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## THE CONTROVERSY AND THE BROADER ISSUES AT STAKE

At first glance, the Yanomami controversy might be perceived as being focused on a narrow subject. It centers on the accusations made by the investigative journalist Patrick Tierney against James Neel, a world-famous geneticist, and Napoleon Chagnon, a prominent anthropologist, regarding their fieldwork among the Yanomami, a group of Amazonian Indians. But it would be a mistake to see the Yanomami controversy as limited to these three individuals and this one tribe.

First, the accusations Tierney made against Neel and Chagnon in his book *Darkness in El Dorado* (2000) generated a media storm that spread around the world. People knew about the accusations in New York, New Zealand, and New Guinea. Tierney accused Neel and Chagnon of unethical behavior among the Yanomami that at times bordered on the criminal. Many perceived the problem as being larger than the mistakes of two famous scientists. They wondered if anthropology and perhaps science itself had gone astray in allowing such behavior to take place.

Second, and critical for the themes of this book, the way the controversy played out offers an important lens through which to examine the entire discipline of anthropology. We see not only how anthropologists idealize themselves in describing their work to others. We also see the actual practice of anthropology—up close and clear. We are led to explore questions central to the discipline.

Readers should keep this point in mind as they read *Yanomami: The Fierce Controversy and What We Can Learn from It*. The controversy goes beyond what Neel and Chagnon stand accused of. It extends beyond the media storm generated by Tierney's accusations and the accusations that others, in turn, made against him. The controversy draws us into examining issues at the heart of modern anthropology. As we will see, there are lessons for the learning here for everyone, whatever their specialty, whatever their status within the discipline. Let me begin by providing certain background information. For clarity's sake, I order the material as a set of commonly asked questions.

**WHO ARE THE YANOMAMI AND WHY  
ARE THEY IMPORTANT IN ANTHROPOLOGY?**

Through the work of Chagnon and others, the Yanomami have become one of the best-known, if not the best-known, Amazonian Indian group in the world. People in diverse locales on diverse continents know of them. They have become a symbol in the West of what life is like beyond the pale of “civilization.” They are portrayed in books and films, not necessarily correctly, as one of the world’s last remaining prototypically primitive groups.

The Yanomami are also one of the foundational societies of the anthropological corpus. They are referred to in most introductory textbooks. Anthropology has become increasingly fragmented over the past several decades, with anthropologists studying a wide array of societies. The Yanomami—along with the Trobriand Islanders, the Navajo, and the Nuer—constitute shared points of reference for the discipline in these fragmented times. The Yanomami are one of the groups almost every anthropology student learns about during his or her course of study.

The Yanomami tend to be called by three names in the literature: Yanomami, Yanomamö, and Yanomama. The names all refer to the same group of people. Different subgroups are labeled (and label themselves) with different terms; there is no broadly accepted indigenous term for the whole group. There is a politics of presentation regarding which of these three terms one uses. *Yanomamö* is the term Chagnon gave the collective group, and those who refer to the group as *Yanomamö* generally tend to be supporters of Chagnon’s work. Those who prefer *Yanomami* or *Yanomama* tend to take a more neutral or anti-Chagnon stance. I use *Yanomami* in this book because of its wide usage and greater neutrality. (When citing Chagnon in describing the group, I use *Yanomamö* to remain consistent with his usage.) Readers can substitute whichever term they wish.

Chagnon wrote *Yanomamö: The Fierce People* (1968) at a critical time in the discipline’s development. American universities expanded significantly in the 1960s and 1970s, and, related to this, so did the discipline of anthropology. Prior to the 1950s, American anthropology had focused on the native peoples of North America and was only seriously turning, in the 1950s and 1960s, to other areas of the world. The Holt, Rinehart and Winston series in which Chagnon published *Yanomamö* emphasized a broadening of the anthropological corpus. The series offered new works for new times. The foreword to *Yanomamö* states that the case studies in the series “are designed to bring students, in beginning and intermediate courses . . . insights into the richness and complexity of human life as it is lived in different ways and in different places” (1968:vii).

I presume, though I have no way of knowing for certain, that at one time or another the majority of anthropologists have read Chagnon’s book. At least one, and perhaps several, generations of American anthropologists have been raised on it.

The Yanomami are a tribe of roughly twenty thousand Amazonian Indians living in 200 to 250 villages along the border between Venezuela and Brazil. "The fact that the Yanomamö live in a state of chronic warfare," Chagnon writes, "is reflected in their mythology, values, settlement pattern, political behavior and marriage practices" (1968:3). He continues: "Although their technology is primitive, it permits them to exploit their jungle habitat sufficiently well to provide them with the wherewithal of physical comfort. The nature of their economy—slash-and-burn agriculture—coupled with the fact that they have chronic warfare, results in a distinctive settlement pattern and system of alliances that permits groups of people to exploit a given area over a relatively long period of time. . . . The Yanomamö explain the nature of man's ferocity . . . in myth and legend, articulating themselves intellectually with the observable, real world" (1968:52–53). Chagnon notes that members of one patrilineage tend to intermarry with members of another, building ties of solidarity between the lineages through time. The local descent group—the patrilineal segment residing in a particular village—does not collectively share corporate rights over land. Rather it shares corporate rights over the exchange of women (1968:69), whose marriages are used to build alliances. Chagnon observes, "The fact that the Yanomamö rely heavily on cultivated food has led to specific obligations between members of allied villages: . . . The essence of political life . . . is to develop stable alliances with neighboring villages so as to create a social network that potentially allows a local group to rely for long periods of time on the gardens of neighboring villages" when they are driven from their own by enemy raids (1968:44). While stressing the violent nature of Yanomamö life, Chagnon indicates that there are graduated levels of violence with only the final one—raiding other villages—equivalent to what we would call "war."

It is Chagnon's description of the Yanomami as "in a state of chronic warfare" that is most in dispute. The French anthropologist Jacques Lizot, in *Tales of the Yanomami*, writes: "I would like my book to help revise the exaggerated representation that has been given of Yanomami violence. The Yanomami are warriors; they can be brutal and cruel, but they can also be delicate, sensitive, and loving. Violence is only sporadic; it never dominates social life for any length of time, and long peaceful moments can separate two explosions. When one is acquainted with the societies of the North American plains or the societies of the Chaco in South America, one cannot say that Yanomami culture is organized around warfare as Chagnon does" (1985:xiv–xv).

Chagnon depicts the Yanomami as "the last major primitive tribe left in the Amazon Basin, and the last such people *anywhere on earth*" (1992b:xiii). We need to note, however, that the Yanomami have been in direct or indirect contact with westerners for centuries (see Ferguson 1995:77–98). They are not a primitive isolate lost in time. Ferguson writes: "The Yanomami have long depended on iron and steel tools. All ethnographically described Yanomami had begun using metal tools long before any anthropologist arrived" (1995:23).

In providing this brief overview, I have focused on Chagnon's *Yanomamö* because it is the most widely known account. But there are other recognized

ethnographers who have written about the Yanomami who might be cited as well: notably, Bruce Albert, Marcus Colchester, Ken Good, Ray Hames, Jacques Lizot, Alcida Ramos, Les Sponsel, and Ken Taylor.

### WHO ARE THE CONTROVERSY'S MAIN CHARACTERS?

The three individuals who have played the most important roles in the controversy and whose names are repeatedly referred to in discussions of it are James Neel, Napoleon Chagnon, and Patrick Tierney.

The late **James Neel** has been called by many the father of modern human genetics. He served on the University of Michigan's faculty for more than forty years, becoming one of its most distinguished members. He was elected to the National Academy of Sciences as well as to the American Academy of Arts and Sciences and was awarded the National Medal of Science and the Smithsonian Institution Medal. Neel is perceived as the first scientist to recognize the genetic basis for sickle cell anemia. He conducted research on the aftereffects of atomic radiation with survivors of the Hiroshima and Nagasaki bombings of World War II in Japan. He also suggested not only that there was a genetic basis for several modern diseases such as diabetes and hypertension but that such propensities resulted from an evolutionary adaptation to environments where salt and calories were less than abundant. He died in 2000, some months before the publication of *Darkness in El Dorado*.

Neel became interested in Amazonian Indians because of his research relating population genetics to principles of natural selection—whether certain genetic structures contained particular evolutionary adaptive advantages. Realizing that detailed studies of “civilized populations” would prove less instructive for examining early human genetic adaptations than “tribal populations,” having the Amazon region fairly accessible, and knowing that Amerindians had entered the Americas fairly recently (he believed between fifteen and forty thousand years ago), Neel sought out relatively undisrupted groups in the Amazon for study. He wrote in his autobiography: “I realized we would probably never assemble from studies of existing tribal populations the numbers of observations necessary to relate specific genes to specific selective advantages, but at least we could take steps to define the range of population structures within which the evolutionary forces shaping humans had to operate” (1994:119). And in the journal *Science* Neel indicates that his studies were based on the assumption that Amazonian Indians were “much closer in their breeding structure to [early] hunter-gatherers than to modern man; thus they permit cautious inferences about human breeding structure prior to large-scale and complex agriculture” (1970:815). Initially, Neel studied the Shavante, another Amazonian Indian group. But in 1966 he turned to the Yanomami and worked with them until roughly 1976.

Two additional points need to be noted. First, Neel worked closely with Napoleon Chagnon during this period and, in the early years, helped fund Chagnon's research through his own research grants (which came partly from the Atomic Energy Commission). He viewed Chagnon as "indispensable" to his program: Napoleon Chagnon "had sought me out in Ann Arbor . . . having heard of our developing program. By virtue of the contacts I had already made, I could facilitate his entry into the field; he, for his part, in addition to pursuing his own interests, could put together the village pedigrees so basic to our work" (1994:134). Neel indicates in his autobiography that he encouraged Chagnon to work among the Yanomami.

Second, a devastating measles epidemic broke out "coincident with," to use Neel's phrasing, his arrival in the field in 1968. Neel indicated he had brought two thousand doses of measles vaccine and had planned to hand these over to missionaries in the region. But faced with the epidemic, Neel and his team vaccinated many Yanomami as well. Here is how Neel described his actions: "Much of our carefully designed protocol for that expedition was quickly scrapped as we dashed from village to village, organizing the missionaries, ourselves doing our share of immunizations but also treatment when we reached villages to which measles had preceded us. We always carried a gross, almost ridiculous excess of antibiotics—now we needed everything we had, and radioed for more" (1994:162). To what degree this description accurately reflects Neel's actions during the epidemic is one of the critical questions in the controversy. Tierney accused Neel of worsening the measles epidemic through his actions; others have suggested Neel could have done more than he did to save Yanomami lives during the epidemic.

**Napoleon Chagnon**, a retired professor of anthropology at the University of California, Santa Barbara, is one of the best-known members of the discipline. His writings, particularly his introductory ethnography *Yanomamö: The Fierce People* and the films associated with it have made his name familiar to millions upon millions of college students since the 1960s. It is not too far-fetched to suggest that Chagnon helped make the Yanomami famous as a tribe around the world and the Yanomami, in turn, have been the basis for Chagnon's own fame.

As is perhaps fitting given the evolutionary orientation of the University of Michigan's Anthropology Department at the time he received his doctorate (1966), Chagnon has emphasized an adaptive/evolutionary perspective in his writings. In the first edition of *Yanomamö*, for example, he stressed that one needed to see Yanomamö social life as an adaptation not only to the physical environment but also to the social and political environment—including chronic warfare.

Readers should keep in mind several points regarding Napoleon Chagnon as they proceed further into the politics surrounding the controversy.

First, Chagnon is a good writer. His chapter "Doing Fieldwork among the Yanomamö" has become a classic in the social sciences. It portrays in vivid terms his early fieldwork experiences in a way that captures the imagination of readers within and beyond anthropology. His basic ethnography of the Yanomami,

*Yanomamö*, has sold perhaps three million copies—far more than any other ethnographic work in modern times.

Second, Chagnon is a dedicated field-worker. Unlike most anthropologists of his or the present generation, Chagnon has—admirably in my view—striven to go back to the Yanomami year after year to study them through time. He has made at least twenty-five visits since beginning his fieldwork among them in 1964, has resided among the Yanomami for over sixty-three months, and has visited more than sixty of their villages. Few anthropologists can make such a claim, especially for a group in a remote region that is far from the creature comforts of their own homes. The problem is that when the Venezuelan and Brazilian governments restricted his field access, Chagnon engaged in various efforts, some of them in violation of Venezuelan law, to continue studying the Yanomami.

Third, Chagnon is controversial. His adaptive/evolutionary approach runs counter to the dominant trend in cultural anthropology, which focuses on how cultural contexts shape human behavior. He is more concerned with the biological underpinnings of human behavior. In trying to make sense of Yanomami conflicts over women, Chagnon states (as quoted in an article about him in *Scientific American*): “I basically had to create . . . my own theory of society.” The article continues: “Chagnon’s Darwinian perspective on culture jibed with Harvard University scientist E. O. Wilson’s 1975 treatise on animal behavior, *Sociobiology*. Chagnon—who tends to refer to his detractors as Marxists and left-wingers—thus became identified with that school of thought, which also made him unpopular among social scientists who believe that culture alone shapes human behavior” (Wong, 2001:2). Chagnon writes, “For better or worse, there is a definite bias in cultural anthropology favoring descriptions of tribal peoples that characterize them as hapless, hopeless, harmless, homeless, and helpless. . . . The Yanomamö are definitely not that kind of people, and it seemed reasonable to me to point that out, to try to capture the image of them that they themselves held. They frequently and sincerely told me . . . ‘We are really fierce; Yanomamö are fierce people’” (1992 b:xv).

As previously noted, this depiction of the Yanomami as the “fierce people” has been challenged by other Yanomami specialists. There is a political context to this. During the debates over whether or not to set aside a large reserve in Brazil for the Yanomami in the 1980s and early 1990s—one was finally established in 1992—various Brazilian politicians used the depiction of the Yanomami as violent to suggest that they needed to be split up into several small reserves to reduce conflict among them. (The plan, not coincidentally, would have allowed for more gold mining in the region.) What upset many Yanomami specialists was that Chagnon spoke out against this misuse of his work by Brazilian politicians only in the English-speaking press, never in the Portuguese-speaking press of Brazil, where it would have done the most good.

Fourth, Chagnon has been far more forthcoming regarding the details of his fieldwork than have most anthropologists. He is quite open, for instance, about the manipulative techniques he adopted to gather information when informants

lied to him, as well as about the lies he himself told to keep Yanomami from asking for his food. He openly admits that the Yanomami made death threats against him. Few anthropologists have been as candid about their fieldwork experiences as Chagnon, and fewer still at the time he wrote about them. Most anthropologists depict their fieldwork in fairly rosy terms, whether or not they actually experienced it that way. The problem for Chagnon is that certain of the fieldwork details he is so forthcoming about violate the American Anthropological Association's code of ethics.

**Patrick Tierney** is a freelance investigative journalist based in Pittsburgh. He obtained an undergraduate degree in Latin American studies from the University of California at Los Angeles. Those who interact with him on a personal level describe him as gentle and soft-spoken.

Tierney's first book, *The Highest Altar: The Story of Human Sacrifice*, was published in 1989. Clarebooks.co.uk Online Used Books describes it thus: "In 1983 Patrick Tierney went to Peru on an assignment to cover the autopsy of a well preserved five-hundred year old mummy. It was discovered that the child had been buried alive, the victim of human sacrifice. . . . [Tierney] went on to discover that this ancient ritual is apparently still being practiced and tells of his attempts to track down these stories in order to discover the motives behind sacrifice, the motives of the shamans and brujos who perform it." The book is now out of print. But according to Tierney's biographical information, it has been the subject of a National Geographic documentary.

Tierney spent eleven years researching and writing *Darkness in El Dorado*. He started out investigating the disruptive impact gold mining and gold miners were having on the Amazonian region, including on the Yanomami. At some point in this research he turned his attention to the scientists and journalists who have worked among the Yanomami. His gives an account of his research in an article in the *Pittsburgh Post-Gazette*:

I originally went there [to the Amazon] just documenting the mayhem that was going on . . . and trying to understand what was happening and perhaps alert people as to what can be done to help them. But as that evolved, my own participation changed. . . . It just didn't seem to be an adequate response to document people's deaths in the middle of these kinds of circumstances. . . . [The story about Neel and Chagnon] wasn't the story I was looking for initially, but it's what I came up with. . . . And what seemed to me to be the real story is that these people [the Yanomami] have been used to fulfill fantasies, scientific paradigms and preconceptions. And they've been used in ways that have been extremely harmful to them. (Srikameswaran 2000)

Tierney makes a considerable effort to give *Darkness in El Dorado* the trappings of academic scholarship. The book contains more than 1,590 footnotes; the bibliography contains more than 250 books. The question, however, is whether Tierney's years of research and voluminous citations add up to a credible work.

Many suggest that his supporting data are stronger for his case against Chagnon than for his case against Neel. Regarding his claim that Neel helped make the 1968 measles epidemic worse through his actions, the overwhelming consensus is that Tierney is wrong.

To understand the media storm surrounding *Darkness in El Dorado*, readers should take note of how Tierney's publisher publicized it. A statement inside the book's dust jacket (in the hardcover edition) reads in all capitals: "One of the most harrowing books about anthropology to appear in decades. *Darkness in El Dorado* is a brilliant work of investigation that chronicles the history of Western exploitation of the Yanomami Indians." And a CNN.com "Book News" report, dated October 2, 2000, notes, the "publisher W. W. Norton . . . is billing the book as 'an explosive account of how ruthless journalists, self-serving anthropologists, and obsessed scientists placed one of the Amazon basin's oldest tribes on the cusp of extinction.'"

In addition to James Neel, Napoleon Chagnon, and Patrick Tierney, there are three minor characters and one religious group that should be noted here because they are sometimes referred to in the controversy.

**Marcel Roche** is a Venezuelan doctor. As part of his goiter research, he administered to Yanomami small doses of radioactive iodine in 1958, 1962, and 1968 to measure their iodine metabolism. Apparently none of the Yanomami tested suffered from goiter problems, nor have Yanomami in general suffered from the disease. The Yanomami were simply used as a control study to enhance Roche's understanding of the disease. Most people agree that Roche never asked for what is today termed informed consent—permission from subjects to conduct research on them.

**Jacques Lizot** is a prominent French anthropologist who lived among the Yanomami for more than twenty years. He is highly critical of Chagnon's writings. Two points tend to be repeatedly asserted about Lizot's time in the field: that he was a strong public defender of Yanomami rights and that he had homosexual relations with a number of Yanomami boys. Related to these sexual relations, Tierney writes: "Lizot probably distributed more clothes and shotguns than any other individual among the Yanomami" (2000:141). And: "Whatever homosexual practices the Yanomami had prior to Lizot's arrival, shotgun-driven prostitution is nothing to brag about in their culture" (2000:137). Lizot has written two books on the Yanomami: *The Yanomami in the Face of Ethnocide* (1976) and *Tales of the Yanomami: Daily Life in the Venezuelan Forest* (1985).

**Ken Good** was a doctoral student of Chagnon's who had a falling-out with him after they spent time together in the field. (He ultimately got his doctorate working with Marvin Harris, a critic of Chagnon.) Good spent twelve years among the Yanomami and married a Yanomami (Yarima), from whom he is now divorced. He has written about his experiences in *Into the Heart: One Man's Pursuit of Love and Knowledge among the Yanomama* (1991). Building on what Lizot wrote, Good observes, "Chagnon made . . . [the Yanomama (or Yanomami)] out to be warring, fighting, belligerent people. . . . That may be his image of the Yanomama; it's certainly not mine" (1991:175).



The Catholic *Salesian missionaries* have had a prominent presence in Yanomami territory for decades. Early in the twentieth century, Venezuela legally granted the Salesian missionaries responsibility for educating the indigenous inhabitants of the Amazonas region (which includes the Yanomami). That responsibility continues today. Both Chagnon and Lizot have come into conflict with the Salesians. While they have had positive things to say about the missionaries, both have been highly critical as well. One outside observer labeled Chagnon's conflict with the Salesians a "turf war" over who would control research among the Yanomami (Salamone 1996:4). (Chagnon views the Salesians as partly to blame for his being officially barred from studying the Yanomami in Venezuela.)

### WHAT EXACTLY IS THE YANOMAMI CONTROVERSY?

Answering this question draws us into examining not only the accusations Tierney made against Neel and Chagnon in *Darkness in El Dorado* but a number of other issues as well. Let me start with Tierney's accusations and then move on to the additional issues.

#### The Accusations

Tierney made a number of accusations against a number of people in *Darkness in El Dorado*. But the central ones—and the ones latched onto by the media—involved Neel and Chagnon.

Tierney makes two basic accusations against Neel: (1) that Neel helped make the measles epidemic worse, rather than better, through the actions he took to fight the epidemic and (2) that Neel could have done more than he did to help the Yanomami at this time. Because the first of these accusations in effect charged a distinguished scientist with facilitating the deaths of Yanomami, it received the most media attention. This accusation has been dismissed by most people; the second is very much with us.

Tierney makes seven basic accusations against Chagnon: (1) He indicates that Chagnon misrepresented key dynamics of Yanomami society, particularly their level of violence. The Yanomami were not "the fierce people" depicted by Chagnon. They were significantly less bellicose, in fact, than many Amazonian groups. (2) What warfare Chagnon noticed during his research, Tierney asserts, Chagnon himself helped cause through his enormous distribution of goods, which stimulated warfare among the Yanomami as perhaps never before. (3) Tierney accuses Chagnon of staging the films he helped produce, films that won many cinematic awards and helped make *Yanomamö: The Fierce People* a best seller. The films were not what they appeared to be—live behavior skillfully caught by the camera—but rather staged productions in which Yanomami fol-

lowed preestablished scripts. (4) Tierney accuses Chagnon of fabricating the data used in Chagnon's most famous article, which appeared in *Science* in 1988. The article asserted that Yanomami men who murdered tended to have more wives and more children—or, phrased another way, that violence was an evolutionary adaptive principle (5) Tierney asserts that Chagnon acted unethically in collecting the genealogies needed for Chagnon's and Neel's research. The Yanomami have a taboo against naming deceased relatives. When asked about deceased relatives, Yanomami would invent names, essentially making a shambles of Chagnon's genealogical data. Tierney claims that Chagnon used unethical techniques to get around this difficulty. (6) Tierney asserts that Chagnon's self-depiction as being the first outsider to make contact with several Yanomami villages is untrue. Long before Chagnon arrived, Helena Valero, an outsider who was kidnapped by the Yanomami in 1932 and who lived among them for fifty years, had visited all the villages Chagnon claimed to have contacted. And (7) Tierney accuses Chagnon of violating Venezuelan law while participating in a plan with two prominent Venezuelans to establish a private Yanomami reserve that would have been controlled by the three of them. This is termed the FUNDAFACI (Foundation to Aid Peasant and Indigenous Families) project. For Chagnon, the project represented a way around the restrictions placed on his visiting the Yanomami by the Venezuelan government.

The publicity generated by Tierney's *Darkness in El Dorado* became part of the controversy. Here is a sampling of what the media said. *ABC News.com* reported: "Another red-hot scientific scandal. This time anthropologists and geneticists are getting a noisy wake-up call. A book written by journalist Patrick Tierney, titled *Darkness in El Dorado*, . . . raises a stink so high that the space station astronauts will get a whiff of it" (Regush 2000). *Time* asked: "What Have We Done to Them? . . . A new book charges scientists with abusing the famous Yanomami tribe, stirring fierce debate in academia" (Roosevelt 2000). *USA Today* noted that the "face of anthropology stands riddled with charges that its practitioners engaged in genocide, criminality and scientific misconduct" (Vergano 2000). *Business Week* added: "Tierney makes a persuasive argument that anthropologists for several decades engaged in unethical practices" (Smith 2000). The *New Yorker* spread across its cover: "What happened in the jungle? Patrick Tierney reports from South America on the anthropologist who may have gone too far" (October 9, 2000: cover overleaf).

How did anthropologists respond to the media reports? The *New York Times* wrote: A "new book about anthropologists . . . has set off a storm in the profession, reviving scholarly animosities, endangering personal reputations and, some parties say, threatening to undermine confidence in legitimate practices of anthropology" (Wilford and Romero 2000). The *Chronicle of Higher Education* reported: "Some anthropologists fear that their discipline faces a scandal because of the imminent publication of a book charging several prominent researchers with egregious misbehavior in their work with Amazon tribes. . . . Scholars are worried that the allegations will make it hard for all cultural anthropologists who

do fieldwork to persuade their subjects and the public that they are responsible, objective, and trustworthy” (Miller 2000b).

As time went on, other accusations were piled on top of the ones listed above.

Regarding Neel, there were two. First, critics suggested that he had never gotten informed consent for his medical research among the Yanomami. (Informed consent, touched on above, involves getting formal permission from subjects to conduct research on them and is required today in all medical research.) Even if standards of informed consent during the 1960s differed from those existing today, several critics asked if Neel couldn't have done more to inform the Yanomami about the details of his research. This constitutes a critical issue because many Yanomami today claim that they had been led to expect additional medical assistance that drew on the results of Neel's research among them. This assistance has not been forthcoming. Second, with the publication of Tierney's book many Yanomami came to realize that the blood collected during Neel's research was still being preserved in American laboratories. They felt they had never been informed that this would occur. While some Yanomami want to be suitably paid for their deceased relatives' blood, others want it destroyed, viewing it as a sacrilege to preserve the blood of dead Yanomami. What the Yanomami concur on is that they want to reopen negotiations regarding the blood and are willing to contest continued use of it until a suitable agreement is reached.

Regarding Chagnon, three accusations came to the fore. First, various anthropologists in Brazil and the United States brought up the old question of why Chagnon had never openly opposed misuse of his work in the Brazilian press. It seemed a violation of the American Anthropological Association's ethical injunction to do no harm. Second, some anthropologists brought up Chagnon's earlier criticism of Davi Kopenawa, a prominent Yanomami activist who played a key role in the effort to establish a Yanomami reserve in Brazil. They asked if it was right that an anthropologist should undermine the work of an indigenous activist seeking to protect his people. And third, there was the question of how Chagnon should distribute the more than \$1 million he made in royalties from his best-selling book *Yanomamö*. Chagnon at one time had set up a fund to assist the Yanomami, but there is no record of the fund ever doing anything to help them. Many asked, shouldn't Chagnon share some of this money with the Yanomami who assisted in the research? Clearly, Chagnon could not have written the book without their help.

As the controversy continued, Tierney was subjected to criticism as well. Several supporters of Neel and Chagnon suggested that *Darkness in El Dorado* was full of inaccuracies. They described many of the footnotes used to back up statements in the main text as distortions of the original sources. Some critics suggested Tierney's book was little more than a malicious, irresponsible attack on two prominent scientists.

With all the attention focused on the Yanomami controversy, we might ask whether the Yanomami have benefited in some way from the controversy that

has swirled around them. To date, the answer is essentially no. Despite all the publicity and all the good intentions expressed by anthropological organizations and anthropologists, the Yanomami essentially still live under the same tenuous health conditions as before. This is a scandal in itself. It suggests that the Yanomami seem, for many anthropologists, to be primarily tools for intellectual argument and academic advancement.

### American Anthropology's Response

One might think these issues quite sufficient to create debate in anthropology departments around the world. But there is more. There are also important questions regarding the way American anthropology has responded to the controversy.

For example, why did no American organization ever investigate the accusations surrounding Chagnon before the publication of Tierney's *Darkness in El Dorado* in 2000, although the accusations had been circulating for years and were supported, in part, by Chagnon's own writings. Rather than investigating these accusations, most members of the discipline seemed content to ignore them. In fact, thousands of anthropologists continued to use Chagnon's ethnography *Yanomamö* in their classes, even though it was clear that the field practices he described in it violated the American Anthropological Association's code of ethics. Whatever Chagnon's ethical lapses, he remained a hero to many in the discipline. We might ask why so many chose to ignore, rather than investigate, the accusations against him.

We might also voice concern over the way the American Anthropological Association (AAA), American anthropology's largest organization, initially responded to the publicity generated by the publication of Tierney's book. The AAA organized an "open forum" with a number of panelists at its 2000 annual meeting. But as readers will see in chapter 3, most of the panelists were biased against Tierney. In criticizing him, they focused on Tierney's accusation against Neel that had already been disproved. Tierney's accusations against Chagnon were not really addressed.

Readers will have a chance to evaluate for themselves where they stand on this issue. But my impression—if I may inject it at this point—is that the leaders of the American Anthropological Association initially addressed the controversy more as a problem in public relations than as a problem of professional ethics: they were more concerned with protecting the discipline's image than with dealing directly with the issues Tierney had raised.

To its credit, the association set up a task force to inquire further into the matter. But when the El Dorado Task Force's preliminary report was made public, it appeared to be following the same tack as the panelists at the open forum. The preliminary report caused an uproar among those who wanted to call Chagnon to account. In an effort to calm the troubled waters generated by the report, the Task Force requested public comment on it. The more than 170 responses posted

at the association's Web site—119 of them from students—caused the Task Force to change course. The comments drew the Task Force into seriously assessing, in its final report, Chagnon's various deeds and misdeeds. It was the first time the association had seriously done so.

Whatever one's view of the Task Force's final report—and opinions differ—it is important to acknowledge the role students played in this phase of the controversy. Never before in the discipline's history, I believe, had students participated with such impact in such a prominent disciplinary debate. That participation is the reason I am dedicating this book to these 119 students. At a critical time, they stood up, got involved, and made a difference in the discipline's politics.

To summarize, the controversy is not simply about the accusations Tierney made against Neel and Chagnon or the accusations various other people have made against Neel, Chagnon, and Tierney. It is also about how American anthropology has responded to these accusations. There is room for cynicism regarding how the controversy has played out in the discipline. But there is also room for hope, given how students helped draw the association's Task Force into directly assessing accusations against a former member.

### The Larger Questions

At a still higher level, beyond the accusations and counteraccusations and beyond American anthropology's responses to them, there is yet another set of issues anthropologists and anthropologists-in-the-making need to confront regarding the controversy. These are the generally unspoken questions that lie at the heart of the discipline and that help to explain why American anthropology has been hesitant to confront the controversy head-on. These are the big questions we need to ask but often are afraid to because they put into doubt what we have come to accept as foundational and firm in anthropology.

The first is the *inequality of power* between anthropologists and those whom they study. Since anthropologists tend to come from countries that are more economically developed and militarily powerful than those they study, it is reasonable to ask, what ethical standards should govern how the more powerful use the intellectual and biological resources of the less powerful? Phrased another way, how does anthropology move beyond colonial practices built up when anthropologists mostly studied the subjugated peoples of imperial powers? What today constitutes a fair and just relationship among the parties concerned? Related to the inequality of power are the issues of informed consent, "doing no harm," and just compensation.

Today the first of these, *informed consent*, is required by almost all funding agencies supporting medical and social research. But how do anthropologists acquire permission from the people being studied? How does one explain a project to a group of people (or inform them) and gain their approval (or consent)

when the project involves unfamiliar concepts and practices? Also relevant is the question of the duration of such consent. Is it a one-time thing, or do researchers need to gain it again as they find new ways to use and make money from the initial research that was never envisioned in the initial consent agreement?

The second is the anthropological injunction (embodied in the American Anthropological Association's code of ethics) *to do no harm* to those whom anthropologists study. What this means in practice—what specific actions this directive commits an anthropologist to—remains unclear. Remember that Chagnon, who essentially admitted in his own writings to violating this ethic, was lionized by many within the discipline.

We might, moreover, wonder why the focus is on doing no harm rather than on the third issue, offering *just compensation* to those who assisted in one's research. Anthropologists tend to present generous gifts to informants. But are such gifts sufficient compensation, given that anthropologists take the informants' information back to their universities and use it to build financially satisfying careers that often far exceed what their informants can expect in their own lives? Should these informants, who are living in less-privileged circumstances, be given the assistance to create better lives for themselves as well?

There are no easy answers here, and readers should not expect anthropology, by itself, to right the world's inequities. But these issues should be openly addressed. We need to consider how anthropology as a discipline might reach across the political and economic divides that separate researchers from informants and justly compensate those who help anthropologists build professional careers.

Most anthropologists care deeply about the people they work with. But they get caught up in broader power structures that keep the discipline from moving beyond the colonizing practices of times past. The persistence of such practices today is also a part of the Yanomami controversy.

This point leads to another, the issue of *professional integrity*. Is the American Anthropological Association's code of ethics simply a set of nice-sounding abstractions—window dressing to impress those beyond the discipline—or are anthropologists held accountable to the code in some way? What responsibilities does the code entail for individual anthropologists? What does it entail for the discipline as an organized profession? Some might prefer to deal with such questions in terms of abstract pronouncements (of *shoulds* and *should nots*), but the fact is that anthropologists cannot simply claim to be moral and expect others in nonacademic settings to trust them on that basis, especially given the discipline's record to date. Again, there are no easy answers. But we all need consider how to move anthropology beyond talking about morality to practicing a morality that embodies the best ideals of the discipline and that ensures a positive reception for us in places where our reputations precede us.

We need to also consider *the way anthropologists tend to argue past one another* in controversies such as this. Is anthropology simply a matter of vexation and debate—a form of entertainment for intellectual aficionados of the obscure—or is something approaching a consensus possible in a heated matter where the

discipline's own behaviors are called into question? Are controversies such as this ever resolvable? Or do people simply give up arguing after a while and go on to something new?

For anthropology, Chagnon is the central character. The discipline embraced him and his work for years, making *Yanomamö* the best-selling ethnography in the past half-century. Understandably, partisans of Chagnon—and there are many in the discipline—tend to focus their criticism of Tierney on his account of Neel, reasoning that if Tierney's case is weakened in one area it is weakened in others. That is why the "Referendum on Darkness in El Dorado" (sponsored by Chagnon partisans and passed in October-November 2003 by the American Anthropological Association) focused on Tierney's fallacious claim that Neel helped make the measles epidemic worse. While Chagnon was a participant in Neel's project, he played a minor role in Neel's measles immunization campaign. Chagnon partisans downplay his violations of the association's ethical code and Venezuelan law. Partisans of Tierney, on the other hand, tend to pass over the charges against Neel and focus on Tierney's accusations against Chagnon, where they feel their case is stronger. One can often tell a person's position in the controversy simply by noting the topic he or she wishes to discuss.

As a result of these tactics, there have been few sustained, back-and-forth discussions between opposing partisans regarding the accusations surrounding Neel and Chagnon. Most of the time opposing partisans talk past one another. The only two sustained conversations I know of are in part 2 of this book and the final report of the AAA's El Dorado Task Force, which is summarized in chapter 11.

In summary, beyond the accusations surrounding Neel, Chagnon, and Tierney, there are critical—indeed, from my perspective, far more critical—issues that need to be addressed in the controversy: those involving relations with informants as well as professional integrity and competence. Given how central these issues are to anthropology, readers can understand, perhaps, why many in the discipline have sought to sidestep the controversy. Confronting these issues will be hard. But the discipline needs to address them if it is to outgrow its image as an agent of colonizing powers and be both welcomed and understood outside the halls of academia.

#### **WHAT IS RIGHT ABOUT CONTROVERSIES SUCH AS THIS?**

I have referred above to the problems controversies such as this can create. They may generate negative publicity for the discipline, making the broader public less willing to support it. They may also foster disciplinary divides as anthropologists passionately argue past one another without resolution. Let me turn now to what is right about these controversies and why they are important, indeed essential, for the discipline's cumulative development.

First, controversies such as this provide a basis for conversations across the

specialized research worlds anthropologists now participate in. They enable people grounded in different regions and absorbed by different problems to talk about issues that interest—and in this case affect—them all. In Victor Turner's phrasing, controversies such as this offer a temporary "communitas," a temporary moment of community that transcends the structural boundaries that traditionally separate anthropologists from one another. Turner suggests that such "antistructural" moments allow people to perceive the problematic nature of the structures that shape their everyday lives. We see that here. The Yanomami controversy allows us to reflect on the discipline's dynamics in a special way.

Second, controversies such as this are essential for building a cumulative discipline. There has been a sea change in the way anthropologists think about their research since Napoleon Chagnon began his Yanomamö fieldwork in 1964. At that time, there was a general disciplinary sense that anthropologists—in seeking to be scientific—were concerned with "just the facts," as Detective Joe Friday famously put it in the 1950s television program *Dragnet*. Anthropologists saw their job as collecting facts and letting the facts speak for themselves.

Today, there is a greater appreciation that gathering "just the facts" is not a simple process. During the past two decades, the discipline has worked its way through what has been termed "a crisis of representation," an "uncertainty about adequate means of describing social reality" (Marcus and Fischer 1986:8). "No longer is it credible," Fischer asserts, "for a single author to pose as an omniscient source on complex cultural settings" (in Barfield 1997:370). While this perspective has been warmly embraced by a substantial portion of the discipline, it has mostly involved—at the case-study level—authors challenging their own authority in ways that, at times, might be perceived as self-serving.

In examining opposing viewpoints as we do in this controversy, readers have a chance to move beyond such accounts to a deeper, fuller sense of how anthropologists, in fact, construct ethnographies. There are, no doubt, self-serving elements in Chagnon's and Tierney's accounts. But we can ferret many of these out by comparing one account with another and comparing both with other accounts written by different anthropologists who have also worked among the Yanomami.

What is now increasingly evident to most members of the profession—and perhaps should have been in the 1960s—is that anthropology needs different accounts of the same subject to gain greater objectivity, to gain a better sense of the social processes described by anthropologists. Multiple accounts allow us to step behind the screen of anthropological authority—something like seeing the Wizard of Oz in person rather than from behind a screen—and perceive the underlying dynamics at work.

In the search for objectivity, we cannot put our faith in a single account, regardless of the status of the person who produced it. There is always the problem of self-serving rhetoric. *Objectivity does not lie in the assertions of authorities. It lies in the open, public analysis of divergent perspectives.*

What is essential to developing cumulative knowledge—rather than continually increasing the amount of uncertain knowledge, as frequently occurs



today—is that anthropological results be publicly called into question. The results must be challenged, the researchers involved must respond, and the broader community must work its way toward consensus on the issue. The problem, of course, is that as long as the material remains obscure—known only by this or that expert—there can never be a real collective resolution of differences.

The hope held out, in chapter 6 and part 2 of this book, is that we can collectively listen to the arguments and counterarguments of experts as they debate. And as in a trial where the jury does not know all the relevant details beforehand but learns them as various experts with opposing views present them, we can come to a set of shared conclusions.

### HOW THIS BOOK IS ORGANIZED TO BRING DISCIPLINARY CHANGE?

The book is organized into two parts. Part 1, chapters 1 through 7, elaborates on themes discussed above. Critically, it offers readers a chance to decide where they themselves stand on the issues raised by the controversy.

Chapter 2 introduces readers to the controversy's specifics by highlighting some key statements by Chagnon and Tierney. It uses the various editions of Chagnon's famous *Yanomamö* to better understand Chagnon and why he sought to repeatedly return to work among the Yanomami. It also highlights, with direct quotes, Tierney's precise accusations against Neel and Chagnon.

Chapter 3 shows how the controversy unfolded within American anthropology. It elaborates on the concerns regarding Chagnon's behavior before the publication of *Darkness in El Dorado*, the reasons American anthropology so widely embraced Chagnon and his work despite Chagnon's obvious ethical problems, and the ways in which the American Anthropological Association responded to the controversy over time. The chapter lets us see American anthropology in a new light.

Chapter 4 discusses two questions at the heart of the controversy and of the discipline. First, it considers the "do no harm" ethical standard for research and the power relations implicit in it. It goes on to suggest that just compensation is a better standard for negotiating relationships in the field. Second, the chapter examines how anthropologists seek to credentialize statements—how they seek to make their assertions seem true—and the flaws in the methods used.

Chapter 5 presents a sampling of Yanomami views regarding the controversy. Understandably, the concerns of the Yanomami interviewed are not necessarily the concerns of Western readers. The two perspectives are entwined in interesting ways.

Chapter 6 sets out key questions regarding the controversy that readers need to consider, need to answer for themselves. Extensive quotations from part 2 of the book illustrate opposing perspectives and provide the background readers require to reach their own conclusions. It is the central chapter in the book.

Chapter 7 draws key themes of the controversy together, asking readers to help foster the ethical discipline most anthropologists assert they want. It builds on the model of “student power” discussed in chapter 3.

Part 2, chapters 8 through 11, presents a detailed debate among six leading experts. Rather than having the experts present their opinions and leave it at that, the experts engage with each other through three rounds of argument and counterargument. Part 2 constitutes the fullest, most open discussion of the controversy’s central concerns to date.

In chapter 8 the six experts offer their positions on the central ethical issues raised by Patrick Tierney’s *Darkness in El Dorado* and the best manner for dealing with them. In chapter 9 the experts comment on one another’s positions (as set out in chapter 8). Chapter 10 concludes the discussion by having each expert comment one final time on the other participants’ perspectives (especially as presented in chapter 9). The arguments and counterarguments of the six experts, as they unfold through the three rounds, allow readers to make sense of the issues more effectively than if there were only one expert enunciating his or her views. Readers are better able to weigh one position against another.

Chapter 11 concludes part 2 with two assessments of the controversy. The first involves a joint letter written by the six experts in the part 2 discussion plus myself. It offers our points of agreement regarding issues central to the controversy. The chapter also includes a summary of the final report of the American Anthropological Association’s Task Force on the controversy as well as a description of the Task Force’s preliminary report and a sampling of comments made about it. The chapter concludes by asking you, the reader, for your personal assessment of the controversy.

An appendix summarizes who affirms what about which topics in chapters 8, 9, and 10. Readers can use the summary as a guide for exploring a particular participant’s position or a particular issue in the controversy.

Behind this formal organization is another organization that is meant to draw readers into not only reflecting on key disciplinary issues but addressing them directly in a way that fosters change. Archibald MacLeish wrote, in “Ars Poetica,” that “a poem should not mean but be.” That is what this latter organization strives for. The book is conceptualized and structured to draw readers into a disciplinary activism that can help shape anthropology’s development over the coming decade.

First, the book seeks to enlarge the public sphere of discussion. As noted, experts frequently argue past one another, leaving the rest of the discipline as passive observers, trying to make sense as best they can of what is going on. Chapter 6 sets out the information readers need to draw their own conclusions. If readers wish to explore particular subjects further, they need only turn to part 2, where experts on both sides present their arguments and counterarguments vis-à-vis one another. The model, as noted, is of a jury trial where ordinary citizens listen to conflicting arguments and gain enough information to reach a consensus with their peers on an issue. The goal is to draw more people—both stu-

dents and professors—into discussing the controversy's central questions. The issues raised by the controversy are too important to leave to a few experts. They involve us all. We should, therefore, all participate in the deliberations regarding them.

Second, the book seeks, in empowering readers, to develop a new political constituency for transforming the discipline. It is understandable that many anthropologists have had trouble addressing the controversy's central issues because they are invested in the present system. These anthropologists worked their way through the discipline's existing structures as they progressed from being graduate students to employed professionals. While they may acknowledge the limitations of the discipline, these structures represent the world they know, the world they feel comfortable with. One would not expect most of them to lead the charge for change. But introductory and advanced students are less invested in this system. If anything, they have a stake in changing it so as to create new spaces for themselves. Chapter 7 gives them the tools to foster this change.

Readers might wonder how this suggested activism will ultimately affect the people anthropologists study. In terms of specific changes in the discipline as a whole, that remains to be seen. But *all* the royalties from this book will go to helping the Yanomami. Neither the political projects presented in chapter 7 nor the royalties from this book are the final word on helping those who help us in our research. But they do represent a start in nourishing the change many want and hope for in the discipline. There is possibility in the air.