ISSUE 17

Did Napoleon Chagnon's Research Methods and Publications Harm the Yanomami Indians?

YES: Terence Turner, from *The Yanomami and the Ethics of Anthropological Practice* (Cornell University Latin American Studies Program, 2001)

NO: Edward H. Hagen, Michael E. Price, and John Tooby, from *Preliminary Report,* `http://www.anth.ucsb.edu/ucsbpreliminaryreport.pdf` (Department of Anthropology, University of California Santa Barbara, 2001)

ISSUE SUMMARY

YES: Anthropologist Terence Turner contends that journalist Patrick Tierney's book *Darkness in El Dorado* accurately depicts how anthropologist Napoleon Chagnon's research among the Yanomami Indians caused conflict between groups and how Chagnon's portrayal of the Yanomami as extremely violent aided gold miners trying to take over Yanomami land.

NO: Anthropologists Edward Hagen, Michael Price, and John Tooby counter that Tierney systematically distorts Chagnon's views on Yanomami violence and exaggerates the amount of disruption caused by Chagnon's activities compared to those of others such as missionaries and gold miners.

In September 2000 a startling message flew around the e-mail lists of the world's anthropologists. It was a letter from Cornell University anthropologist Terry Turner and University of Hawaii anthropologist Leslie Sponsel to the president and president-elect of the American Anthropological Association (AAA), with copies to a few other officers, warning them of the imminent publication of a book that they said would "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." The book in question, which they had read in galley proofs, was Patrick Tierney's *Darkness in*

El Dorado: How Scientists and Journalists Devastated the Amazon (W.W. Norton and Company, 2000). The letter summarized some of Tierney's charges that medical researcher James Neel, anthropologist Napoleon Chagnon, and others seriously harmed the Yanomami Indians of Venezuela, even causing a measles epidemic that killed hundreds.

The leaking of the letter and the subsequent publication of an article by Tierney in *The New Yorker* caused great excitement at the annual meeting of the AAA in San Francisco in mid-November 2000. Discussion climaxed at a panel discussion that filled a double ballroom at the Hilton Hotel with several thousand anthropologists and media representatives from around the world. The panel included Tierney, anthropologist William Irons (representing Chagnon, who declined to attend), and experts on the history of science, epidemiology, and South American Indians. (Neel could not defend himself, as he had died earlier that year.) Numerous members of the audience also spoke. During and after the meeting a consensus developed that Tierney was wrong in his claim that the measles epidemic in 1968 was caused by Neel's and Chagnon's inoculations, since measles vaccines are incapable of causing the actual disease. However, Tierney's accusations that Chagnon had treated the Yanomami in an unethical manner during his research and had distorted his findings were not so easily dismissed. Later, the Executive Board of the AAA established a task force to examine the allegations in Tierney's book. The 300-page Task Force Final Report was completed on May 18, 2002 and is now posted on the AAA Web site.

Why would a book about researchers' treatment of a small Amazonian tribe have caused such an uproar? The reason is that, due to the enormous sales of Chagnon's book *The Yanomamö* (now in its fifth edition) and the prize-winning films he made with Timothy Asch, the Yanomami (as most scholars spell their name) are probably the world's best-known tribal people and Chagnon one of the most famous ethnographers since Margaret Mead. Chagnon's use of his Yanomami data to support his sociobiological explanation of human behavior has also had influence outside anthropology (for example, in evolutionary psychology). The idea that the most aggressive men win the most wives and have the most children, thus passing their aggressive genes on to future generations more abundantly than the peaceful genes of their nonaggressive brethren, is a cornerstone of the popular view that humans are innately violent (see Issue 2). Thus, Tierney's attack on Chagnon's credibility sent shock waves through the scholarly community.

In this selection, Turner argues that Tierney is correct in claiming that Chagnon failed to object when gold miners used his portrayal of the Yanomami as violent to hinder the establishment of a Yanomami reserve in Brazil, that Chagnon manipulated his demographic data to support his hypothesis that more violent Yanomami men have more wives and children than less violent ones, and that his fieldwork practices caused conflict between Yanomami groups. Hagen, Price, and Tooby counter that Tierney deliberately misrepresents evidence in claiming that Chagnon violated Yanomami taboos in his fieldwork and that he manipulated his data to support his claim that killers have more offspring than nonkillers.

NO

Edward H. Hagen, Michael E. Price, and John Tooby

Preliminary Report

Introduction

As we will begin to show in this report, *Darkness in El Dorado* is essentially a work of fiction. Its author, Patrick Tierney, has very selectively quoted hundreds of sources in order to, first, caricature anthropologist Napoleon Chagnon's work on the Yanomamö, and second, to discredit what he claims is "Chagnon's ethnographic image of the ferocious Yanomami" by instead portraying them as meek, peaceful, helpless, and, ultimately, victims of Chagnon himself. Tierney's creative use of primary sources in this venture begins almost immediately. After a brief introductory chapter, Tierney wastes little time attempting to undermine Chagnon's portrayal of Yanomamö males as relatively healthy and frequently engaged in war:

> Before going into the jungle, I had read and admired The Fierce People. So it was surprising to see that the Yanomami—so terrifying and "burly" in Chagnon's text—were, in fact, among the tiniest, scrawniest people in the world. Adults averaged four feet seven inches in height, and children had among the lowest weight-height ratios on the planet.

References are supposed to support, not refute, the claims one is making. Tierney's reference above cites a relatively short paper by Rebecca Holmes on Yanomamö health. Although the paper does confirm the widely known fact that Yanomamö are short, it does not support one of Tierney's major themes: that Chagnon has exaggerated the frequency of Yanomamö warfare. What Tierney fails to mention here or anywhere else in his book is what Holmes says in her paper about Yanomamö war:

> Raids resulting in serious wounds and death occur *several times a year* in spite of missionary pressure to restrict warfare. About 20 warriors from Parima A, a two-day walk through the jungle from Parima B, raided one of the settlements in Parima B during our fieldwork. There were no injuries, although a study of the nurse's recent medical records indicates that these raids not uncommonly result in wounds from poison arrows. (Holmes 1985, p. 249; emphasis added).

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Tierney cites Holmes' paper four times but he fails to mention her evidence on war and violence on any of these occasions, evidence which is directly relevant to one of the major themes of his book. This failure is obviously deliberate. . . .

Preliminary Evaluation of Chapter 3

Naming the Dead

Tierney, in Chapter 3 (The Napoleonic Wars) and elsewhere in his book, fingers Chagnon's method of obtaining accurate genealogies as a source of conflict between individuals and villages, and, more generally, as an affront to Yanomamö dignity (Chagnon's recent statement on this issue can be found in Appendix XIV). What we will show below is that Tierney's account is substantially undermined by the very sources he cites.

First, however, it may be useful to note that most societies, including the US, have a 'name taboo.' In the US, for example, it is not wrong to mention one person's first name or nickname to another person who does not know it, but it is often considered rude to *use* the nickname or first name of someone if you do not know them well. For example, even if Judith Smith's friends call her 'Judy', she might be offended if a stranger used that name instead of 'Judith' or Ms. Smith. How many news articles on *Darkness* have referred to 'Pat' or even 'Patrick' instead of 'Patrick Tierney' or 'Mr. Tierney'? None. In professional contexts, it is also rude to use someone's first name instead of their title and last name (e.g., Dr. Smith). In court rooms, we do not even use the judge's name, but instead address him or her as 'your honor' even though it is perfectly OK to know the judge's name, or ask someone what his or her name is. So, Americans have a rather elaborate name taboo.

The Yanomamö 'name taboo' is quite similar to the American 'name taboo.' Names are *not* 'scared [*sic*] secrets' (almost everyone knows them, in fact), but their *use* in particular social contexts is considered rude and insulting, just as, for Americans, *knowing* someone's first name or nickname is not insulting or wrong, but the *use* of nicknames and first names is rude and insulting in certain social contexts. (For the Yanomamö, the improper use of names is much more insulting than for Americans, however.) Here is Chagnon explaining the name taboo:

> The taboo is maintained even for the living, for one mark of prestige is the courtesy others show you by not using your name publicly. This is particularly true for men, who are much more competitive for status than women in this culture, and it is fascinating to watch boys grow into young men, demanding to be called either by a kinship term in public, or by a teknonymous reference such as 'brother of Himotoma' (see Glossary). The more effective they are at getting others to avoid using their names, the more public acknowledgment there is that they are of high esteem and social standing. Helena Valero, a Brazilian woman who was captured as a child by a Yanomamö raiding party, was married for many years to a Yanomamö headman before she discovered what his name was. The sanctions behind the taboo are more complex than just this, for they involve a combination of fear, respect, admiration, political deference, and honor.

The Yanomamö were understandably concerned that if the stranger in their midst (Chagnon) learned their names, he might *use* them disrespectfully. Chagnon *never* did this. Chagnon *always* addressed individuals in the proper manner, and he never intentionally used names disrespectfully (nor does Tierney present any evidence that Chagnon used names disrespectfully). Chagnon always used the Yanomamö equivalent of 'Judith' when that was appropriate, 'Ms. Smith' when that was appropriate, and 'Your Honor' when that was appropriate. Because he was struggling with a foreign culture, Chagnon occasionally but *unintentionally* offended individuals. Unlike academics, the Yanomamö are forgiving; they knew his missteps were accidental, and took no lasting offence.

Chagnon also found that it was easier to obtain a person's name from non-kin or enemies. In the US, Judith Smith's friends might be reluctant to reveal Judith's nickname to a stranger—not because *knowing* the nickname is taboo, but because its improper *use* might offend their friend—but people who were not close friends of Judith's would feel no such reluctance, nor would they violate any taboo by revealing the nickname. The same applies to the Yanomamö—asking non-kin and enemies about names is *not* taboo (remember, these names are widely known, and there is no taboo against outsiders knowing these names).

Contrary to Tierney's claims, Chagnon did *not* play enemies or villages off one another to obtain names. Notice that in Tierney's account of Chagnon's method, these claims have no supporting citations:

> Chagnon found himself in a difficult predicament, having to collect genealogical trees going back several generations. This was frustrating for him because the Yanomami do not speak personal names out loud. And the names of the dead are the most taboo subject in their culture.
>
> "To name the dead, among the Yanomami, is a grave insult, a motive of division, fights, and wars," wrote the Salesian Juan Finkers, who has lived among the Yanomami villages on the Mavaca River for twenty-five years.
>
> Chagnon found out that the Yanomami "were unable to understand why a complete stranger should want to possess such knowledge [of personal names] unless it were for harmful magical purposes." So Chagnon had to parcel out "gifts" in exchange for these names. [Anthropologists have 'to parcel out gifts' for most interviews with most informants on most topics. Giving gifts in exchange for extensive genealogical information is common practice in anthropology] One Yanomami man threatened to kill Chagnon when he mentioned a relative who had recently died. Others lied to him and set him back five months with phony genealogies [both these events are discussed in detail by Chagnon]. But he kept doggedly pursuing his goal.
>
> Finally, he invented a system, as ingenious as it was divisive [no citation], to get around the name taboo [Chagnon was not trying to 'get around the name taboo,' a claim that makes no sense ('getting around the name taboo' would entail *using* names disrespectfully—something he never did, nor had any desire to do). Chagnon was trying, not only get information necessary to his research, but also to integrate himself into Yanomamö society by learning what was common knowledge: everyone's name, including those of ancestors]. Within groups, he sought out "informants who might be considered 'aberrant' or 'abnormal,' outcasts in their own society," people he could bribe and

> isolate more easily. These pariahs resented other members of society, so they more willingly betrayed sacred secrets [names are not 'sacred secrets'—they are public knowledge] at others' expense and for their own profit. [son-in-laws doing bride service—who are therefore not living with their kin—are a common example of what Tierney terms 'pariahs'] He resorted to "tactics such as 'bribing' children when their elders were not around, or capitalizing on animosities between individuals." [using children as informants is, again, common practice among anthropologists—usually because they have the patience for the all the tedious questions that anthropologists ask]
>
> Chagnon was most successful at gathering data, however, when he started playing one village off against another. "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 my base camp and check with local informants the accuracy of the new information. 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." [see below for the material that Tierney has omitted from this quote]
>
> When one group became angry on hearing that Chagnon had gotten their names, he covered for his real informants but gave the name of another village nearby as the source of betrayal [no citation]. It showed the kind of dilemmas Chagnon's work posed. In spite of the ugly scenes he both witnessed and created, Chagnon concluded, "There is, in fact, no better way to get an accurate, reliable start on genealogy than to collect it from the enemies."
>
> His divide-and-conquer information gathering exacerbated individual animosities [no citation], sparking mutual accusations of betrayal [no citation]. Nevertheless, Chagnon had become a prized political asset of the group with whom he was living, the Bisaasi-teri.

As usual, Tierney deliberately omits critical evidence that readers need to fairly evaluate his accusations and insinuations. With the exception of the quote from the Salesian missionary Juan Finkers, all of the cited information in the above quote comes from Chagnon's publications.

Tierney also conveniently fails to mention that Kaobawa, a Yanomamö headman, *demanded* that Chagnon learn the truth, even though he knew that would involve Chagnon learning the names of his dead kinsmen:

> [Kaobawa's] knowledge of details was almost encyclopedic, his memory almost photographic. More than that, he was enthusiastic about making sure I learned the truth, and he encouraged me, indeed, *demanded* that I learn all details I might otherwise have ignored. . . . With the information provided by Kaobawa, and Rerebawa [another informant], I made enormous gains in understanding village interrelationships based on common ancestors and political histories and became lifelong friends with both. And both men knew that I had to learn about his recently deceased kin from the other one. It was one of those quiet understandings we all had but none of us could mention.

This information is in Chagnon's popular monograph, *Yanomamö* (which Tierney cites numerous times).

When Chagnon began his fieldwork with a Yanomamö village in the sixties, the Yanomamö did not know why Chagnon wanted to know their names, and were understandably quite reluctant to reveal this information to an outsider who might use it disrespectfully. Chagnon recounts the humorous and ingenious tactics the villagers used to deceive him about their real names during his initial stint in the field, and his own equally ingenious method of penetrating this deception by getting the information from other Yanomamö in enemy villages (see Appendix XIII for the monograph excerpt). Indeed, this is one of the major flaws in Tierney's account: he conveniently fails to mention that the methods that Chagnon discusses are those he used during the first six months or so of his fieldwork, before the Yanomamö had come to trust that Chagnon was not going to use the information disrespectfully. That Chagnon made strenuous attempts to avoid offending anyone while collecting names is clear from sentences that immediately follow those Tierney chooses to cite (material in bold not cited by Tierney):

> 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 my base camp and check with local informants the accuracy of the new information. 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. **For this kind of checking I had to use informants whose genealogies I knew rather well: they had to be distantly enough related to the dead person that they would not go into a rage when I mentioned the name, but not so remotely related that they would be uncertain of the accuracy of the information. Thus, I had to make a list of names that I dared not use in the presence of each and every informant. Despite the 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. This always terminated the day's work with that informant, for he would be too touchy to continue any further, and I would be reluctant to take a chance on a accidentally discovering another dead kinsman so soon after the first.**
>
> **These were always unpleasant experiences, and occasionally dangerous ones, depending on the temperament of the informant.**

Chagnon stresses his efforts to avoid mentioning the names of the dead to close kin in all five editions of his monograph, yet Tierney *deliberately* fails to mention this. . . .

However history may judge Chagnon's method of obtaining accurate genealogies (Native North Americans rely heavily on accurate genealogies in laying claim to valuable government benefits, etc.) it is important to properly represent what he did. Tierney instead deliberately omits key evidence that would allow the reader to evaluate his claims and improperly characterizes names as "sacred secrets" of the Yanomamö as a group; instead, their public *use* reflects the status and respect accorded to particular individuals. Using the same sources cited by Tierney, it is clear that Chagnon never used names disrespectfully, and soon came to be trusted on this matter by the Yanomamö. . . .

Detailed Evaluation of Chapter 10: To Murder and to Multiply

Brief Introduction

Chapter 10 of *Darkness in El Dorado* by Patrick Tierney is an extended attack on a well-known 1988 paper published by Chagnon in *Science* entitled "Life Histories, Blood Revenge, and Warfare in a Tribal Population." In this paper, Chagnon argues that warfare among the Yanomamö is characterized by blood revenge: an attack on one group by another prompts a retaliatory attack, which itself prompts retaliation, *ad infinitum*. In other words, Yanomamö war is quite similar to the patterns of conflict we see in the Balkans, the Middle East, Africa—anywhere ethnic groups come into armed conflict. In order to understand this pattern among the Yanomamö (and thus, perhaps, everywhere else), Chagnon presents data which suggest that successful Yanomamö warriors (unokai—men who have killed) are rewarded for their bravery and success. Among the Yanomamö, these rewards take the form of wives. Chagnon showed that unokai have more wives, and consequently more offspring, than non-unokai. Chagnon argued that if, over evolutionary time, cultural success lead [sic] to reproductive success, individuals would be selected to strive for cultural success. He further argued that cultural success is often achieved by engaging in successful military actions against enemies. Perhaps, then, the cycles of violence suffered by countless groups worldwide are driven, in part, by men who seek status and prestige by successfully attacking enemies.

This entire thesis has been assailed by Chagnon's critics, and Tierney hopes to bury it by demonstrating that Chagnon's research was shoddy, dishonest, and contradicted by other studies. In fact, whether or not Chagnon's theory is correct, *many* studies have demonstrated that, in small-scale societies, cultural success does lead to reproductive success, that cultural success is frequently associated with military success, and conflicts are often caused by conflicts over women. Tierney reviews almost none of these studies, and when he does, he omits key evidence that supports Chagnon's thesis.

Before we begin our analysis of Tierney's efforts in this chapter, we note that people often misconstrue Chagnon's work to mean that the Yanomamö are exceptionally violent, unlike other groups. Nothing could be further from the truth. In fact, we now know that most non-state societies have (or had) high rates of violence compared to state societies. Chagnon was one of the first to document in detail the profound impact of intergroup violence on a non-state society. . . .

Chagnon has also famously claimed that Yanomamö wars often start with conflicts over women. Tierney implies or states several times that this is either unimportant, "secondary," or a fabrication of Chagnon's. For example:

> Yet the popular image of the Yanomami waging war for women persisted. Chagnon deftly *created it* by repeatedly claiming that men went on raids, captured women, and raped them at will afterward.

If Chagnon had created this image, then there should be no independent reports of Yanomamö raiding for women, and there should especially be no

such reports predating Chagnon's. There are, however, many accounts of Yanomamö raiding for women that predate Chagnon's, accounts that place more emphasis on wife-capture than Chagnon does (Chagnon has stated several times that it is often not the principle [sic] motivation for a raid). . . .

Selective Omission of Data Which Support Chagnon's Findings

Claim Tierney argues against Chagnon's claim that warriorship and reproductive success are correlated in tribal societies, citing a study of the Waorani:

> Among the Waorani of the Ecuadorian Amazon, a tribe with the world's highest known rate of attrition of war, every known male has killed at least once. But warriors who killed more than twice were more than twice as likely to be killed themselves—and their wives were killed at three times the rate of other, more peaceful men. Most prolific killers lost their wives and had to remarry—which made it look as if they had more wives if they survived.

Misrepresentation Here, Tierney omits important information which supports the validity of Chagnon's result. Tierney refers to a recent ethnography of the Waorani in which the authors actually went out and collected the data to test Chagnon's model. The problem was, since all Waorani males had participated in a killing, they could not separate killers from non-killers. Instead they categorized men based on how many killings they had participated in: 1–5, 6–10, and 11+. Then they compared the numbers of wives and offspring among men in each of these categories. They found that killers of 1–5 people averaged 1.35 wives and 4.37 offspring, killers of 6–10 people averaged 2.00 wives and 6.08 offspring, and killers of 11+ people averaged 2.25 wives and 8.25 offspring. Thus, these data are highly consistent with those of Chagnon. The Robarcheks have essentially replicated Chagnon's finding, although they have a different interpretation of this result. They go on to present data showing that more prolific killers are more likely to get killed themselves and to lose a wife to violence; the latter are the only data that Tierney chooses to report. Tierney thus omits what is both the crux of the Robarcheks' study, and also the most useful element for evaluating the reliability of Chagnon's result: the successful replication of that result. . . .

Insinuates That Chagnon Dishonestly Confounded Unokais and Headmen

Claim Tierney insinuates that Chagnon dishonestly includes headmen, in addition to unokais, in his sample and that the presence of headmen somehow skewed his results:

> "In his *Science* piece all headmen were also included as "killers," a confusion of categories; when the headmen were factored out, the study's statistical significance in one of its major age categories collapsed, Chagnon admitted. He would not say which category it was. . . . Again, Chagnon maintained a tenacious silence in the face of public challenge, this time by the anthropologist Brian Ferguson."

Misrepresentation Chagnon does indeed include headmen in his sample of unokais, but only because these headmen are unokai, as Chagnon states clearly: "All headmen in this study are unokai." Tierney seems to suggest that Chagnon includes some headmen that he knows not to be unokai. Brian Ferguson, in *American Ethnologist,* did challenge Chagnon's inclusion of headmen in his study, saying that since headmen usually have more wives and children, and since all headmen in the study were unokai, the inclusion of headmen might increase the correlation between unokainess and reproductive success. Ferguson's point is actually misguided: the fact that all headmen were unokai is highly consistent with Chagnon's theory that in tribal societies "cultural success leads to biological success," i.e. good warriorship leads to high social status, which in turn leads to high reproductive success, and it is absurd to suggest that the presence of unokai headmen somehow contradicts a theory which it in fact strongly supports. Nevertheless, in a piece entitled "Response to Ferguson" which immediately followed Ferguson's challenge in the same issue of *American Ethnologist,* Chagnon agreed to reanalyze the data with headmen removed. Even with headmen removed, unokais (compared to non-unokais) had significantly more offspring in all four age categories, and more wives in three of four age categories ($ps < .05$). In one age category (ages 31–40), the difference between unokai and non-unokai wives was just barely not significant ($p = .07$). The statistical "collapse" to which Tierney refers is apparently the fact that $p = .07$ rather than $<.05$ for the 31–40 category, an extremely minor discrepancy misleadingly referred to as a "collapse." And there was no "tenacious silence" by Chagnon with regard to which age category was affected by the removal of headmen: Chagnon states clearly in his *American Ethnologist* piece that the category is "31–40." Tierney is clearly aware of this article (he cites it and it appears in his bibliography), so it is odd that he seems to overlook it here. . . .

Misrepresents Chagnon's Explanation for Unokai Reproductive Success

Claim Tierney suggests that Chagnon claims that the link between killing and reproductive success is due solely to the fact that Yanomamö killers are more successful in abducting women in raids. Tierney notes that this link is "tenuous" because only a "low" number of women are actually abducted in raids:

> Nor was there anything but the most tenuous connection between killing, raiding, and the capture of women. The number of women captured in the warfare of the Yanomami is low, despite their reputation. . . . Yet the popular image of the Yanomami waging war for women persisted. Chagnon deftly created it by repeatedly claiming that men went on raids, captured women, and raped them at will afterwards.

Misrepresentation In fact, Chagnon has stated repeatedly that when he says the Yanomamö "fight over women," he does not mean that they usually initiate raids for the purpose of abducting women. He simply means that most conflicts begin as some kind of sexual dispute, and he makes this clear in the target article: "most fights begin over sexual issues: infidelity and suspicion of

infidelity, attempts to seduce another man's wife, sexual jealousy, forcible appropriation of women from visiting groups, failure to give a promised girl in marriage, and (rarely) rape." On the same page he is clear that most wars are perpetuated by revenge, not the desire to abduct women: "The most common explanation given for raids (warfare) is revenge for a previous killing, and the most common explanation for the initial cause of the fighting is 'women'." In his famous ethnography—cited extensively by Tierney—Chagnon says "although few raids are initiated solely with the intention of capturing women, this is always a desired side benefit" and "Generally, however, the desire to abduct women does not lead to the initiation of hostilities between groups that have no history of mutual raiding in the past." Tierney completely ignores that Chagnon downplays the significance of abduction as a motivation to raid and then claims that Chagnon "deftly created" the image of the Yanomamö waging war in order to abduct women.

Further, by concentrating exclusively on abduction as the only explanation for the high reproductive success of unokais, Tierney ignores what Chagnon claims might be "the most promising avenue of investigation to account for the high reproductive success of unokais," the fact that "cultural success leads to biological success." Chagnon explains that unokais, because of their prowess and willingness to take risks in military matters, are regarded as more valuable allies than non-unokais: "in short, military achievements are valued and associated with high esteem." This high status of unokais makes them more attractive as mates. In a published response to criticism about the target article, Chagnon goes into even greater detail about how unokai status makes men more attractive as mates.

Why Has Tierney Been So Dishonest?

To conclude our preliminary report, we ask the obvious question, "Why has Tierney been so dishonest?" The short answer is, we don't know. We offer the following two speculations [one included here]—but we must stress that these are only speculations, speculations we ourselves find less than satisfying. . . .

The field of anthropology has been riven for at least the last two decades by a debate between 'scientifically oriented' anthropologists and 'humanistically oriented' anthropologists. The former tend to believe that there is an objective human reality and that scientific methods will help us discover it. The latter tend to believe that realities are relative, and socially or culturally constructed, and they are often extremely skeptical and critical of Western science. The debate between these two camps has frequently been so bitter that it has caused prominent anthropology departments, like Stanford's, to split in two (`http://www.stanford.edu/group/anthro`). The debate is not confined to anthropology. It is widespread in the humanities and social sciences, and has come to be known as the Science Wars.

Tierney clearly hoped to successfully indict two of the most famous scientists to work with indigenous people in the Amazon, Chagnon and Neel, with serious crimes and breaches of ethics, and thus strike a blow against scientific, and particularly evolutionary, anthropology. For students and others, we

provide our perspective on this issue, and how it may account, in part, for Tierney's dishonesty.

There are three fundamental aspects of Chagnon's career that place him at ground zero in the debate between 'scientific' anthropologists and 'humanistic' anthropologists. First, Chagnon has been a staunch and vocal proponent and practitioner of scientific anthropology, one whose books and films are widely assigned in anthropology courses around the world. Second, and even more galling to 'humanistically' oriented anthropologists (and disconcerting to many 'scientific' anthropologists as well) is Chagnon's use of sociobiological theory. Sociobiology is a set of theories and general principles about animal social behavior that derive from Darwin's theory of evolution by natural selection. Although biologists were excited by the sociobiological theories that appeared in the 1960's and 1970's, there was an immediate outcry by some biologists (e.g., Stephen J. Gould) and many social scientists when E. O. Wilson suggested that sociobiology might be useful for understanding *human* social behavior. It was 'obvious' to both sides in the sociobiology debate that the other side was motivated entirely by politics. In the ensuing war of words between supporters and critics of sociobiology, the field became stigmatized. Few social scientists are willing to use the theory, and even the many biologists employing sociobiology in their study of non-human animals avoid mentioning the word 'sociobiology.' Despite this, sociobiology is a standard part of the theoretical toolkit used by biologists in virtually every biology department in the world. It is, without doubt, the theory most widely used to study and understand the social behavior of all (non-human) living things. The world's most prestigious scientific journals, Science and Nature, routinely publish research articles using sociobiology, and hundreds of research articles using sociobiology are published every year in major biology journals. Applying sociobiology to humans, however, remains strictly taboo. Chagnon has openly violated this taboo by interpreting his data in light of sociobiological theories.

Finally, Chagnon has focused his career on one of the most contentious issues in anthropology: violence and aggression in small-scale, 'primitive' societies. Critiquing Western culture has been a popular topic in anthropology since the 1920's. (In fact, a widely used cultural anthropology text is titled *Anthropology as Cultural Critique*.) In order to critique Western culture, anthropologists often feel they must find non-Western cultures that do things better. Because violence and aggression in Western societies are well deserving of critique, anthropologists hoped to discover societies with little aggression or violence that could serve as examples of a better way of living. Chagnon, by contrast, argues that violence and aggression are common in most non-Western societies—even small-scale societies like the Yanomamö—and that violence and aggression are probably part of human nature. This has infuriated the many anthropologists who prefer practicing anthropology as cultural critique. The favorite alternative to Chagnon's interpretation of Yanomamö war is that of Brian Ferguson. Ferguson, unsurprisingly, blames Yanomamö war on the influence of Western culture.

By taking aim at Chagnon, Tierney has charged into the middle of this debate on the side of the humanists against the scientists, particularly against

the tiny minority who apply Darwinian theory to people. The subtitle of his book is "How Scientists and Journalists Devastated the Amazon." The very first words in the book, in the frontpiece, are from Daniel Dennett: "It is important to recognize that Darwinism has always had an unfortunate power to attract the most unwelcome enthusiasts—demagogues and psychopaths and misanthropes and other abusers of Darwin's dangerous idea." (Although Tierney doesn't mention it, Dennett is actually a strong advocate of Darwinian approaches to social science, and has written in defense of Chagnon.) And much of the book is a muddled attempt to attack Chagnon's sociobiological approach to Yanomamö warfare. Tierney constantly inserts comments like "Chagnon picked up where Social Darwinists left off" (Ch. 2), and he is even willing to make unsupported accusations of murder: "the incredible faith the sociobiologists had in their theories was admirable. Like the old Marxist missionaries, these zealots of biological determinism sacrificed everything—including the lives of their subjects—to spread their gospel." (Ch. 2).

Maybe Tierney thought that if he could destroy Chagnon, arch-enemy of many humanistic anthropologists and culture critics, he would be a hero in the Science Wars. And maybe he really thought a victory in the Science Wars would help the Amazon and its peoples. But the Amazon is not being devastated by scientists. Or journalists. Or sociobiologists. It is being devastated by logging, mining, road building, and slash-and-burn farming by the region's burgeoning population. Character assassination will do precisely nothing to change this.