
**HELPING THE YANOMAMI**

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Here is a small but significant way to help the Yanomami that extends beyond good intentions. Your commitment to provide the Yanomami with royalties, combined with similar commitments from others, means that the Yanomami will to some degree benefit from the controversy that has swirled around them and disrupted their lives.
The area where the Yanomami live in southern Venezuela and northern Brazil with the names and locations of the most prominent Yanomami subgroups. (This map is drawn from Roberto Lizaralde’s *Grupos Lingüísticos Yanomami*, prepared for the Venezuelan government’s census.)
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 groups 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 Yanomamó 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 Yanomamó 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 Yanomamó 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 Yanomamó 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 Yanomamó 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 Tierney's _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 Yanomamo: 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 Yanomamo, 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 recent 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 violations 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’” (1992b: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.
Several anthropologists 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 El 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—ininvolved 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. ABCnews.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 an 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 *Yanomamó* 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 the controversy’s issues. 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—most 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 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 their informants. 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 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 Yanomamó 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 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 Yanomamo 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.

ORGANIZING THIS BOOK
FOR 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 certain key statements by Chagnon and Tierney. It uses the various editions of Chagnon's famous Yanomamō 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 way.

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

CHAGNON'S DESCRIPTION OF HIS FIRST DAY OF FIELDWORK

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

My first day in the field illustrated to me what my teachers meant when they spoke of “culture shock.” I had traveled in a small, aluminum rowboat propelled by a large outboard motor for two and a half days. This took me from the Territorial capital, a small town on the Orinoco River, deep into Yanomamó country. . . .

We arrived at the village, Bisaasi-teri, about 2:00 PM and docked the boat along the muddy bank. . . . It was hot and muggy, and my clothing was soaked with perspiration. It clung uncomfortably to my body, as it did thereafter for the remainder of the work. The small, biting gnats were out in astronomical numbers, for it was the beginning of the dry season. My face and hands were swollen from the venom of their numerous stings. . . .

The entrance to the village was covered over with bush and dry palm leaves. We pushed them aside to expose the low opening to the village. The excitement of meeting my first Indians was almost unbearable as I duck-waddled through the low passage into the village clearing.

I looked up and gasped when I saw a dozen burly, naked, filthy, hideous men staring at us down the shafts of their drawn arrows! Immense wads of green tobacco were stuck between their lower teeth and lips making them look even more hideous, and strands of dark-green slime dripped or hung from their noses. We arrived at the village while the men were blowing a hallucinogenic drug up their noses. One of the side effects of the drug is a runny nose. The mucus is always saturated with the green powder and the Indians usually let it run freely from their nostrils. My next discovery was that there were a dozen or so vicious dogs snapping
at my legs, circling me as if I were going to be their next meal. I just stood there holding my notebook, helpless and pathetic. Then the stench of the decaying vegetation and filth struck me and I almost got sick. I was horrified. What sort of a welcome was this for the person who came here to live with you and learn your way of life, to become friends with you? . . .

We arrived just after a serious fight. Seven women had been abducted the day before by a neighboring group, and the local men and their guests had just that morning recovered five of them in a brutal club fight that nearly ended in a shooting war. The abductors, angry because they lost five of the seven captives, vowed to raid the Bisaasi-teri. When we arrived and entered the village unexpectedly, the Indians feared that we were the raiders. On several occasions during the next two hours the men in the village jumped to their feet, armed themselves, and waited nervously for the noise outside the village to be identified . . .

I pondered the wisdom of having decided to spend a year and a half with this tribe before I had even seen what they were like. I am not ashamed to admit, either, that had there been a diplomatic way out, I would have ended my fieldwork then and there. I did not look forward to the next day when I would be left alone with the Indians; I did not speak a word of their language and they were decidedly different from what I had imagined them to be. The whole situation was depressing, and I wondered why I ever decided to switch from civil engineering to anthropology. (1968:4–6)

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

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

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

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

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

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

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

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

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

A third theme is Chagnon’s concern with maintaining the Yanomamö’s popularity with student audiences. As he noted in the second edition, “I decided [in writing the book] that I would let my own experiences as a student be my guide as an author, for I wanted to communicate with students of anthropology as with my professional colleagues. I remember ... how much I enjoyed reading monographs that were sprinkled with real people, that described real events, and that had some sweat and tears, some smells and sentiments mingled with the words” (1977:xi). True to his word, in the third edition he added a case study entitled “The Killing of Ruwâhiwâ.” The fifth edition added another case study, “Alliance with the Mishimishimaböwei-teri.” (Chagnon notes that he added the latter case study partly to fit with a new interactive CD-
ROM he helped produce on the topic. By 1997 such interactive materials were becoming increasingly popular in classes and a major selling point for texts.) Clearly critical to the publishing success of Chagnon's *Yanomamö* are a number of vivid ethnographic films relating to his fieldwork. One, *The Feast*, won first prize in every film competition in which it was entered. The films convey a realistic sense of Yanomamö life, building upon and, in turn, enhancing points Chagnon develops in his text. (The second and later editions list these films in case readers wish to view them.)

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

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

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

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

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

(1992a:233–34)

Chagnon concludes the chapter with a position statement:

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

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

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

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

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

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

PATRICK TIERNEY

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

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

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

Yanomami at the Ocamo mission received the Edmonston B [vaccine] without the recommended gamma globulin coverage [meant to lessen adverse reaction to the shot], which doubled the risk of reaction [to the vaccine]. . . .

There was no doubt . . . that a full measles rash and fevers first appeared among the Ocamo Yanomami within a week of the Indians' vaccination. Prior to the Yanomami's severe vaccine reactions . . . no one had seen the disease's telltale lesions. (2000:60, 67)

Chagnon and Neel described an effort to "get ahead" of the measles epidemic by vaccinating a ring around it. As I have reconstructed it, the 1968 outbreak had a single trunk, starting at the Ocamo mission and moving up the Orinoco with the vaccinators. (69)

Clearly he [Neel] and his doctors distributed medicine and cared for some of the sick they encountered. But his choice of vaccine [the Edmonston B] suggested he wanted new data [on genetic questions of selective adaptation] and his impatience with Venezuelan authorities meant that he had no backup from government doctors when crisis occurred.

Moreover, Neel barely slowed his pace of blood-collecting or filming, both of which required massive payments of trade goods, a reckless policy during an epidemic [because the infected people would, in trading goods to other villages, spread the disease]. . . . The scientists kept moving on and the epidemic moved on with them. (82)

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

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

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

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

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

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

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

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

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

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

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

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* Tierney also discusses the staging of the well-known NOVA/BBC special on the Yanomami, Warriors of the Amazon. Andy Jillings, the director of the documentary, noted in a telephone interview with Tierney: “I was looking for a group that was fairly unacclimated and that was at war and suing for peace. So Jacques [Lizot] and I went out to another group that was at war, but they were not home much of the time. I wanted an unacclimated group because you can’t make a film about the Yanomami if they’re wearing Black Sabbath T-Shirts. We spoke to the more remote group but, basically, we were hijacked because the Karo people said, ‘Why don’t you have the feast here?’ They saw all our trade goods and they didn’t want them going to the other group. The feast of reconciliation [between two warring groups] was a set-up. We might have facilitated it. But they wanted it” (2000:220). The film’s highlight was the cremation ritual of a recently deceased woman. Rather than nursing the woman back to health, the film crew recorded her dying. Mike Dawson, who had lived among the Yanomami for more than twenty-five years, told Tierney, “With a little bit of help, they [the mother and her newborn infant] could have pulled through. The film crew interfered in every aspect of their [Yanomami] lives. Let’s be real. They’re giving them machetes, cooking pots, but they can’t give a dying woman aspirin to bring her fever down?” (2000:217)
Sixth, Tierney indicates that Chagnon misrepresented his first-contact experiences: "It is a remarkable fact and a remarkable theft. Every single place... and every single village... that Chagnon has touted as his discovery, was intimately known and visited by Helena Valero [before him]" (2000:246).

Finally, Tierney asserts that Chagnon violated Venezuelan law in what came to be known as "the FUNDAFACI affair." Chagnon allied himself with Charles Brewer-Carías, an entrepreneur with a reputation for mining remote regions of Venezuela, as well as with Cecilia Matos, the mistress of the then president Pérez. The three hatched a plan to set up, under their control, a vast Yanomami "nature reserve" roughly the size of Maine (2000:9; on page 188 Tierney states the area involved was the size of Connecticut). Tierney notes that the reserve "would have given him [Chagnon] unprecedented power, but it required overthrowing the legal structure already established in Yanomami territory" (10).

Tierney suggests that by 1990, opposition to Chagnon's research among both Venezuelan academics and the Yanomami had increased, and, as a result, Chagnon was finding it ever harder to continue his periodic trips to the field. "With Matos and Brewer... as allies, Chagnon devised a... bold... plan to permanently circumvent all the institutions that controlled the Yanomami Reserve. The three... would simply create their own, private reserve, a Yanomami park. At the same time,... they began a fierce press campaign against the Salesian" missionaries who opposed the plan (186). "According to... Venezuela's assistant attorney general for indigenous affairs, the various trips by Brewer and Chagnon, which cost millions of dollars in government transportation costs, 'were illegal because there is no evidence they even submitted their plans to the DAI [Indian Agency] for approval'" (191).

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

Second, Tierney never accuses Neel or Chagnon of committing genocide. As we have seen, Tierney makes a number of serious accusations against each of
them, but he never refers to genocide. My research suggests that it was Chagnon who first brought up the accusation of genocide. In 1989 Chagnon responded to a published letter from the Brazilian anthropologist 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.
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-Carias. 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 Marquez, 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 Carias. 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 advo-
ating 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 Warm'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 Yanomamó, 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 Yanomamó’s reactions to his presence in various villages: “There was great danger, for as my personal relationship with Möawí 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 Börösowä’s village, and how Börösowä and his brothers tried to do me in while I slept. . . . And beyond this village lay Tananowä’s. . . . I turned back from the 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 Yanomamó, Chagnon writes: “My study of the Shamatari group began with threats to my life and ended that way” (1974:166).

Chagnon’s relationships with several informants, in other words, tended 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 interest [in fieldwork], an agonistic drama, resulting in mutual respect, complicity in 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 *Yanomamô* that he visited some sixty villages during his first forty-two months in the field (1983:ix). Given the difficulties he faced in traveling to and dealing with informants in a host of diverse locales, it is—in my opinion—an impressive effort. But why do it? Especially when he notes that "it takes months to establish rapport with individuals in a new group and to discover who the good informants are" (1974:94).

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

Chagnon explains his mobility in the following terms: “It became increasingly clear that each Yanomamó 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 Yanomamó. “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 airline 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 Yanomamó. In the second edition, for example, he notes: “The reciprocal and generally good-natured mischief with which the Yanomamó 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 Yanomamó: 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 Kaobawá, Rerebawá, and Dedehewá 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 Yanomamo 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 Yanomamo at their own game: "I soon learned that I had to become very much like the Yanomamo 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 Yanomamo 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 outward 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 AMBITIOUS 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 Anthro-
How the Controversy Has Played Out

How the Controversy Has Played Out

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 criticized 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.