of the blood test results—what was learned about Yanomami blood. As Kopenawa suggests, Yanomami presumed the blood samples were being taken not just for the benefit of the outsiders but for the benefit of Yanomami as well. He states: "We want to know the findings. What did they find in the blood—information regarding disease?" (2001b). José Seripino (in his presentation at George Washington University) makes the same point: "I was only ten years old. I thought 'Okay. This will help us.' But what happened? We haven't seen the outcome'" (2001). Julio Wichato observes, "The problem is that they studied it [the blood] and didn't send us the results. If they help us it's different. . . . It's important that they send the results" (2001). Wawanawetery asserts that the researcher "created fear . . . when he [the Yanomami] didn't give up his blood, the guy was going to get sick, right? If he didn't give blood the guy was going to get sick, he was going to die. Those who were donating blood would live" (2001). The implication I draw from this statement is that the Yanomami were told by a researcher that analysis of their blood would help them learn who had which diseases, thus facilitating treatment.

Yanomami appear to be of two minds regarding what should now be done with their deceased relatives' blood. On the one hand, some would like to be justly compensated for the blood used by Americans. This is Davi Kopenawa's point: "You should give something in return for what it [the blood] is worth. . . . The fact is, they already took away that blood. Go ahead. But give something in return for what it's worth. If you go ahead [with your research] without compensating us, we will feel injured" (2001a). Kopenawa says in another interview, "If . . . our blood is good for their bodies [i.e., helps them cure diseases]—then they'll have to pay. If it helped cure a disease over there, then they should compensate us" (2001b). This is the implication, too, in Toto Yanomami's statement quoted previously: "Doctors already took from this blood that which is good—for their children, for the future" (2002). Kopenawa suggests, "If they don't want to pay, then they should consider returning our blood. . . . If he [the American researcher] doesn't want to return anything, then lawyers will have to resolve the issue. I am trying to think of a word that whites do . . . sue. If he doesn't want to pay, then we should sue" (2001b). Kopenawa says (in the Martins interview, translating for Geraldo Parawautheri), "I'm going to translate. . . . He's the son of a chief, but his father passed away. . . . the blood can't be kept as if the Yanomami were alive. Since he's not alive [Geraldo's father], they can't [keep his blood]. . . . The *napë* [non-Yanomami] prohibit [things] as well. . . . If they want to [do] research, they have to pay the Yanomami. Then they can use it" (2001c).

On the other hand, some Yanomami want their blood destroyed or returned, period—even if it is valuable to American researchers. They feel that the researchers have had enough time to gain what they want from the blood. Wichato says, "We don't want them to continue studying our blood. . . . Whether they send the [collective] results or not—they cannot study it anymore. They have to return it or destroy it. That's all! If they send the results we won't know whose blood belongs to whom anyway. . . . We don't want them to continue working with this blood" (2001). In translating the Alexandre Hawarixapopitheri interview, Kopenawa states that Hawarixapopitheri's "father died. . . . This blood that's there is already dead, already died, and we don't want this blood kept as if the owner were alive. But the owner's dead, so they have to get rid of the blood. That's what he wants" (2001c). Toto Yanomami says, "This blood belonging to the Yanomami is here in this country [the United States]. We met in our communal longhouse to talk about this. We thought that it had been thrown out. But it still exists. So I came here to find this blood and take it back. . . . We hope that you whites can help us resolve this situation to get this blood and take it back. . . . I want all that the whites took. I want all of it returned" (2002).

THE YANOMAMI AS FIERCE

Another topic that comes up in the interviews, particularly for Davi Kopenawa, is how anthropologists depict the Yanomami—especially as fierce. Kopenawa is aware of Chagnon's criticisms of him, and he in turn has strong criticisms of Chagnon. We must remember that Yanomami fierceness is a politically charged issue and Kopenawa is dealing not only with the explicit question of whether Yanomami are fierce but with the implicit concern that the Yanomami way of life be valued by outsiders (to protect the Yanomami and their reserve from opposing political forces in Brazil). Following is an excerpt from Chernela's interview with Kopenawa:

KOPENAWA: So this Chagnon, . . . he said that the Yanomami are no good, that the Yanomami are ferocious. So this story, he made this story [up]. He took it to the United States. He had a friend who published it. It was liked. His students thought that he was a courageous man, an honest man, with important experience.

CHERNELA: What is the word for courageous?

KOPENAWA: *Waiteri*. He is *waiteri* because he was there. He is *waiteri* because he was giving orders. . . . He ordered the Yanomami to fight among themselves. He paid with pans, machetes, knives, fishhooks. . . . The life of the Indian that dies is very expensive. But he paid little. He made them fight more to improve his work.

CHERNELA: But why did he want to make the Yanomami fight?

KOPENAWA: To make his book. To make a story about fighting among the Yanomami. He shouldn't show the fights of the others. The Yanomami did not authorize this. He did it in the United States. He thought it would be important for him. He became famous. He is speaking badly about us. He is saying that the Yanomami are fierce, that they fight a lot, that they are no good. . . . The Yanomami should not authorize every and all anthropologist who appears. . . . When he [Chagnon] arrived [at a village], and called everyone together, he said, [Yanomami] . . . "That *shabono* [village], three or four *shabonos*," as if it were a ball game [with Yanomami fighting each other]. "Whoever is the most courageous will earn more pans. If you kill ten more people I will pay more. If you kill only two, I will pay less." . . . Our relatives came from Wayupteri and said, "This Chagnon is very good. He gives us a lot of utensils. He is giving us pans because we fight a lot."

CHERNELA: They killed them and they died [i.e., Yanomami killed other Yanomami]?

KOPENAWA: Yes. Because they used poison on the point of the arrow. This isn't good. This kill[ing] . . . Children cried; fathers, mothers, cried. Only Chagnon was happy. Because in his book he says we are fierce. We are garbage. The book says this; I saw it. I have the book. He earned a name . . . *Watupari*. It means king vulture—that eats decaying meat. We use this name for people who give a lot of orders. . . . He ordered the Yanomami to fight. (2001b)

José Seripino, too, perceives Yanomami fierceness not as a trait embedded in Yanomami society but as something stimulated by Chagnon. "It's not all the time that the Yanomami are angry. . . . Sometimes not. It's not all the time. This is a lie that he [Chagnon] invented in his book. If he treats the Indian badly then the Yanomami could get angry" (2001).

PERCEPTIONS OF CHAGNON

This brings us to the question of how Yanomami perceive Napoleon Chagnon and his fieldwork. While some have mixed reactions, most Yanomami interviewed perceive Chagnon's fieldwork in negative terms. Kopenawa is the most vocal but also is perhaps the most interesting. One might read Kopenawa's remarks about Chagnon's losing his fear as the Yanomami version of Chagnon's description in chapter 2 of how he learned to hold his own against repeated Yanomami demands. The following is from Chernela's interview with Kopenawa:

> When he [Chagnon] first arrived he was afraid. Then he developed courage. He wanted to show that he was brave. If the Yanomami could beat him, he could beat them. This is what the people in Toototobi told us. I am here in Watorei, but I am from Toototobi. . . .
>
> So I knew him. He arrived speaking Yanomami. People thought he was Yanomami. He accompanied the Yanomami in their feasts . . . taking [the hallucinogen] *ebena*, and after, at the end of the feast, the Yanomami fought. . . . [he] took photos. And so he saved [the fight], he "kept" the fight. So, after, when the fight was over, and the Yanomami lay down in their hammocks, in pain, the anthropologist recorded it all on paper. He noted it all on paper. He wrote what he saw, he wrote that the Yanomami fought. He thought it was war. This isn't war, no! . . . He should have helped us to stop fighting. But he didn't. He's no good. (2001b)

Regarding the collecting of genealogical information, Kopenawa observes: "He wrote down the day, the time, the name of the *shabono*, the name of the local descent group. He put down these names. But he didn't ask us. So we are angry." Regarding Chagnon's gifts, Kopenawa states: "He had a lot of pans. I remember the pans. Our relatives brought them from there. They were big and they were shallow. He [Chagnon] bought them in Venezuela" (2001b).

Wichato provides a personal account of an event described in Tierney, the destruction of a Yanomami village's *shabono* by a helicopter (2000:4–5). "Yes, I saw [Chagnon] . . . when I was eighteen. . . . He contracted a helicopter. The pilot knew me and I had no way to get there. So he said, let's go. He took me [to the helicopter] and there was Chagnon. We got to Ocamo. . . . He took me to Siapa. . . . There was a VERY large *shabono*. "Let's go down here" [he said]. The helicopter was BIG—it blew out houses [i.e., parts of the *shabono*] within twenty meters! So people came out with bows and arrows to shoot the helicopter. Chagnon said to go back down. The pilot said no and went up again. Then Chagnon wanted to go back. . . . He ruined the *shabono*. . . . This is what he was like, Chagnon. He got fuel and we went again" (2001).

Regarding Chagnon's gift giving, José Seripino suggests that Chagnon did not always keep his promises. In the village of Shakita (a village Tierney claims was named for Chagnon [2000:137]), Seripino asserts that Chagnon "worked with this man closely. Now . . . he [Chagnon] promised this person a motor and he disappeared without giving it. He never [returned and] paid that debt" (2001). In fairness to Chagnon, it is unclear whether this "promise" was part of what Chagnon perceived as yet another Yanomami demand on his limited goods—a subject discussed in chapter 2—or a real commitment made but never fulfilled. Nor is it clear whether Chagnon intended to provide the motor but was unable to because of restrictions on his returning. Still, it is important to recognize what is being conveyed: some Yanomami feel Chagnon should have provided particular informants with more gifts than he did.

KOPENAWA'S VIEW OF THE CONTROVERSY

Davi Kopenawa has a definite view on the controversy between Chagnon and Tierney. As part of her interview, Chernela had Kopenawa ask her questions and comment on her answers. Kopenawa asks: "I want to ask you about these American anthropologists. Why are they fighting among themselves? Is it because of this book [by Patrick Tierney]? Is this book bad? Did one anthropologist like it and another one say it's wrong?" About Tierney, Kopenawa says, "I met him in Boa Vista. I went to his house. He didn't say anything to me about what he was doing" (2001b).

Kopenawa continues, "I don't like these anthropologists who use the name of the Yanomami on paper, in books. One doesn't like it. . . . This isn't good. They are using our name as if we were children. The name Yanomami has to be respected. It's not like a ball to throw around, to play with, hitting from one side to another" (2001b).

Kopenawa discusses the money he presumes various people are making from publishing books on the Yanomami.

> I think that the head of the anthropologist . . . has [or is focused on] money. . . . Chagnon made money using the name of the Yanomami. He sold his book. Lizot too. . . . I want to know how much they are making each month. How much does any anthropologist earn? And how much is Patrick [Tierney] making? Patrick must be happy. This is a lot of money. They may be fighting but they are happy. They fight and this makes them happy. They make money and fight. . . .
>
> Patrick left the fight to the others! He can let the anthropologists fight with Chagnon, and he, Patrick, he's outside, he's free. He's just bringing in the money—he must be laughing at the rest. It's like starting a fight among dogs. Then they fight, they bark and he's outside. He spoke bad of the anthropologist—others start fighting, and he's gaining money!
>
> The name Yanomami is famous . . . more famous than the name of any anthropologist. So he [Patrick] is earning money without sweating, without hurting his hands, without the heat of the sun. He's not suffering. He just sits and writes, this is great for him. He succeeded in writing a book that is bringing in money. Now he should share some of this money with the Yanomami. We Yanomami are here, suffering from malaria, flu, sick all the time. But he's there in good health—just spending the money that he gained in the name of the Yanomami Indians. . . . This is a fight between men who make money. (2001b)

YANOMAMI VIEWS OF ANTHROPOLOGY AND ANTHROPOLOGISTS

The Yanomami interviewed were familiar with enough anthropologists to realize that not all anthropologists are alike. Toto Yanomami stated (in his presentation to anthropologists at Cornell): "You are anthropologists. You work! Some work well, others badly" (2002). The problem, as Peri Porapitheri indicates, is that Yanomami cannot initially tell the good from the bad:

> If he [the anthropologist] wanted to work clean, without doing things against us, he could work in any region. . . . We do not recognize an exploiter at first, but we are going to find out through his voice: "Look! I came here to help sick people, to [build] a hospital, to give medicine, help to acquire vaccines. I will give you presents."
>
> This type of talk I already know, he wants to negotiate, right? He wants to do research, and there are so many things, right? Research on health, research to deceive the Yanomami, research to exploit. He will take advantage if Yanomami believe in what he says. He is going to say that the Yanomami are agreeing to everything.
>
> I am not going to believe in everything. . . . I am going to ask him a lot of questions. . . . There are . . . many white people whose work we do not know. . . . So, he [the anthropologist] . . . start[s] working, then after[ward] he take[s] away pictures, books, he sells, he produces and sells. He will say, "Look! I did this, I did that. And I will send the government to help you and money will come in your name because you are suffering very much. I have spoken with the Ministry of Health." Then everybody will believe him because the majority of the Yanomami do not know the white people's ways. (2001b)

From the Yanomami perspective, "helping" generally means helping to fight disease. Geraldo Kuesitheri Yanomami explains: "An anthropologist, if he learns to work with health teams, this helps us. This is very important. . . . [The anthropologist] knows how to work with photos, with the writing of the Yanomami [language], translation, translating Yanomami [to] Portuguese. If someone were to do this without an anthropologist, it wouldn't come out right. There would be no way [for outsiders] to explain things to the Yanomami. Without an anthropologist, there's no way [for the Yanomami] to understand" (2001).

Kopenawa makes the same point: "An anthropologist should really help, as a friend. He shouldn't deceive. He should defend . . . [a Yanomami] when he is sick, and defend the land as well . . . saying [to others] 'You should not come here—the Yanomami are sick' [and could get sicker if you come because the Yanomami are so vulnerable to outside diseases]. If a Yanomami gets a cold, he can die" (2001b).

Kopenawa makes clear that anthropologists can act as intermediaries for the Yanomami, helping outsiders to understand them and vice versa. Anthropologists can give voice to what Yanomami are thinking. Even Chagnon, from Kopenawa's perspective, has an important role to play in this regard. (He is more fluent in the Shamatari dialect than Kopenawa.) "Bruce Albert, Alcida Ramos are not Yanomami. . . . Look, Alcida [Ramos] speaks Sanuma. Chagnon speaks Shamatari. And Bruce [Albert] speaks our language. So there are three anthropologists who can call three Yanomami [groups together] to speak at . . . [a collective] meeting [of these distinct groups]. . . . The Yanomami can speak his own language. These anthropologists can translate [for outsiders what is being said]. They [the outsiders] have to hear our language. They have to hear us in our own language. What does the Yanomami think? What does the Yanomami think is beautiful? You have to ask the Yanomami themselves" (2001b).

Kopenawa is clearly familiar with the ways to woo Western readers as well as with the value of positive publicity. He has to be if he is to be an effective advocate of Yanomami interests in Brazil and in the international community. Kopenawa is able to phrase his points in a way that resonates with Western values. Of all the Yanomami quoted in this chapter, Kopenawa is most able to inspire us about the Yanomami cause. Asked by Chernela if he had a message for the American Anthropological Association, Kopenawa replied:

> I would like to speak to the young generation of anthropologists. Not to the old ones who have already studied and think in the old ways. I want to speak to the anthropologists who love nature, who like indigenous people—who favor the planet earth and indigenous peoples. This I would like. . . . To write a new book that anyone would like, instead of speaking badly about indigenous peoples.
>
> There must be born a new anthropologist who is in favor of a new future. And the message I have for him [or her] is to work with great care. If a young anthropologist enters here in Brazil or Venezuela, he should work like a friend. Arrive here in the *shabono*. He should say, "I am an anthropologist; I would like to learn your language. After, I would like to teach you." Tell us something of the world of the whites. The world of the whites is not good. It is good, but it is not all good. There are good people and bad people. So, "I am an anthropologist here in the *shabono*, defending your rights and your land, your culture, your language; don't fight among yourselves, don't kill your own relatives."
>
> We already have an enemy among us—it is disease. This enemy kills indeed. It is disease that kills. We are all enemies of disease. So the anthropologist can bring good messages to the Indian. They can understand what we are doing, we can understand what they are doing. . . . [They can help] the Yanomami understand the ways of the whites [so we can] . . . protect ourselves.
>
> They cannot speak bad of the Yanomami. They can say, "The Yanomami are there in the forest. Let's defend them. Let's not allow invasions [of gold miners]. Let's not let them die of disease." But not to use the name of the Indian to gain money. The name of the Indian is more valuable than paper. The soul of the Indian that you capture in your image is more expensive than the camera with which you shoot it.
>
> You have to work calmly. You have to work the way nature works. You see how nature works. It rains a little. The rain stops. The world clears. This is how you have to work, you anthropologists of the United States. (2001b)