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Yanomami

THE FIERCE CONTROVERSY AND WHAT WE CAN LEARN FROM IT

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WITH

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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 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. 11). 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