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DEPARTMENT OF ANTHROPOLOGY

This report was prepared by members of a team at UCSB investigating the allegations. The report reflects the findings of this team, however, and is not an official statement issued by the University of California or the Department of Anthropology.

Preliminary report

The major allegations against Napoleon Chagnon and James Neel presented in *Darkness in El Dorado* by Patrick Tierney appear to be *deliberately* fraudulent.

The investigation is ongoing, and not all questions have been answered. However, this book appears to be deliberately fraudulent. On those points where we have reached firm conclusions, we find that Patrick Tierney has misconstrued or misrepresented his primary sources to a considerable degree in an effort to support his allegations. The report below is preliminary. As such it contains some tentative conclusions which require further investigation and checking by experts. The failure of this report to address many of the less significant allegations should NOT be construed as an implicit endorsement of those claims; we have focused only on the most serious charges in this preliminary report.

Additional information is available at the following web sites:

The National Academy of Sciences statement: http://national-academies.org/nas/eldorado
The UCSB Anthropology team web site: http://www.anth.ucsb.edu/chagnon.html
The University of Michigan statement: http://www.umich.edu/~urel/darkness.html
U. Michigan investigator: http://www.egroups.com/message/evolutionary-psychology/7934
Slate article by John Tooby: http://slate.msn.com/HeyWait/00-10-24/HeyWait.asp
More from John Tooby: http://www.psych.ucsb.edu/research/cep/eldorado/witchcraft.html
SLAA commentary on Neel: http://www.egroups.com/message/evolutionary-psychology/8370
Susan Lindee's AAA talk: http://ccat.sas.upenn.edu/hss/faculty/neel.htm
Int. Genetic Epidemiology Society: http://hydra.usc.edu/iges/Darkness/Darkness.html

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Document History

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November 12, 2000:	Original posting of the Preliminary report on the print copy of *Darkness*.
December 5, 2000:	Material by Wilson and Hornick on vaccine safety added; Jane Lancaster's letter added; Hurtado AAA talk added.
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October 11, 2001:	Misquotation of Chagnon's Santa Barbara Magazine article added to introduction. Minor edits of several sections.

Executive summary

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http://slate.msn.com/HeyWait/00-10-24/HeyWait.asp

Lately I've been engrossed in—and in some sense involved in—the most sensational scandal to emerge from academia in decades. The scandal erupted last month when two anthropologists, Terry Turner and Leslie Sponsel, sent a searing letter to the president of the American Anthropological Association. The letter distilled a series of chilling "revelations" made by the journalist Patrick Tierney in his forthcoming book *Darkness in El Dorado: How Scientists and Journalists Devastated the Amazon*. According to Turner and Sponsel, the scandal unearthed by Tierney, "in its scale, ramifications, and sheer criminality and corruption," is "unparalleled in the history of Anthropology." Turner and Sponsel listed a horrifying series of crimes—"beyond the imagining of even a Josef Conrad (though not, perhaps, a Josef Mengele)"—including genocide, allegedly committed by U.S. scientists against the Yanomamö, an indigenous people living in the Venezuelan and Brazilian rain forest.

Turner and Sponsel's letter spread like a virus over the Internet, quickly driving the controversy into the mainstream press. A story in Britain's *Guardian*—"Scientist 'killed Amazon indians to test race theory' "—was followed by accounts in *Time* and the *New York Times*, on NPR's *All Things Considered*, and so on. The accusations drew strength from two institutions that endorsed Tierney's credibility: *The New Yorker*, known for its obsessive fact-checking, published an adapted excerpt from the book early this month; and the fact that the book is scheduled for publication next month by W.W. Norton, which is highly respected by academics.

Pre-publication galleys of the book show why it inspired such trust. Tierney's argument is massively documented, based on hundreds of interviews, academic articles, and items uncovered under the Freedom of Information Act, not to mention his own visits among the Yanomamö. Through 10 years of dogged sleuthing, it would seem, Tierney dragged a conspiracy of military, medical, and anthropological wrongdoing into the light. Last week, when finalists for this year's National Book Awards were announced, *Darkness in El Dorado* was listed in the nonfiction category.

There is only one problem: The book should have been in the fiction category. When examined against its own cited sources, the book is demonstrably, sometimes hilariously, false on scores of points that are central to its most sensational allegations. After looking into those sources, I found myself seriously wondering whether Tierney had perpetrated a hoax on the publishing world. Of course, only he knows whether he consciously set out "to trick into believing or accepting as genuine something that is false and often preposterous"—the dictionary definition of a hoax. But the book does seem systematically organized to do exactly that. And, to a frightening extent, it has succeeded.

The accusations are directed primarily against James Neel, a physician and a founder of modern medical genetics (now dead), and Napoleon Chagnon, perhaps the world's most famous living social anthropologist. Tierney describes Neel as an unapologetic "eugenicist" who believed as a "social gospel" that "democracy, with its free breeding for the masses and its sentimental supports for the weak" is a eugenic mistake.

Tierney argues that, starting in the 1960s, Neel and his researchers were funded by the Atomic Energy Commission to conduct horrifying medical "experiments" on the Yanomamö. Far and away the most serious allegation is that the researchers killed hundreds or even thousands by knowingly releasing a contagious measles virus into the previously unexposed Yanomamö population. As Turner and Sponsel put it, "Tierney's well-documented account … strongly supports the conclusion that the epidemic was in all probability deliberately caused as an experiment designed to produce scientific support for Neel's eugenic theory." Chagnon—described by Tierney as a "disciple" of Neel's—was implicated in this crime and charged with inadvertently bringing other devastating diseases as well. What's more, Chagnon was said to have been the main cause of the violence he saw among the Yanomamö and more generally to have twisted his scholarly portrayal of them to bolster his Hobbesian theories of human nature.

I was an early recipient of this ethics complaint, in that small number of Internet nanoseconds when it was still considered confidential. As president of the Human Behavior and Evolution Society, of which Chagnon was a prominent member, I was obliged to investigate the allegations, just as the American Anthropological Association would be doing. Chagnon had been my departmental colleague since I moved to the University of California, Santa Barbara, a decade ago, and I consider him a friend. But I'd never met Neel, and for all I knew, he really was a eugenics crackpot, exploiting the isolation of his field site in some warped way. And as for Chagnon—well, how much do we really know about the person in the next office?

Starting with the most serious charge—genocide—I looked up what Neel himself wrote about the measles epidemic. Tierney alleged that a measles vaccine Neel's team administered to the Yanomamö, Edmonston B, was a dangerous agent—and was known to be so at the time—and triggered the epidemic. In Neel's account (a cover-up?), what Tierney finds suspicious—that a measles outbreak started around the time Neel first administered the vaccine—has a different explanation: After Neel learned about the incipient outbreak, he started vaccinating people, trying furiously to head off an epidemic.

To my nonspecialist ears, Tierney's theory sounded possible: Many vaccines, including measles vaccines (then and now), use attenuated live virus, which, when injected, gives the recipient an infection that is supposed to stimulate the immune system. So why couldn't a live virus have spread contagiously from Yanomamö to Yanomamö, launching a deadly epidemic?

I started putting in calls to the Centers for Disease Control and Prevention in Atlanta. Conversations with various researchers, including eventually Dr. Mark Papania, chief of the U.S. measles eradication program, rapidly discredited every essential element of the Tierney disease scenarios.

For example, it turns out that researchers who test vaccines for safety have never been able to document, in hundreds of millions of uses, a single case of a live-virus measles vaccine leading to contagious transmission from one human to another—this despite their strenuous efforts to detect such a thing. If attenuated live virus does not jump from person to person, it cannot cause an epidemic. Nor can it be *planned* to cause an epidemic, as alleged in this case, if it never has caused one before.

Experts elsewhere have confirmed this—and have confirmed the safety of the Edmonston B vaccine under the conditions in which it was used. All told, the evidence against Tierney's genocide thesis is now so overwhelming that even Turner, its once-enthusiastic supporter, has backed off. He concedes that the medical expert he finally got around to consulting took Tierney's medical claims and "refuted them point by point."

You'd think the Tierney book, 10 years in the making, might mention the relevant and easily discoverable fact that, as the Michigan medical report puts it, "live attenuated vaccine has never been shown to be transmissible from a recipient to a subsequent contact." Somehow it omits it (even though this information is featured prominently in a paper Tierney cites five times!). The *New Yorker* piece also fails to mention it and instead says, "Today, scientists still do not know whether people who have been vaccinated with Edmonston B can transmit measles." This is literally true, but only because scientists use the word *know* very carefully. Scientists also do not *know* that *The New Yorker* is not riddled with a cult of pedophilic Satan worshipers or that the Pentagon is not in the control of extraterrestrials masquerading as generals. If you ask a *good* scientist about each of these allegations, she would be forced to answer, yes, it's possible. But she will consider it relevant and worth mentioning, as *The New Yorker* does not, that the failure to substantiate a hypothesis given millions of opportunities floats the hypothesis out toward the scientific neighborhood inhabited by ESP and UFOs.

Once I had seen Tierney's most attention-getting claim crumble, I started through the galleys of his book systematically, evaluating it against available sources with the help of various colleagues. Almost anywhere we scratched the surface, a massive tangle of fun-house falsity would erupt through.

We had to accept from the outset that scores of conversations reported in the book are with people scattered through the rain forest, virtually impossible to contact (even for *The New Yorker*'s energetic fact-checkers). So Tierney's veracity would have to be judged on the basis of sources that could be reached. I had already run into one such source—Papania of the CDC, whom Tierney had interviewed for the book. Papania told me that he was troubled to find, in galleys he'd recently been sent, that Tierney had misquoted him. Tierney had him endorsing the idea that the vaccine was a plausible cause of the epidemic, which was not, in fact, his view.

It soon became evident that Tierney was no more faithful to written sources than to oral ones. To begin with, comparing Neel's autobiography with Tierney's use of it is an education in audacity. Whatever Tierney might have wished to convey by calling Neel a "conservative" and claiming that "Neel's politics were too extreme for Reagan's council on aging," Neel's book shows him to be a supporter of Al Gore ("superb," "the most hopeful recent sign"), a Reagan-Bush basher ("chilling," "myopic"), pro-nuclear-disarmament, and an enthusiastic environmentalist. Neel's conflict with the advisory council on aging, it turns out, came when he objected to the diversion of money from poor children into research on how to artificially extend the human life span—research that, Neel speculated, would wind up benefiting mainly the affluent.

And what of Tierney's claim that Neel was a "eugenicist" who believed as a "social gospel" that "democracy, with its free breeding for the masses and its sentimental supports for the weak" was a eugenic mistake? It turns out that Neel had been a fierce opponent of eugenics for 60 years, since his student days. To dramatize his opposition, he labeled his beliefs *euphenics*, emphasizing the medical and social importance of environmental interventions. As Neel put it, the "challenge of euphenics is to ensure that each individual maximizes his genetic potentialities" through the creation of environments in which each can flourish, and "to ameliorate the expression of all our varied genotypes"—ameliorate the *expression* of our genes, not the genes themselves. Neel lists, as examples of good social investments, prenatal care, medical care for children and adolescents, good and equal education for all children, and so on.

There is not a word on any of the pages Tierney cites about how “democracy … violates natural selection.” Indeed, though worried about overpopulation, Neel argues that there is no scientific or moral basis for preventing anyone from being a parent, and he says that guaranteeing the equal right to reproduce would “preserve insofar as it’s possible all of [our species’] poorly understood diversity.” Neel even does an extended calculation to debunk the eugenicist fear that reproduction by those with genetic defects threatens the gene pool!

Neel does analyze, in the standard way population geneticists do, how unfavorable genetic mutations were “selected out” more rapidly before the invention of agriculture and subsequent creature comforts, and before the transition from polygamy to monogamy (which slows the form of natural selection known as “sexual selection”). Here, as elsewhere in the book, Tierney works feverishly to erase the simple distinction—basic to all scientific discussion—between describing something and endorsing it. In this case, it was a difficult erasure, since Neel, far from wanting to return humanity to a lost world where natural selection is more intense, had called this “unthinkable.” (Incidentally, if you’re wondering why Neel might have found a measles epidemic useful as a test of his supposed eugenic theories, as Tierney claims, the answer is that Tierney never provides a coherent explanation.)

This pattern of falsification—of which I have mentioned only a small sampling—extends to Tierney’s assault on Napoleon Chagnon. To begin with, Tierney—like some other Chagnon critics—caricatures Chagnon’s view of human nature, as if Chagnon considered people innately violent, period. In reality, Chagnon, pondering the relative rate that “people, throughout history, have based their political relationships with other groups on predatory versus religious or altruistic strategies,” concludes that “we have the evolved capacity to adopt either strategy,” depending on what our culture rewards.

Still, there’s no doubt that Chagnon has a more Hobbesian view of human nature than is popular in most anthropological circles. Tierney claims that Chagnon, to support this view, exaggerates Yanomamö violence. He doesn’t mention the fact that the rates of violence Chagnon documents are not high compared with the rates found by anthropologists in other pre-state societies. Nor does he mention Chagnon’s view that, if anything, the Yanomamö’s rate of lethal violence is “much *lower* than that reported for other tribal groups.”

Not only does Tierney generally ignore inconvenient data, citing only anthropologists who disagree with Chagnon. He also, time and again, has a way of magically turning anthropologists whose data support Chagnon into anthropologists who contradict him. For example, Tierney cites a study of the Jivaro by Elsa Redmond that he claims undermines one of Chagnon’s Yanomamö findings: that the effective use of violence contributes to social status, the acquisition of multiple wives, and the having of many offspring.

Here is Tierney’s summary of Redmond:

> Among the Jivaro, head-hunting was a ritual obligation of all males and a required male initiation for teenagers. … Among the Jivaro leaders, however, those who captured the most heads had the fewest wives, and those who had the most wives captured the fewest heads.

Here is what Redmond actually says:

> Yanomamo men who have killed tend to have more wives, which they have acquired either by abducting them from raiding villages, or by the usual marriage alliances in which they are considered more attractive as mates. The same is true of Jivaro war leaders, who might have four to six wives; as a matter of fact, a great war leader on the Upano River in the 1930s by the name of Tuki of José Grande had eleven wives. Distinguished warriors also have more offspring, due mainly to their greater marital success.

Similarly, Tierney cites anthropologist John Peters at various points in his argument that Chagnon exaggerates Yanomamö violence. But what Peters actually writes in his book *Life Among the Yanomamo* is far stronger than anything Chagnon has written: “Anyone who is even minimally acquainted with the Yanomami is familiar with the central role of war in this culture. Violence seems always just a breath away in all Yanomami relations.”

Throughout the book, Tierney is comically self-aggrandizing, often presenting as his own discoveries things plainly described in Chagnon’s publications. After complaining that Chagnon concealed the identity of villages from which some of his more controversial data were drawn, Tierney writes, “It took me quite a while to penetrate Chagnon’s data, but, by combining visits to the villages in the field with GPS locations and mortality statistics, I can identify nine of the twelve villages where all the murderers come from in his *Science* article.” Or, if he didn’t want to do all that walking and calculating, he could have gotten this information by consulting sources listed in his own bibliography, such as a 1990 Chagnon article and Chagnon’s *Yanomamo Interactive* CD.

Although Tierney’s many misrepresentations are riveting, his omissions are equally important—and harder for fact-checkers to spot, since omissions don’t have footnotes. They figure centrally in two of Tierney’s core accusations: that Chagnon inadvertently introduced various diseases besides measles into the region just by going there; and that Chagnon, by giving pots, machetes, and other steel tools to the Yanomamö, somehow exacerbated the rate of warfare, thus influencing the very data he gathered.

Both of these claims are logically possible. But Tierney fails to mention some relevant facts (well known to him) that call them into question.

Tierney presents the Yanomamö as if they were isolated in a petri dish, except when Chagnon visited and sneezed. In reality, the Yanomamö are tens of thousands of people, surrounded by other people with real diseases who have regular transactions with them. Moreover, this 70,000-square-mile area is penetrated by thousands of non-Yanomamö: missionaries, gold miners (over 40,000), highway workers, government officials, tin miners, loggers, ranchers, rubber tappers, drug smugglers, soldiers, moralists like Tierney, and on and on. This whole area is beset by epidemics of various kinds, as the Yanomamö tragically encounter diseases from the industrialized world. So, the probability that Chagnon or Neel or Tierney in particular is the source of any specific epidemic is, crudely speaking, one divided by these tens of thousands. Yet Tierney strangely insists that disease, like war, somehow specifically dogs Chagnon’s movements.

To reliably identify the major sources of disease, one would need to collect demographic data in many villages and map it against the various forms of contact. As it happens, this is just what Chagnon did, and he gradually concluded that the Catholic missions were serious sources of disease, largely because of their regular roles as points of contact and entry. Yanomamö living at the missions benefited from the medical care, but those living close enough to catch their diseases yet too far to get the medical care suffered. When Chagnon saw the pattern, he blew the whistle. This did not endear him to the missionaries, who have ever since been the source of enough anti-Chagnon anecdotes to keep an enterprising journalist busy for years.

Similarly, Tierney says that competition over the pots and machetes and other steel tools that Chagnon gave the Yanomamö sometimes led to war. This too is logically possible. The Yanomamö certainly valued Chagnon’s gifts, since cutting the jungle back for their crops was much easier with machetes. But Tierney fails to mention that Chagnon’s contributions (made so that he would be allowed to collect data) were dwarfed by all the other sources of such items, such as the military, who hired Yanomamö laborers, and especially the vast mission system, which imports boatloads of machetes and other goods, and even has its own airline.

While Tierney considers Chagnon's distribution of steel tools an outrageous threat to peace, he amazingly gives a free pass to the introduction by others—including some missionaries—of hundreds of shotguns. These weapons are known to have been used by the Yanomamö in raiding from mission areas to the less well-armed villages where Chagnon worked. Chagnon blew the whistle on this, too.

In short, what Tierney leaves out of his story is that what his key sources have accused Chagnon of—causing disease and warfare—just happens to be what Chagnon had previously accused some of them of doing. Indeed, a prerequisite of Tierney's ability to do research in this restricted area was almost certainly his endorsement of one side in this feud. Tierney's translators, his guides, his selection of interviewees—all carry the strong implication that he received a guided tour drenched with these local politics. Throughout the book, Tierney goes to extraordinary lengths to explain away real causes of disease and violence that trace back to his patrons. (He has a whole appendix devoted to attacking evidence that the missionaries spread disease.) When this context is supplied, the unremitting denunciations of Chagnon start to sound different, and Tierney, *The New Yorker*'s intrepid "Reporter At Large," appears in a less flattering light.

Chagnon has made enemies in academia as well as in the rain forest. Anthropology is full of people who still subscribe to Rousseau's "noble savage" view of human nature, and their battles with Chagnon have been intense. That is why Tierney could pepper his *New Yorker* article, and his book, with anthropologists who question Chagnon's Yanomamö data—a technique of great rhetorical power unless you know about all the anthropologists Tierney doesn't mention whose data support Chagnon. Chagnon's longtime critics include Turner and Sponsel, a fact that explains their uncritical and hyperbolic embrace of the Tierney book, and a fact that isn't mentioned in their incendiary letter to the American Anthropological Association.

With experts increasingly coming forward to debunk various aspects of the Tierney book, the accusations against Neel and Chagnon "are crumbling by the hour," as it was put by Lou Marano of UPI, one of the few reporters to deeply examine the credibility of Tierney's charges. But much damage has already been done—and not just to the reputations of Neel and Chagnon. Tierney's claim that an immunization program can start an epidemic has been carried around the world in media reports. This myth could compromise the ability of health workers to administer such programs, especially in poor countries, and people could die as a result. Moreover, indigenous cultures will not benefit from the public's impression that they are endangered only by the occasional anthropologist, when in fact they are victims of far more powerful forces, ranging from well-meaning missionaries to untrammeled modernization.

The slow-motion tragedy of the world's indigenous peoples continues, and Tierney's thoroughly dishonest book is just one more exploitation of them.

In the subsequent sections of this report, we document this dishonesty in detail.

Introduction

"My guess is that it will become a classic in anthropological literature…." *John Horgan, New York Times, Nov. 12, 2000.*

"But, as the instances accumulate and their implications come home, it all, in some strange way, begins to add up." *Clifford Geertz, The New York Review of Books, Feb. 8, 2001.*

"While some of Tierney's reporting has come under fire, this is nonetheless a revealing book, with a cautionary message that extends well beyond the field of anthropology. It reads like an allegory of American power and culture since Vietnam." *Marshall Sahlins, The Washington Post, Dec. 7, 2000.*

"In this climate, I gradually changed from being an observer to being an advocate. It was a completely inverted world, where traditional, objective journalism was no longer an option for me." *Patrick Tierney, Darkness in El Dorado.*

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.(9) Adults averaged four feet seven inches in height,(10) and children had among the lowest weight-height ratios on the planet.(11) (Tierney, Ch. 2; numbered notes in the original.)

References are supposed to support, not refute, the claims one is making. Tierney's reference (10) 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).

Tierney cites Holmes' paper four times (here; Ch. 5, note 47; and twice in Ch. 16, notes 18 and 30) 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.

Tierney not only claims that Chagnon has painted an inaccurate portrait of the Yanomamö, but that he terrorized them and spread sickness among them as well. Here is Tierney's harrowing account of what appears to be reckless helicopter descents by Chagnon and his colleagues, and the Yanomamö's desparate attempt to drive Chagnon away:

> In the Siapa Highlands, there were no gold miners, but the FUNDAFACI expeditions [expeditions in which Chagnon took part] led to a similar crisis, in which wars and epidemics spread from uncontrolled contact by outsiders who possessed marvelous, terrible machinery. To start with, four villages had their roofs blown off in the tornadolike descents of the expedition helicopters.
>
> Narimobowei-teri had its roof blown away in August 1990. Napoleon Chagnon described this experience for *Santa Barbara Magazine:*
>
> "A few feet away from landing, we aborted when we saw the leaves of their roofs being blown away by the chopper's downblast. We saw people fleeing in terror and men throwing sticks and stones at us as we retreated up and away . . . ."

But, what follows those four dots at the end of Tierney's quotation of Chagnon? This is the quote in full (material omitted by Tierney in bold):

> A few feet from landing, we aborted when we saw the leaves of their roofs being blown away by the chopper's downdraft.
>
> We saw people fleeing in terror and men throwing sticks and stones at us as we retreated up and away. **I later learned this was because they were disappointed we were not landing--they were terrified by the helicopter, but delighted to have us come.** (Napoleon Chagnon, To Save the Fierce People, Santa Barbara Magazine, January/February 1991, pp 37-38)

A bit further on in the short article, Chagnon recounts the warm welcome he and other expediction members received. Tierney also fails to mention that Chagnon was traveling with a team medical doctors associated with Parima Culebra (a Venezuelan government program) who were working to develop a health-delivery program for the Yanomamö, and he fails to mention the Yanomamö's lack of concern about losing a few leaves off their roofs—some, in fact, insisted that the helicopters land in the center of the village despite the minor roof damage (see appendix XV).

The most notorious claim in Darkness in El Dorado is that James Neel, a doctor and geneticist, and Chagnon used a measles vaccine called Edmonston B that they knew, or should have known, was dangerous. We will examine this claim in detail later in this report. Here, we present one example of the 'evidence' Tierney offers in support of his allegation. In Tierney's version, G. S. Wilson, a public health expert, appears to clearly state that Edmonston B was known to be dangerous, or even fatal:

> In 1961, the National Institutes of Health sponsored a conference on the Edmonston vaccine. The keynote speaker was G. S. Wilson, head of England's Public Health Laboratory Service, who warned of possible fatalities. And, in unusually blunt language, he said the test of a vaccine was whether "the disturbance caused by the vaccination" was "greater than that caused by the disease itself." With most vaccines, the difference was obvious; in the case of the Edmonston strain, however, Wilson thought the difference between the disease and the vaccine was "not so clear." (Tierney, p. 56)

Here, now, is Wilson's actual statement (material quoted or referred to by Tierney in bold):

> This conference is called to consider immunization against measles, and it is fitting therefore, that in introducing measles as a universal disease, I should refer to the subject of vaccination. There are certain general principles that should govern the policy of vaccination against any disease:

> (a) The vaccine should be harmless to the healthy child. In practice no vaccine has yet been devised that has not occasionally given rise to a severe and **sometimes fatal reaction**. The risk is much higher with some vaccines than with others. Unfortunately, for any given vaccine the risk can be assessed only by experience.
>
> (b) **The disturbance caused by vaccination** should not be **greater than that caused by the disease itself**. There is no doubt that in the prevention of smallpox the febrile eruption that follows primary vaccination is far less severe than that caused by the natural disease. In measles, however, this **not so clear** [sic]. Though at one time measles had a high case fatality rate resulting in a serious mortality, it has now in many parts of Europe and America become so mild that death is quite exceptional. In 1959, for example, an epidemic year in England and Wales, the total deaths from measles numbered only 98 in a total population of 45 million. Under these conditions, is the disease worth preventing, or should we concentrate on shielding infants and very young children from the risk of infection and protecting them with gamma-globulin when this is impossible? It is difficult to answer this question without knowing more exactly how much permanent damage measles does to the healthy child. *In the tropics, of course, the position is different. There the case fatality rate for measles is high, and a much stronger case can be made out for vaccination*. (Wilson 1962; italics added)

As any reader can easily see, Wilson was not warning about possible fatalities from Edmonston B, he was making introductory comments on vaccines against *any* disease. Nowhere in his article does he single out Edmonston B or any other measles vaccine as particularly dangerous. Wilson was concerned, but not about the safety of Edmonston B relative to other vaccines; rather, he was concerned because measles is so *mild* in Europe and the United States that even mild vaccine reactions might indicate against using it. As Wilson reasonably asks, “Under these conditions, is the disease worth preventing...?” Regarding tropical populations like the Yanomamö, Wilson has this to say (material in italics above): “In the tropics, of course, the position is different. There the case fatality rate for measles is high, and a much stronger case can be made out for vaccination.” Tierney of course fails to mention those two sentences, sentences that support the use of Edmonston in tropical populations like the Yanomamö.

It is impossible that Tierney misunderstood Wilson. His misquotation of Wilson is, without doubt, deliberately fraudulent. This dishonest use of source material characterizes almost every citation in the book that we have investigated. We are stunned that Tierney’s publishers W. W. Norton and the New Yorker would want to be associated with this kind of ‘journalism.’

Detailed Evaluation of Chapter 4: Atomic Indians, & Chapter 5: Outbreak

Tierney, in Chapter 4 and Chapter 5 of his book, attempts to convince the reader that the 1968 measles epidemic among the Yanomamö may have been caused by an experiment conducted by James Neel, Napoleon Chagnon, and others. This conjecture relies on several elements, each of which is easily shown to be false, often using the same sources that Tierney cites. Tierney's argument goes something like this: James Neel, a prominent geneticist, was the mastermind. He had morally and scientifically questionable theories that he wanted to test using the Yanomamö as unwitting subjects. Testing these theories required Neel to administer a vaccine known to be dangerous, in order to observe its effects on a population uniquely suited for such an experiment. The experiment went horribly wrong, causing an actual epidemic that killed thousands. In the aftermath, Neel, Chagnon, and their associates attempted a cover-up, concocting a plausible story that they were merely attempting to halt an epidemic already in progress, and pointing their finger at a sick Brazilian as the implausible cause of this epidemic. Nonetheless, they still managed to collect valuable information, information which they claimed supported Neel's eccentric ideas.

Tierney's views were advertised (and even somewhat exaggerated) by two credulous anthropologists, Terence Turner and Leslie Sponsel, in a breathless email to officers of the American Anthropological Association. This email was soon widely circulated on the internet (Turner has since retracted his support for the notion that measles vaccine can cause an epidemic).

Tierney presents much of his argument by laying out a set of closely aligned and supposedly factual dots, and allowing the reader to draw the obvious lines between them. This may protect him and his publishers, W. W. Norton and the New Yorker, in a court of law, but we won't waste time quibbling about what Tierney actually meant. We will merely address the conclusions that Tierney clearly hopes the reader will draw from his account. We will show that these conclusions are false. We will also show that a much weaker version of Tierney's thesis–that the epidemic was accidentally caused by Neel and Chagnon during a humanitarian vaccination program–is also false. There *was* an humanitarian vaccination program, but it saved hundreds of lives, and caused no mortality whatsoever.

Tierney strongly implies that Neel et al. caused the 1968 measles epidemic among the Yanomamö by administering vaccine:

> There was a much simpler explanation for the measles epidemic, however, and it was also implicit in the original account by Neel and Chagnon. According to them, the Yanomami first vaccinated at Ocamo "had definite rash" in strong reactions that began six days after vaccination and continued for more than ten days (January 29-February 8). Significantly, "a few reactions were indistinguishable from moderately severe measles." There was no doubt, then, that a full measles rash and fevers first appeared among the Ocamo Yanomami within a week of the Indian's vaccination. Prior to the Yanomami's severe vaccine reactions, according to Neel's own chronology, no one had seen the disease's telltale lesions. (Tierney, p. 67)

Although experts, including the co-developer of the measles vaccine (who reviewed the materials cited by Tierney), have clearly stated that there is no scientific basis to the claim that one can cause an epidemic by administering vaccine, is it possible that Tierney somehow did not know this? One of Tierney's favorite references on measles vaccine is an article by Markowitz and Katz (1994). He cites it five times in Chapter 5 (ref. # 29, 38, 63, 86, & 87). Despite his heavy reliance on this article, Tierney fails to either note or mention the following paragraph, which appears in a section relevantly subtitled ‘Results of Vaccination’:

> Because wild virus is so highly transmissible, both virological and clinical studies with susceptible contacts were conducted in early vaccine investigations [10 references follow]. These studies showed no evidence of virus excretion by vaccinees. Taking into consideration the sensitivity of the methods used, person-to-person transmission of vaccine virus has never been documented. (Markowitz and Katz, p. 244)

If Tierney wishes to imply that the vaccine virus was transmissible, he needs to grapple with the conclusion and the 10 (!) supporting references cited in this paragraph. Instead, he fails to mention them at all. As we shall see, this is quite typical of the entire book–virtually every major source cited by Tierney contains information that directly and clearly contradicts his claims, but which he fails to discuss or even mention. (Of course, the scientific consensus that the vaccine cannot be transmitted undermines entirely Tierney’s insinuation that Neel *planned* an experiment: why would he have planned an experiment that relied upon an effect that was never known to have occurred?)

Vaccine Safety

Perhaps the most important issue raised by Tierney is: was the use of Edmonston B measles vaccine, the vaccine used by Neel et al., appropriate? Although measles is often quite mild in North American and European populations, it is deadly in ‘virgin soil populations (populations with little or no previous exposure to the disease). It is therefore imperative to vaccinate these populations against measles. Did Neel and his colleagues use the right vaccine? Tierney strongly implies that Neel et al. used the wrong vaccine (he also implies that they deliberately chose a vaccine that was known to be dangerous for use among Native Americans in order to produce the kinds of symptoms that supposedly would test Neel’s theories. Tierney’s misrepresentation of Neel’s theories will be examined later in this report). Here is Tierney implying that the vaccine was dangerous and inappropriate:

> Yet, throughout these various accounts, the AEC researchers have never explained their choice of vaccine: the Edmonston B live virus. It was the most primitive measles vaccine, first developed in the late 1950’s. From the beginning, it was described as “a new disease” with serious symptoms (14). In 1959, researchers in Panama hospitalized nine children after vaccinating them with the Edmonston B; they advised against using it anywhere without emergency facilities (15). Among Canadian children, 60 percent of the Edmonston vaccines contracted fevers over 103 degrees Fahrenheit (16). These results looked suspiciously like natural measles. No rigorously controlled study of the Edmonston B and wild measles was ever conducted, because it would have meant denying children aspirin and antibiotics. In general, the Edmonston virus raised temperatures about four degrees; wild viruses, about five degrees. (Tierney p. 55, numbered citations in the original)

We have examined every source cited by Tierney on this issue, and we have found that he has substantially misrepresented *each* source; that these sources often directly and clearly contradict Tierney; and that Tierney’s falsification of the record must have been deliberate. Experts, including two of those cited by Tierney (one is the co-developer of measles vaccine, and the other a CDC expert on measles who found himself misquoted by Tierney in his book) have stated that the choice of vaccine was entirely appropriate. Another measles expert, Francis Black, also quoted by Tierney as questioning the choice of vaccine, actually advised the Neel team in 1967-68 about proper dosages of gamma globulin to be provided with Edmonston B. If he had questions about the choice of vaccine, as Tierney claims, why did he not raise them then, or three years later when he published an article discussing Neel et al.'s use of Edmonston B among the Yanomamö?

Vaccines, including measles vaccines, often produce reactions. The two *principle* reactions to measles vaccines are fever and rash. In this literature, a high fever is generally considered to be one equal to or exceeding 103 F. Edmonston B without gamma globulin produced large numbers of high fevers (sometimes in over 50% of recipients); Edmonston B with gamma globulin also produced high fevers, but in a smaller fraction of cases. Even the most modern measles vaccines produce high fevers in 5-15% of vaccinees. Thus, high fevers are *expected* in any measles vaccination program in *any* population. Importantly, Yanomamö mortality during the epidemic was largely from pneumonia, a dangerous complication of measles, *not* fevers:

> …a minimum of 36 per cent of the Indians with measles developed pneumonia. This was the direct cause of a majority of the deaths thus far known to be associated with the epidemic. (Neel et al. 1970).

To our knowledge, there was no mortality caused by fevers in this vaccination program, nor have they caused mortality in any other measles vaccination program. Tierney provides no evidence whatsoever that there were any complications from fevers, much less any mortality. This doesn't mean that doctors aren't concerned about fevers. The Neel team had an active program for managing the fever reactions caused by vaccination, as do modern vaccination programs. Francis Black, in his vaccine experiments among the Tiriyo of Brazil (Black 1969), used a more attenuated version of the vaccine, referred to as the Schwarz vaccine, that produced reactions in fewer individuals even when used without gamma globulin (which reduces reactions to the vaccine). WHO studies in measles-experienced populations showed that Edmonston B w/o gamma globulin raised average temperatures 0.92 C; Edmonston B w/ gamma globulin raised average temperatures 0.43 C; and Schwarz raised average temperatures 0.43 C. Note that the fever reaction after vaccination with Edmonston B plus gamma globulin is identical to that caused by the more attenuated Schwarz vaccine in measles-experienced populations.

Neel et al. used gamma globulin in all cases except the first round, when the gamma globulin was accidentally not available–Neel was vaccinating locals elsewhere in the region. Individuals were vaccinated without gamma globulin by a French and Venezuelan team of doctors (who were coincidentally also in the region) because there was serious concern that susceptible individuals had been, or would soon be exposed to the wild virus, and it would have been extremely dangerous to wait. Edmonston B was licensed for use without gamma globulin, and all experts recently consulted on this matter have endorsed the use of Edmonston B without gamma globulin.

Tierney cites the following sources on the safety of the vaccine:

1. A 1962 article by G. S. Wilson, Director of the Public Health Laboratory Service in England.
2. A 1962 research summary by Hornick and colleagues.
3. A field trial of Edmonston B among Native Americans and 'mestizos' in Panama.
4. A field trial of Edmonston B among Native Americans in Alaska.
5. A case study of a child with Leukemia who was vaccinated with Edmonston B.
6. A vaccine study by Francis Black among the Tiriyo of Brazil (this study was published after Neel and colleagues completed their vaccination program among the Yanomamö).

We will show that Tierney substantially misrepresents each source.

Researchers' conclusions on vaccine safety

We have discussed Tierney's dishonest use of the 1962 article by G. S. Wilson in our introduction. Tierney then cites Hornick et al. (1962):

> The consensus of participants was that "the occurrence of fever of this magnitude and incidence...will be deleterious to public acceptance and widespread use of the vaccine in its present unmodified form."

Hornick et al. were not providing a consensus of participants at the conference, they were presenting a summary of the results of their research at the conference. Hornick et al. were actually saying that although the "benign febrile response" caused the by the vaccine (and caused by measles vaccines to this day) will be deleterious to *public* acceptance, the vaccine* worked well and did not appear to have any serious complications.

Here is a statement from their introduction:

> Clinical and immunologic evaluation of attenuated measles-virus vaccines derived from Edmonston strain has revealed the relatively benign nature of vaccine-induced infection and potent immunogenicity of this virus. (Hornick et al. 1962).

And here is their entire summary (with inserted comments):

> Attenuated measles-virus vaccine propagated in canine renal or chick embryo cell culture elicited immune reactions in virtually all susceptible recipients. Experience to date indicates that the presence of neutralizing antibody and resistance to infection are synonymous. Furthermore, successfully vaccinated children were subsequently immune to naturally occurring disease [i.e., the vaccine successfully stimulated the immune system and prevented infection by wild measles in virtually all recipients]. Attenuated measles virus appeared in the blood and pharynx *but did not spread among susceptible populations*. Reaction to vaccine was overt in most children, and a significant percentage of vaccinees experienced marked febrile responses. *Although reactions were not characterized by systemic disability and the virus did not evoke any serious complications*, the occurrence of fever of this magnitude and incidence, in our opinion, will be deleterious to public acceptance and widespread use of the vaccine in its present unmodified form. (Hornick et al. 1962, emphasis added)

Tierney systematically fails to discuss any of the large number of positive conclusions on vaccine safety that appear in the sources he cites, and he ignores many other studies on vaccine safety that are easily available in the literature, almost certainly because the conclusions of these studies do not support his allegations.

Vaccine reactions in measles-inexperienced populations

Was the Edmonston B vaccine dangerous to measles-inexperienced, Native American populations? Although Black et al. 1971 concluded that the average temperature after vaccination with any of the vaccines in these groups was about 0.4 C higher than in comparative, measles-experienced groups, this is not evidence that these vaccines were dangerous. Let's compare Tierney's claim that it was known that there were dangerous reactions to Edmonston B

* The article does not identify the vaccine used in this study as Edmonston B. A close examination of the derivation of the vaccine reveals that it may actually be Edmonston A or a close relative. Edmonston A was never licensed for use due to the somewhat greater reactions associated with this vaccine. We have not yet come to firm conclusions about the identity of the vaccine used in this study, however.

in Native American populations, with his two cited sources on the matter (these sources were also cited by the New Yorker in their reply to John Tooby). These are the two sources cited in Tierney's paragraph reproduced in the preceding section (references 15 and 16).

Reference 15: Hoekenga et al. (1960). This source is interesting because they used Edmonston* *without gamma globulin* in a previously unexposed, indigenous Latin American population in Panama during an epidemic (according to the article, about 1/2 of those afflicted were mestizos, and one half "Indians"). This is the same supposedly evil or careless thing that Neel, Chagnon, and their colleagues did during the first round of vaccinations (actually, it was the French and Venezuelan doctors); during all later rounds they used gamma globulin. Tierney is right to cite this study; it is one of two whose results were available prior to the Neel et al. vaccination program in 1968. In an attempt to call into question the safety of Edmonston B, he correctly states that nine children (out of 453 inoculated) were hospitalized due to reactions to the vaccine. However, here is what the authors actually say about the hospitalizations:

> Nine children were hospitalized for reactions, but it was believed that only four really needed hospital care; the other five arrived at the hospital at night and were retained because of the limited transportation facilities. (Hoekenga et al. 1960)

So, of nine children brought to the hospital because of fevers, five didn't need any care, and only stayed because they didn't have a ride home. Considering that the four remaining children represent less than 1% of the vaccinated population, and that these children suffered no lasting harm, this is not much of an indictment of the vaccine. In fact, this was one of the first uses of Edmonston during an epidemic, and it worked well: only 0.7% of vaccinated individuals developed measles compared to 9% of controls, a thirteenfold difference (even modern measles vaccines are only about 95-98% effective in creating immunity after one dose). Here is the conclusion of this same article:

> In the overall picture, it is apparent that the measles vaccine provided good protection in all age groups. It must be emphasized, though, that vaccine reactions were somewhat severe in some children, even to the point of requiring hospitalization of a few. Since hospital facilities were available to these people at no cost, and ***since even a marked vaccinal reaction was preferable to the risk of the naturally occurring disease in infants****, it was thought that the reactions would neither constitute a drawback for use in the Panama area nor prevent the use of measles vaccine in many other areas*. It is possible, however, that in some parts of the world the rather high reaction rate might be considered a contraindication to the use of this vaccine in small children. Further attenuation of the virus should eliminate this problem. (Hoekenga et al. 1960, emphasis added)

The authors are stating that the vaccine reaction was preferable to the risk of infection with the wild virus, and they are endorsing the use of Edmonston (without gamma globulin) in this mestizo/indigenous measles-inexperienced population. Both these facts are very inconvenient for Tierney, and he doesn't mention either of them, even though this study directly addresses the key issues involved in his insinuation that Neel et al. either caused or exacerbated the epidemic.

* As with the Hornick et al. study, this article does not identify the vaccine used in this study as Edmonston B. A close examination of the derivation of the vaccine reveals that it may actually be Edmonston A or a close relative. Edmonston A was never licensed for use due to the somewhat greater reactions associated with this vaccine. We have not yet come to firm conclusions about the identity of the vaccine used in this study, however.

Reference 16: Brody et al. (1964), is the other study among Native Americans that was available prior to 1968. Here are the opening two sentences of this article:

> Two regimens of measles vaccination were tested in Alaskan Eskimo villages in March, 1963 [one of which was Edmonston B with gamma globulin]. *Clinical reactions to vaccines were no more severe than those observed in other populations*. (Brody et al. 1964, emphasis added)

Again, this introduction is hardly the indictment of the vaccine that Tierney and the New Yorker imply. Here is what the authors say about those individuals who reacted to the vaccine:

> Vaccinees with high fever were moderately ill and listless, *although the degree of illness was considerably less than that associated with true measles* (Brody et al. 1964, p. 341, emphasis added).

Once again, the vaccine reactions were not seen to be dangerous, and were viewed as far preferable to infection with the wild virus in a Native American population. Once again, Tierney fails to mention either of these facts. Here is the study's full conclusion (with inserted comments):

> Our studies indicate that response to measles vaccine among Eskimos was similar to responses encountered in other populations, in spite of the fact that clinical measles is apparently more dangerous for these people. It is difficult at this time to draw conclusions concerning the methods and combinations of vaccination most appropriate for remote areas such as those encountered in Alaska. Administering gamma globulin plus LV [live virus] has a great advantage in field work since it can be given in one visit [contrast with the three diluted doses program of Venezuela in 1968 noted by Tierney]. The major problem, however, is that the population is submitted to risk of febrile response greater than 103 F in 15% to 20% of vaccines 7 to 14 days following administration [modern vaccines typically cause a similar reaction in 5-15% of recipients]. It is unlikely that trained personnel could remain in villages for the length of time necessary to give vaccine and be available during the reaction phase [note that the concern is managing expected reaction fevers, not the inherent danger of the vaccine]. Hopefully, a safe and effective single dose vaccine such as that described by Schwarz will be available in the near future. At present, however, the relative freedom from reactions after a single dose of KV [killed virus] followed in several months by LV merits serious consideration for use in the isolated and inaccessible areas. [they did two trials, one with LV + GG, and one with KV and then LV six weeks later. In the former case, 18% of vaccinees had a temp of 103F; in the latter, only two cases had a temp as high as 102F] (Brody et al. 1964, emphasis added)

In sum, the two studies, which examined Edmonston B in Native American, measles-inexperienced populations, yielded little-to-no evidence that Edmonston B was inappropriate or dangerous in such populations; in fact they concluded that the vaccine reaction was far preferable to infection with the wild virus. These facts, clearly stated in Tierney's principle sources, contradict both his claims and those of the New Yorker, but Tierney fails to mention them.

Can the vaccine virus be transmitted?

If the Edmonston B vaccine caused the epidemic, then it must have done so by transmitting itself from a vaccinated individual to a susceptible contact. Despite repeated efforts to detect transmission of the vaccine virus (as opposed to the wild virus), no such cases are known to exist. Tierney's argument in Chapter 5 rests on the claim that Edmonston B vaccine virus could cause a measles epidemic, an extraordinary claim with no scientific support. However, in a crucial Chapter 5 passage, Tierney reports the results of an autopsy of a boy who died after being vaccinated with Edmonston B. The autopsy allegedly revealed that the vaccine virus had moved to the patient's respiratory tract, a portal from which it could infect others, and, we are led to believe, cause an epidemic. Here is the passage in full:

> I have found only one case of a person suffering from 'sub-clinical' measles, where it 'simmered' for months. This happened to a boy with leukemia who was inoculated with Edmonston B vaccine virus - not natural measles. The boy went 20 days without showing rash, than burst into a full body eruption that lasted weeks. When the skin lesions vanished, the disease did not. He died three months after vaccination, with Edmonston virus in his throat and conjunctivae. That meant not only that the vaccine virus killed him (his leukemia was in remission and did not return), but that it had moved to a portal - the respiratory tract - where he could infect others. John Enders of Harvard University, the creator of the Edmonston vaccine, conducted an autopsy. It revealed gaping inner wounds caused by the virus (Tierney p. 66).

Is the study cited by Tierney (Mitus et al. 1962) evidence that perhaps the vaccine virus could be transmitted, given that the Yanomamö were somehow uniquely vulnerable? (In other similar populations of Native Americans with little or no measles exposure, Edmonston B vaccine had had no such effects.) The leukemia patient (with a severely compromised immune system) indeed died three months after vaccination, and the vaccine virus may have killed him (the authors aren't sure). The authors do not say that the patient died "with Edmonston virus in his throat and conjunctivae" although they do say that 2 months prior to the patient's death, a virus with some characteristics of vaccine virus and some of measles was found in the throat and conjunctivae (Mitus et al. p. 417). Does this mean that he could have infected others? Tierney would like us to think so - but totally omits that the article's authors reach the opposite conclusion! The authors are interested in understanding the patient's illness, so they run several tests which, for most of the post-vaccination period, indicate an absence of measles. They then note another piece of evidence, *the virus' failure to infect other susceptible persons,* suggesting that they're dealing with vaccine virus and not measles:

> The serum of a susceptible sibling who was in contact with this patient, and who did not contract measles, was also tested. No antibodies were demonstrated. This result provides additional evidence that the infecting agent was the attenuated vaccine virus, since *it has been demonstrated that this agent does not pass readily to susceptible persons in contact with vaccinated individuals* (Mitus et al. 1962, p. 417, emphasis added).

In other words, this patient did not infect his susceptible sibling with measles, despite three months of intimate contact (the sibling had never had measles, and measles has an extremely high attack rate: over 90% of those exposed will become infected if they haven't had the disease or haven't been vaccinated). No wonder Tierney leaves this out. His apparent rule of thumb: "When the expert opinion directly contradicts your own, omit it."

Finally, why did Neel et al. use Edmonston B instead of the more attenuated Schwarz vaccine that was also available in 1968? After all, Francis Black decided to use the Schwarz vaccine among the Tiriyo, another Native South American population, and Black had noted high fever reactions to Edmonston B in other studies of the vaccine in Native American populations. First, Black was *not* warning against the use of Edmonston B. He was noting that the vaccine provides a model of the natural disease, and that the higher reactions of Native Americans to the vaccine in previous studies might indicate that these populations were genetically more susceptible to the wild virus. Black cites this information because he wants to conduct a controlled experiment using a very similar vaccine (Schwarz) to test this hypothesis! (One of the three previous studies he cites on high reactions actually used Schwarz.) In fact, the average fever reaction that Schwarz vaccine caused among the Tiriyo during Blacks' vaccine experiment was actually greater than the average fever reaction the Edmonston B vaccine with gamma globulin caused among the Yanomamö! Second, Black was an expert at conducting vaccine trials and experiments, and Neel was not. Neel's expertise lay elsewhere, and he merely wanted to provide vaccine to the Yanomamö for humanitarian reasons (more on this below). That's why he and his colleagues consulted with both Black and the CDC on the use of the vaccine before entering the field. Third, WHO studies in measles-experienced populations had found that Edmonston B with gamma globulin caused exactly the same average fever reaction as did Schwarz (0.43 C in each case), and Neel et al. used Edmonston B with gamma globulin almost exclusively.

We still don't know why Neel et al. chose Edmonston B with gamma globulin over the very similar Schwarz vaccine, but a letter of Neel's indicates that he was able to obtain Edmonston B free. Perhaps drug companies were willing to donate the older Edmonston B vaccine that was being phased out, but were not willing to donate the newer Schwarz vaccine. However, Edmonston B was still a very widely used vaccine: over one million US children were vaccinated with it in 1968.

In sum, Tierney has seriously and deliberately misrepresented each of the key sources underlying his insinuation that Neel et al. either caused or exacerbated the 1968 Yanomamö epidemic. He has wrongly claimed that experts were concerned about the use of Edmonston B among Native American populations like the Yanomamö prior to 1968, even though he knew that they had in fact endorsed its use; he has failed to mention that studies that actually used Edmonston B among these groups found that the reactions were similar to those in other populations and should not preclude the use of the vaccine, even though he knew this to be true; he has failed to mention that the vaccine worked well to prevent infection with the much more dangerous wild virus in these groups, even though he knew this to be true; he wrongly suggests that reactions to the vaccine are comparable to the serious complications of the wild virus, even though he knows this to be false; and he wrongly implies that the vaccine virus could be transmitted, even though he knew that there was substantial evidence against this and even though he knows that the one study he does discusses on this issue came to the opposite conclusion.

Neel's views and ideas, part I

An essential ingredient in any conspiracy theory is a motive. In his attempt to argue that Neel and Chagnon had a theoretical motive to administer a 'dangerous' vaccine, Tierney distorts the views and ideas of James Neel beyond recognition. He also inaccurately conflates Neel's own theories with other mainstream views that Neel also held. Finally, he awkwardly (and again inaccurately) attempts to link Neel's views with Chagnon's views of violence in non-state societies like the Yanomamö. In this section, we show how Tierney misrepresents Neel's own theories on the evolution of human intelligence. In the next section, we show how Tierney mistakenly presents Neel's mainstream views as eccentric. Tierney distorts Neel's views in order to convince his readers that Neel had a motive for subjecting the Yanomamö to a vaccine experiment. In fact, Neel's own theories about the evolution of human intelligence could not be tested, even in principle, with a vaccine experiment. Neel's views on Native American susceptibility to measles epidemics, on the other hand, were entirely mainstream, and were prominently endorsed by Neel and many others in an effort to stimulate the maximum medical response possible to epidemics in what are commonly referred to as 'virgin-soil' populations (populations with little or no exposure to 'herd' diseases like measles).

If Chagnon is Tierney's Darth Vader, then James Neel, a prominent geneticist, is his Evil Emperor:

> Chagnon was actually the advance man for a new order of scientific adventure, the most comprehensive study of a tribal society ever undertaken. This project was conceived by James Neel, a doctor who helped found the modern science of human genetics....Neel is probably the only geneticist of his reputation in the post-Nuremberg world to praise the early eugenicists for their 'concern for the future' of the gene pool. He has also criticized other scientists for spurning the 'eugenic label' and refusing to take strong political stands designed to improve the gene pool." (Tierney p. 37-38).

Early on in Darkness in El Dorado, Tierney identifies Neel as the mastermind of a series of nefarious experiments with the Yanomamö as subjects, designed to test his 'quirky ideas':

> Neel believed that modern society was going soft. From the Amazon's unspoiled inheritance, Neel hoped to find a genetic basis for male dominance–'the Index of Innate Ability'–a kind of elixir to the gene pool. It was Neel who selected the Yanomami as experimental subjects and sent Chagnon to find evidence for his quixotic theory. (Tierney p. 12, citing Neel 1980)

The latter sentence is critical. In this chapter Tierney invokes the Atomic Energy Commission, the atomic bomb, Japanese bomb victims, radiation, and blood in an attempt to cast the vaccination program of Neel's, discussed in the next chapter, in a sinister light. Tierney virtually accuses Neel of deliberately subjecting the Yanomamö to severe and potentially deadly symptoms in order to test his scientific theories:

> The choice of vaccine was particularly odd because administering the Edmonston virus required twice as much work as administering any of the safer strains (because of the extra shot of gamma globulin). Yet, in spite of the risks to the Yanomami and the inconvenience to his own medical team, Neel requested the Edmonston vaccine from Parke Davis Laboratories, Philips Roxane, and Lederle, none of whom manufactured the more attenuated measles vaccine viruses.
>
> Why did Neel do it?
>
> Although I can only speculate about Neel's personal motives, opting for the Edmonston vaccine was a bold decision from a research perspective. Obviously, the Edmonston B, precisely because it was primitive, provided a model much closer to real measles than other, safer vaccines in the attempt to resolve the great genetic question of selective adaptation. (Tierney p. 59) [see the appendix for statements from independent experts that Edmonston B was a safe and proper vaccine for use with the Yanomamö]

That Neel et al. were not conducting an experiment with measles vaccine is clear from this entry from Neel's field log, written almost two weeks before the major outbreak of measles at Ocamo on February 17:

> 5 February 1968
>
> The measles vaccination - a gesture of altruism and conscience - is more of a headache than bargain for [sic]- I would either put this in the hands of the missionaries or place it at the very last. (Neel field log)

There is no hint in the log of a vaccine experiment (and there are many medically sound reasons for such experiments that Tierney fails to discuss–see below).

If Tierney is going to imply that Neel conducted criminal experiments to test his theories, then he has an obligation to accurately represent those theories. This he fails to do. For example, Tierney claims:

> Neel hoped to prove that the Yanomami 'population structure' was the one dictated by natural selection: a society dominated by aggressive, polygamous chiefs, where very few people reached the age of fifty. His core belief was that modern society's gene pool problems arose 'primarily from abandoning the population structure and the selective pressures under which humankind evolved.' (Tierney p. 49, citing Neel 1994)

Before plunging into Neel's theories in depth, it is interesting to see what Neel actually says about 'aggressive, polygamous chiefs':

> A description of the attributes of a headman by someone from so different a culture as our own involves considerable projection. It is easier to define what he is not that what he is. From my perception, among other tribes as well as the Yanomama, buttressed by the anthropological literature, he will not be a poor hunter, one deficient in speaking abilities or one deficient in knowledge of tribal lore, nor will he have been cowardly or inept in his participation in the frequent raids on other villages. While physical strength is an asset, I suggest that mental agility is even more important: he will not be stupid. *Simple aggressiveness will not be a sufficient quality for headmanship: there are too many ways that aggressiveness divorced from judgement can lead to an early demise in the jungle*. (Neel 1980, emphasis added)

Neel closely echoes these views in his autobiography, Physician to the Gene Pool:

> Headman–not just among the Yanomama but probably in all tribal cultures–emerge by a combination of attributes. They are well versed in tribal history and lore, and, since Amerindian cultures operate largely by consensus, must be superior and persuasive speakers. They must acquit themselves well in battle, and be skillful hunters. The intimacy of life in an Indian village is such that there can be none of the discrepancies between public image and private conduct with which political leaders in the United States and elsewhere so regularly surprise us, nor can there be a delay of 20 or 30 years in recognizing the consequences of a hideous misjudgment on the part of a leader. Everything anyone in such a village has ever done is known to all the other members of the village. Dummies don't become headmen. (Neel 1994, p. 186)

We are truly perplexed why Tierney repeatedly states that Neel had a theoretical interest in 'aggressive' headmen, when it is clear that Neel was interested in intelligent headmen. We have yet to find a single instance of Neel characterizing headmen as 'aggressive'. This appears to be Tierney's clumsy attempt to link the theoretical interests of Neel with those of Chagnon. (Tierney's crude misrepresentation of Chagnon's views will be addressed below.)

Tierney also claims about Neel:

> **While almost everyone applauded the democratic freedoms that allowed women to choose their own mates, Neel glumly concluded that the** 'loss of headmanship as a feature of our culture, as well as the weakening of other vehicles of natural selection, is clearly a minus.' (Tierney p. 49, citing Neel 1980)

The words in bold are Tierney's. Again, it is interesting to compare this with what Neel actually says. Well, nowhere in the cited article does Neel ever discuss anything about women being able, or not being able, to choose their own mates. The above quote of Neel's (the non-bolded text) occurs on page 289 in a discussion of the increased mutational load that might result from the loss of a 'primitive' population structure. (Tierney also misconstrues Neel's tone in the 'loss of headmanship' quote above: Neel wasn't glum, he was joking.)

The closest Neel comes to a discussion of mating is the following quote (which comes five pages before the 'loss of headmanship' quote extracted by Tierney above):

> Most Amerindian tribes, and primitive man in general, were polygynous. Primarily because of preferential female infanticide, males substantially outnumber females until the third decade. Since marriage occurs at an early age, obtaining a wife under these circumstances is a particularly serious business, involving complicated negotiations. The extent to which headmen might excel in negotiations leading to polygyny became evident in the very first village of Amerindians among whom I worked, in which we encountered a Xavante headman who at the time of our study had thus far been married five times and already had 23 surviving children. (Neel 1980, p. 283).

This is an entirely standard view of marriage in a polygynous, small scale society, and one which most anthropologists would endorse; it obviously has nothing to do with 'democratic freedoms' or the lack thereof. Tierney just made that up.

So, what are Neel's theories and views regarding headmen? Tierney's claims about Neel's views appear to derive from six sources:

1. Neel's autobiography, Physician to the Gene Pool, Wiley 1994.
2. A journal article: On Being Headman, Perspectives in Biology and Medicine, 1980, 23:277-294.
3. A journal article on the 1968 measles epidemic.
4. A film (Yanomamö: A Multidisciplinary Study. Neel is apparently the narrator).
5. An interview with Terence Turner.
6. An interview with Neel.

Because Neel's article on the measles epidemic does not discuss his views on leadership or headmen, Tierney merely had to master two written sources on Neel's ideas: the journal article 'On Being Headman' and the autobiography 'Physician to the Gene Pool' (Tierney cites and quotes heavily from both). Despite an alleged eleven years of research on his book, Tierney fails completely in his attempts to explain the relatively simple ideas of Neel, a key villain in his tale. Entirely obscured is Neel's central focus: the evolution of human intelligence.

We found it impossible to reproduce Tierney's argument on Neel's work; we suspect there really isn't one. But he does manage to slip in Terence Turner's interpretation of Neel's theories, an interpretation based on a snippet of conversation Turner supposedly overheard more than thirty years ago. Terence Turner claims, in an interview with Tierney in 1995, that he recalls Neel saying during a meeting in 1963 "Good. Now we'll have a chance to find the leadership gene." (Tierney, p. 39)

> [A]lthough he never used the phrase 'leadership gene' in his writings, [Neel] proposed a genetic 'Index of Innate Ability." Neel believed that this Index of Innate Ability (IIA), located at paired

> alleles along the DNA chain, became concentrated in the offspring of dominant, polygynous chiefs, *just as Turner recalled*. (Tierney p. 40, citing Neel 1980, emphasis added)

Turner's recollections notwithstanding, Neel's 'Index of Innate Ability' actually refers to intelligence, as any reader of Neel's work could not possibly fail to comprehend. We demonstrate this next.

Analysis Of 'On Being Headman'

Tierney mangles Neel's argument in 'On Being Headman', one of his *principle* sources on Neel's views. Whether this mangling is deliberate or merely reflects Tierney's inability to understand what Neel is saying, is not clear. Neel is making an argument using the theory of sexual selection, a standard theory in biology. Neel suggests that sexual selection for superior cognitive abilities (not disease resistance) may have driven human evolution for the last several million years, explaining the explosive growth in human cranial capacity during this period[*]. Neel argues that among the Yanomamö and other Amerindians, 1) headmen achieve their position largely on the basis of their 'mental agility', 2) that a significant component of this mental agility may be heritable (the Index of Innate Ability), 3) that headman have significantly more children (and, based on a computer simulation, grandchildren) than other men, 4) that these dynamics suggest a strong selection pressure for cognitive abilities, and 5) that headmen may have been an important feature of human societies over evolutionary time.

That Neel's 'Index of Innate Ability' refers to cognitive abilities is clear:

> While physical strength is an asset, I suggest that mental agility is even more important: [the headman] will not be stupid. (Neel 1980, p. 283)

> The possible genetic implications of headmanship are obvious. Let us consider that we have at our disposal an Index of Innate Ability (IIA), *which some will be tempted to equate to intelligence*. It is a quantitative trait *certainly related to intelligence*, based on the additive effects of alleles at many loci, but since the quality which we call intelligence has been validated only as a predictor of school performance, we best not allow ourselves to be ensnared by that word. Let us assume that the average Index within a village which contains 50 reproducing adults is 100, but that the headman has an Index of 120, in which case his 49 peers will average 99.6. We will assume that in this egalitarian society where the educational opportunities are remarkably uniform, the Index really measures an innate difference. (Neel 1980, p. 285-6, emphasis added).

Neel then goes on to note that if headmen have twice as many children as other men (and for the Yanomamö he demonstrates that there is good evidence for this), "the potential this population structure offers for positive selection for the IIA seems incontrovertible."

That Neel intends this argument to illuminate the *evolution* of human intelligence is also clear:

> No one has yet developed, let alone applied, the kind of test procedures which could be used to determine whether and to what extent the headman really is characterized by a high IIA. *In any effort to understand the driving forces of human evolution*, I regard the provision of such data as

[*] Interestingly, Neel's views, on the evolution of human intelligence appear to closely parallel those of Geoffrey Miller, put forth in Miller's recent book: The Mating Mind: How Sexual Choice Shaped the Evolution of Human Nature (New York: Doubleday, 2000). This book has been widely reviewed, often favorably, including a friendly interview with Miller by Natalie Angier for the New York Times.

> the number one objective. The gains in IIA predicted by the model must of course have been partially but not entirely offset by the losses imposed by the operation of chance and erosion through mutation, as discussed earlier. I say not 'entirely' on the basis of the fossil evidence for increasing cranial capacity, which must bear some relationship to IIA. Thus if we could get a fix on the IIA of the headman in the few remaining cultures where the institution persists, we would have an important insight into the intensity of the positive selection for IIA necessary to offset the countervectors of mutational erosion and chance and still permit the evolution of IIA we presume to have occurred. (Neel 1980, emphasis added)

It is hard to see how any experiment involving Yanomamö susceptibility to measles or measles vaccine, as is insinuated to have happened during the 1968 epidemic by Tierney (as well as by Turner & Sponsel), would test any part of Neel's theory about the evolution of human intelligence. Nowhere does Neel link his Index of Innate Ability to disease resistance. In fact, Neel makes an explicit distinction between selection pressures that would maintain disease resistance (primarily infant mortality and selective infanticide of congenitally malformed newborns), and those that might be involved in the evolution of uniquely human attributes like intelligence (i.e., the differential reproduction of smart headmen):

> It is tempting to view selection exercised through prereproductive mortality as primarily 'housekeeping' in nature, directed toward the maintenance of disease resistance and metabolic integrity, whereas that exercised through differential fertility was more directed toward the evolving specifically human attributes. (Neel 1980 p. 288-89).

As a separate but related issue, it is important to note that Neel does use the term 'eugenic' frequently and in a positive vein. However, it is crystal clear that he is using the term to refer to limiting or decreasing the frequency of deleterious mutations in modern populations by decreasing the transmission of genetic diseases and by reducing exposure to environmental mutagens; he is not using the term to refer to breeding 'superior' individuals. It is best to let Neel speak for himself:

> I believe we will agree that there is scant prospect of our engineering an early return to Yanomama population structure–small demes, living of course in twentieth-century comfort, in which a generally acknowledged headman of superior attributes enjoys a well-defined reproductive advantage. Since there is little prospect society will ask us to remake it with these or other extensive eugenic measures, there really are available only two practical (i.e., socially acceptable) courses of eugenic action for the immediate future. The first is an increasing concern with the provision of genetic services designed to decrease the transmission of genes causing disease, especially genetic counseling coupled where indicated with prenatal diagnosis and early abortion. The second eugenic measure which geneticists can facilitate is a concern with measures which influence human mutation rates. We are all very aware of the need to minimize human exposure to environmental mutagens and the necessity of careful cost-benefit analyses insofar as these are possible when some exposure seems inevitable in our industrialized society. Beyond this, however, it is now becoming apparent that there may be a more active role for the geneticist than simple protection of the public against unjustifiable exposures to mutagens. One of the very significant developments of the past decade has been the realization of the extent of the cellular potentiality for the editing and repair of lesions in DNA, by a variety of mechanisms...." (Neel 1980, p. 290). [Neel goes on to speculate that we may be able to improve genetic repair mechanisms and thus significantly lower mutation rates. See Kevles 1995 for an account of Neel's *rescue* of human genetics from the eugenicists. See also http://www.egroups.com/message/evolutionary-psychology/8370]

And Neel's concluding paragraph:

> A variety of recent spectacular developments has prompted widespread speculation concerning the potentiality for improving the human condition, not only through the counseling and related services mentioned earlier, but also through 'genetic engineering' *sensu stricto*, that is, involving the germ line. It would be unfortunate if in the surge of enthusiasm for these new discoveries, insufficient attention was directed not only toward minimizing human exposures to mutagens but also toward the possibility of influencing genetic repair mechanisms for the better. These latter developments probably hold greater and much less controversial promise of protecting man's genetic endowment than the former." (italics in the original)

In sum, Neel argues in 'On Being Headman' that the evolution of human intelligence may have been driven, in part, by the differential reproduction of smart headman over the course of human history. He also argues that the relaxation of the intense selection pressures humans were exposed to in ancestral environments such as high rates of polygyny and child mortality may result in increasing degrees of mutational load in modern human populations. He suggests that the latter problem may be addressed by identifying and preventing the transmission of genetic diseases, by minimizing exposure to environmental mutagens, and by improving, if possible, human genetic repair mechanisms. Tierney fails to convey any of these straightforward ideas in the slightest degree, although that doesn't stop him from implying that these ideas motivated Neel to conduct criminal experiments on the Yanomamö.

For more on Tierney's treatment of Neel in Chapter 4, see:

The National Academy of Sciences statement:
http://national-academies.org/nas/eldorado
SLAA commentary on Neel (issue # 17 & 18):
http://www.egroups.com/message/evolutionary-psychology/8370
International Genetic Epidemiology Society:
http://hydra.usc.edu/iges/Darkness/Darkness.html

Neel's views and ideas, part II

Tierney starts off 'Chapter 5: Outbreak' with a quote from a journal article authored by four individuals, Neel, Centerwall, Chagnon, and Casey (Neel is the first author). This quote is meant to inform the reader of the 'dangers' of the measles vaccine used by Neel et al.:

> The reaction to measles vaccine without gamma globulin had been, in some cases, as severe as the disease itself among Caucasian children. (Neel et al. 1970, p. 425)

What Tierney doesn't mention is that among Caucasian children, the 'disease itself' is usually not very severe. Attempting to compare the relatively mild Caucasian reactions to the vaccine to indigenous populations' reactions to the wild virus is absurd, as is made clear in the very next sentence of Neel et al. (not quoted by Tierney):

> When the epidemic of measles [and NOT merely reactions to the vaccine] struck the Indian populations, however, there was no doubt that it was a different entity of far greater severity in terms of prostration, toxicity and complications. (Neel et al. 1970, p. 425)

More importantly, Tierney claims that:

> Equally striking was the fact that scientists had been competing worldwide to observe measles in a "virgin soil" population....Because measles attacked everywhere with such predictable ferocity that geneticists expected that a measles contagion in an Amerindian tribe could allow them to measure the difference in inherited immunity between New and Old World people–a key factor in natural selection. (Tierney, p. 54)

This provocative statement has no supporting documentation whatsoever. Because it was widely known prior to the 1968 Yanomamö outbreak that a measles epidemic in a previously unexposed population would likely result in mortality rates exceeding 20% of the population, what Tierney is claiming in the previous two sentences is that scientists hoped to observe death on a massive scale in order to test what Tierney asserts is 'a key factor in natural selection.' Some support for such a claim would seem to be in order. Tierney provides none. These statements are critical for Tierney because, if true, they would provide a possible (although still extremely unlikely) motive for Neel et al. to administer a supposedly 'contraindicated' vaccine to the Yanomamö in order to observe its effects. If false, there is no motive at all.

Tierney still attempts to portray Neel as espousing eccentric scientific views, views that would supposedly lead Neel to use a 'dangerous' vaccine in a vulnerable group:

> And, despite all the evidence to the contrary, Neel simply did not believe the "the medical dogma that the isolated tribal populations...have a special inborn susceptibility" to diseases like measles. The consensus of scientists is that tens of millions of American Indians, from the Mississippi valley to Tierra del Fuego, died of "Old World germs to which Indians had never been exposed, and against which they therefore had neither immune nor genetic resistance." This conclusion, from UCLA's professor of physiology Jared Diamond, has been echoed by thousands of observers.
>
> But James Neel disagreed. He believed the Yanomami were models of good health." (Tierney, p. 59)

Most readers of the foregoing would probably infer that Neel and colleagues did not believe that a measles epidemic among the Yanomamö would be devastating. However, the very first sentence of their published report on the epidemic states the opposite:

> The impact of measles on a primitive population is well known. (Neel et al. 1970)

This is why Neel and colleagues, upon determining that the Yanomamö had, with few exceptions, not been exposed to measles, made plans to vaccinate as many as possible on their next trip to the field:

> In view of this demonstrated susceptibility of the tribe to measles, the plans for the third expedition to the Upper Orinoco in 1968 included steps to obtain 2000 doses of Edmonston strain measles vaccine, with the intention of vaccinating as many Indians as possible towards the end of the expedition's more scientific objectives. (Neel et al. 1970)

After reading the Neel et al. report on the epidemic, it is clear that the legitimate point of contention is not whether isolated and previously unexposed groups were particularly vulnerable, but why. To this day, no one really knows. What Neel et al. questioned was whether isolated groups' demonstrated vulnerability to 'herd' diseases like measles was due to genetic factors. They instead argued that the incontrovertible vulnerability of these groups was mainly due to social factors. If no one in a village has had measles, for example, then, upon exposure, everyone gets sick, including all the adults, leading to a complete collapse in village life. With everyone sick, there is no one to care for the ill, resulting in far more deaths than would otherwise be the case:

> In addition, with large groups, or even total villages ill with measles, there was a total collapse of village life. The concern of the well Indian for the ill seldom extends outside the immediate family. A febrile person dehydrates rapidly in the tropics. Mothers could not nurse their babies; these Indian children are usually dependent on breast milk for the majority of their diet until about the age of three. Finally, the Indian attitude to measles can best be described as appearing to retire to his hammock where, in a jack-knife position, he rouses only occasionally to expectorate feebly, while awaiting death. (Neel et al. 1970).

Were Neel's views on this distinctly different question eccentric or without basis? More importantly, was he willing to use extreme methods to test his ideas? Turner and Sponsel, in their original email on the forthcoming book by Patrick Tierney, paint a grim portrait of Neel's methods:

> Medical experts, when informed that Neel and his group used the vaccine in question on the Yanomami, typically refuse to believe it at first, then say that it is incredible that they could have done it, and are at a loss to explain why they would have chosen such an inappropriate and dangerous vaccine (Turner and Sponsel, original email to Lamphere & Brenneis).

Turner and Sponsel exaggerated somewhat (and this is not the only such instance): Tierney actually only refers to one expert, Francis Black,

> When I told Francis Black that James Neel had administered the Edmonston B vaccine to the Yanomami in 1968, he did not believe me. 'That happened around 1964' he corrected me. 'It would have been contraindicated any time after about 1967. (Tierney, p. 58)

Several medical experts have, of course, already stated that Edmonston B was an entirely appropriate vaccine to use with the Yanomamö (including both experts cited by Tierney on this matter). So the question becomes, how did Tierney come to the conclusions he did in his manuscript? How did he come to believe that Neel, Chagnon, and others were actually exposing the Yanomamö to a dangerous vaccine in order to conduct an experiment to test an eccentric theory? How did he come to believe that there was some great issue in 'natural selection' that such an experiment would address?

Having read all the works of Neel's cited by Tierney, we were perplexed. Neel's theories about small scale indigenous societies like the Yanomamö mostly involved the evolution of intelligence, not disease resistance, and we couldn't see how an experiment with measles vaccine would even address Neel's or Chagnon's main theoretical interests in this group. We had been awaiting Francis Black's article from interlibrary loan, because Tierney had positioned him as an implicit and explicit critic of Neel, and Black is indeed a major figure in measles research. Neel et al. had argued, in their 1970 article on the epidemic, that the severity of measles in 'virgin-soil' (previously unexposed) populations was due primarily to social factors, not innate differences between populations. Neel's supposedly fringe idea was that social factors outweighed genetic factors in measles epidemics.

Tierney appears to cite Black, the only independent researcher interviewed who has used measles vaccine in a Native American population, to back up his insinuation that Neel's views were out of the mainstream:

> By 1965, the intense measles-vaccine reactions seen among Amerindians had gone a long way toward confirming the theory that Native Americans were more susceptible to Eurasian epidemics. Francis Black, a medical researcher at Yale, was keenly involved in these studies. [Tierney goes on to report Black's surprise at Neel's use of Edmonston B.] (Tierney, p. 57)

So, according to Tierney, Neel's social hypothesis (which Tierney neglects to explain) is heterodoxy, and Black's genetic hypothesis is orthodoxy; not only that, Neel is apparently willing to conduct dangerous experiments in an attempt to prove what Tierney terms his "quirky" theories. When Black's article finally arrived from interlibrary loan, we discovered the inspiration for Tierney's speculations about Neel: it was Black who has administered live measles vaccine to a previously unexposed population as an experiment (which Tierney briefly notes), and it was Black who gave vaccine to half the population, not vaccinating the other half in order to keep them as a control group. Surprisingly, we learned from Black, author of one of the definite works on the measles virus, that Neel's social hypothesis was the majority, orthodox view (at least among epidemiologists in 1971) and the genetic hypothesis was the minority view on Native American susceptibility to measles–completely the opposite of what Tierney said (or what he appeared to be saying. For whatever reason, Tierney's explanations of scientific theories are quite poor). According to a review article by Black et al. (1971), the social hypothesis advocated by Neel had been recognized and discussed for nearly a hundred years, and was widely accepted:

> the epidemics [in the South Pacific in the 19th century] have relevance because, for the first time, epidemiologists became aware of the role that disruption of simple services and lack of elementary nursing care played in virgin-soil epidemics. This became a much discussed topic in the medical journals of the late 1870' s and early 1880's. *The proponents of nongenetic explanations for the high mortality rates seem to have won the day, but nevertheless, the unsubstantiated assumption that the difference was racial continued in both popular and medical literature*. (Black et al. 1971, emphasis added)

It is not the purpose of this report to engage in the debate over the reasons for Native American susceptibility to measles epidemics (and Neel clearly expressed sympathy for both views in his log). We only care to point out that Tierney's characterization of Neel's views as eccentric are false, and this information is clearly stated in material Tierney cites. Black et al. go on to examine whether there might be a genetic component as well, but conclude, contra Tierney's claims, that "the influence of hereditary factors on the reaction of American Indians to measles cannot be determined adequately from presently available information."

According to Black et al., Neel's views were obviously mainstream among experts and can by no stretch of the imagination be portrayed as fringe or eccentric. Rather, it is the competing view of genetic susceptibility that is difficult to sustain.

But could it still be true that Neel's methods were extreme? We've seen that Black used the same methods that Neel is accused of (but there is no evidence that Neel ever did any of the things that Black did). Why, then, was Black so shocked by Neel et al.'s use of the 'dangerous' Edmonston B vaccine that he, in a conversation with Tierney in 1997, at first refused to believe it? We don't know, but it is especially hard to explain in light of the following: Black devoted a significant portion of his review article to the 1968 Yanomamö epidemic, including the use of Edmonston B both with and without gamma globulin. On pages 312 and 313 and in table 4 of the 1971 article, Black et al. review Neel's data on use of Edmonston B among the Yanomamö in detail, comparing it with data from a number of other studies. No criticism of Neel et al.'s use of Edmonston B is made. And there is no confusion that the epidemic happened in 1964 (as

Tierney's quote of Black seems to suggest); the 1968 date is clearly noted in a subheading. We also have recently learned that the Neel team consulted with Black about the dosage of gamma globulin to use with Edmonston B, shortly before leaving for Venezuela in January 1968 (documented in a Dec. 1967 letter from Centerwall, one of the authors of the 1970 Neel et al. article on the epidemic, to Black. Standard doses of gamma globulin were available for children, the only recipients of measles vaccine in measles experienced populations, but these doses needed to be adjusted for adults who were receiving the vaccine in measles-inexperienced populations, and Black was consulted about this). Black was an expert in vaccine studies, but, so far as we can tell, Neel was not. He was a geneticist, and vaccination programs were (we think) well outside his specialty. Neel cites no previous publications of his on this subject in his article on the epidemic, nor have we found any so far. He appears to have been merely concerned with providing measles vaccine to inhabitants and missionaries of the Upper Orinoco.

In sum:

1) Neel and colleagues were merely echoing a mainstream view about Native American susceptibility to measles, according to Tierney's own expert on this issue. Tierney strongly implies the opposite. Furthermore, Neel's views on the evolution of intelligence had little to do with his views on Native American disease resistance, despite Tierney's concerted attempts to link them.

2) The alternative view, that Native American susceptibility was due to genetic factors, was pursued by Tierney's expert, Francis Black, though even he admitted the evidence for this view was far from conclusive.

3) Neel didn't conduct an experiment using measles vaccine, but Black did. Neel et al. had no theoretical motive for conducting a vaccine experiment.

4) Tierney claims Black was shocked to learn, in 1997, of Neel's use of Edmonston B to quell a measles epidemic, but Black provided information on the appropriate dose of gamma globulin to provide with Edmonston B to the Neel team before they left for the field in early 1968. Black also discussed Neel's use of Edmonston B extensively in a journal article in 1971.

The foregoing raises an interesting question. Was Black's measles vaccine experiment ethical? We leave that question to experts, but we see no obvious reason why not. As Black notes, if the social theory is correct, then "much of the mortality reported in the past was preventable and not inherent in the genetic constitution of the people involved." Neel et al. make essentially the same point at the conclusion of their report on the Yanomamö epidemic: "This point of view [the social hypothesis] also suggests that there is no theoretical basis for accepting less than optimal results in the management of these diseases in newly contacted groups." Both Black and Neel appear to be dedicated physicians who had a genuine interest in understanding the true nature of epidemics in vulnerable populations in order to better manage future outbreaks, including outbreaks among other populations of Yanomamö. Determining whether high measles mortality in unexposed populations was due to social or genetic factors would have very important implications for managing such epidemics. As Black carefully explains, experiments with safe vaccines, if conducted according to ethical guidelines, were an excellent means towards this end. This is standard procedure today. If drugs, including vaccines, are going to be marketed, experiments using control groups, etc., are *required* by the FDA, including experiments in vulnerable populations (e.g., populations at risk for HIV).

How did measles arrive at Mission Ocamo, the center of the epidemic?

In attempting to pin the cause of the measles epidemic on Neel et al., Tierney tries to convince the reader that the only possible source of measles at Ocamo, the center of the epidemic, was Neel et al. To do this, he needs to eliminate from consideration all other possible sources of the disease. These other possible sources include the Brazilian visitors identified by Neel et al., and unknown visitors from other disease centers on the Upper Orinoco river near Ocamo. We know that Robert Shaylor, a Protestant missionary, expressed concern about measles on the Upper Orinoco in the Fall of 1967. We know from Neel's log that the Commissioner for Indian Affairs, Mr. Romero, asked Neel when he arrived in Caracas in January 1968 whether he would be able to respond to a measles epidemic on the Upper Orinoco:

> But more important, Eddie Romero "Commissioner for Indian Affairs" was present, and news of measles in the lower Ventuari [a tributary to the Orinoco downriver from Yanomamö territory] and Yonomoma [sic] and Maks [Ye'kwana] in the upper V., and what could we do about it. Discussion: Invite them in also.

Neel and colleagues subsequently spent many days in villages on the Ventuari river vaccinating individuals against measles before heading upriver to Ocamo. We also know from Napoleon Chagnon's field notes that a Yanomamö boy died of measles at Tamatama (another village on the Upper Orinoco near Ocamo) right about the time that measles appeared at Ocamo (see below). Thus, measles appears to have been present on the Upper Orinoco during the period in question, which was also the height of the dry season. This means that people are traveling widely, visiting friends and relatives in the area–ideal conditions for spreading measles. Measles is an extraordinarily contagious disease. Measles anywhere on the Upper Orinoco during this time effectively meant measles everywhere.

Neel et al. tentatively identified a Brazilian visitor as the initial source of the disease:

> Measles was introduced to the Yanomama of the Upper Orinoco by a party of Brazilians from the Rio Negro region who had come up the Orinoco to the Salesian Mission of Santa Maria del Ocamo. On January 22, 1968, a tentative diagnosis of measles was made for one of them, a 14-year-old male, by Dr. Marcel Roche, a physician temporarily engaged in research at the Mission. The boy remained prostrate for a week with a fever often reaching 40 C (axillary); his case was complicated by bronchopneumonia. The characteristic rash never developed, so that the differential diagnosis from any of a variety of "jungle fevers" was uncertain, but nevertheless 40 Indians and Brazilians at the Mission were vaccinated at once with no gamma globulin coverage. Fifteen days later, a second Brazilian, age 21, and an Indian, age about 30, developed a similar illness, characterized by intermittent fever to 40 C (axillary) for four to five days, stupor, conjunctival injection and extreme prostration. Both were seen by the authors; again, the rash was minimal, and the diagnosis of measles uncertain. Both Brazilians were typical "caboclos," probably of mixed Indian, Negro, and Caucasian ancestry. In the acute stages of the disease they were as ill as any Indian seen subsequently. Thereafter, the disease spread rapidly. (Neel et al. 1970)

If Tierney wishes to insinuate that Neel et al. *caused* the epidemic, he needs to establish that the Brazilian visitors to Ocamo could not have had any exposure to measles before arriving at Ocamo mission on the Upper Orinoco, and thus could not have been the source. This requires Tierney to go to some lengths to describe the isolation of these Brazilians both at their original outpost, as well as on their journey to Ocamo:

> The Brazilians had been summoned to the Ocamo airstrip from a frontier outpost, San Carlos del Rio Negro, where fewer than a hundred people lived. There was no measles outbreak at San Carlos while the Brazilians were there [no citation]; none had been there for many years [no citation]. It was the most isolated spot on the Venezuelan map, connected to the Orinoco and rest of the country only through the Casiquiare Canal, la monstruosite en geographie, which had given Humboldt the most painful passage of his career. In 1968, not a single person lived along the banks of this treacherous, insect-plagued waterway. The Brazilians navigated for a week through the 227-mile-long Casiquiare with a tiny outboard motor, traversing uninhabited wilderness. How could they have picked up measles en route to Ocamo? (Tierney, p. 65)

To answer Tierney's question, note that the Casiquiare intersects the Orinoco at Tamatama, well below Ocamo (as Tierney knows full well. See map):

There were many villages and outposts on the Orinoco between the Casiquiare and Ocamo, including Tamatama--a mission and Ye'kwana village located right at the confluence; La

Esmeralda, another mission and village with a large airstrip located several miles upstream; and Koshirowä-teri, another mission and village off the Orinoco on the Padamo. After navigating the long Casiquiare and thus being confined to a small boat for many days, it is virtually certain that the Brazilians stopped at Tamatama. It is also virtually certain that they stopped at Esmeralda, perhaps their last opportunity to refuel, resupply, etc., before heading to Ocamo (since Kosh would have required a small detour up the Padamo). Despite his intimate familiarity with the region, Tierney neglects to inform the reader of the many opportunities the Brazilians had to be exposed to measles on their trip upriver. In fact, his description of their isolation at San Carlos only supports the idea that they may have picked up measles on their journey, since, not having been previously exposed, they would have been susceptible to infection. Tierney quotes Neel as speculating in an interview that measles simmered subclinically in the Brazilians, but they easily could have picked it up on the Orinoco.

The fact that the Brazilians almost certainly stopped at Tamatama in mid to late January is an important fact in this tragedy, since a Yanomamö boy (aged 17) died of measles at Tamatama just prior to the start of the epidemic at Ocamo. Here are the relevant sections of Chagnon's 1968 field notes on this topic:

> 1/31/68
>
> Arrived back at Mavaca from Reyaboböwei-teri about 2:30 - 3:00. Danny Shaylor [a missionary at Tamatama] was not here yet---he will arrive tomorrow. He got involved in taking the remains of a dead Yanomamö back to Koshirowä-teri---a child (a boy of 17) from there died at Tamatama of measles and to prevent the spread of the epidemic he went with the body (ashes) himself rather than let the family carry it back and bring measles to Koshirowä-teri.
>
> 4/13/68
>
> Apparently the New Tribes group told the S.A.S. [S.A.S. is something like "Sanidad y Asistence Social", an official government agency, within which the Venezuelan Malarialogía resides, if memory serves correctly] that measles started with the Catholic Missions. Padre Cocco [of Ocamo mission] was advised of this by the S.A.S. yesterday (12th) and was furious. He then questioned me on dates etc. and pointed out that a Koshirowä-teri boy died of measles in Tamatama about the time we arrived around Jan. 22nd. Yet he was not interested in fixing the blame on anyone over the origin of measles; he merely wanted to get the record straight so as to not have his Mission unjustly accused of "starting" an epidemic.

Thus, there was measles at Tamatama right about the same time there was measles at Ocamo, and the Brazilians could easily have been responsible for either transmitting measles to the boy at Tamatama, or picking it up there and carrying it to Ocamo, with tragic consequences either way. Also, it is a virtual certainty that the records that are available for this brief period of time three decades ago have failed to account for much, if not most of the comings and goings of individuals in the Upper Orinoco, especially indigenous inhabitants. Measles could easily have been carried either directly to Ocamo by unknown individuals, or indirectly by Ocamo residents who visited friends and relatives at disease centers like Tamatama lower on the river, and returned with measles.

Could the Brazilian boy actually have been the source of measles?

Warning: this section on subclinical measles is *very* preliminary. We are still consulting sources and checking with experts.

Neel et al. suggest, in their 1969 article on the epidemic, that a 14-year-old Brazilian who had recently arrived at the Ocamo mission with a case of measles was the source of the epidemic. As Neel et al. clearly state, the diagnosis of measles in this young man was uncertain because he did not develop one of the diagnostic symptoms of measles, the characteristic morbilliform rash. Tierney makes much of this uncertain diagnosis, implying that there was some sort of cover up. Why would Neel et al. attempt to pin the epidemic on an uncertain diagnosis unless they were afraid of being accused of starting the epidemic themselves? We feel, given that measles was in the area, and because it is so contagious, that there were many people who could have brought it to Ocamo; so the idea that a cover-up was necessary is ludicrous. Still, we will make an effort to evaluate Tierney's evidence, even though this effort must be viewed as very preliminary.

Tierney attempts to show that cases of measles that don't develop the rash are almost unheard of, casting doubt on Neel et al.'s account:

> However, in this original version of the epidemic, Neel acknowledged that the Brazilian teenager never showed a measles rash. ("The characteristic morbilliform rash never developed...") That was peculiar. One hundred percent of measles victims develop a rash, according to most medical texts. (Tierney, p. 61)

Tierney cites *one* medical text (Markowitz and Katz 1994), which itself displays a chart from another study of measles. In that particular study, 100% of the 33 cases of measles did exhibit rash; given the relatively small number, however, one can't say that *all* cases of measles exhibit rash. So, how likely is it that the Brazilian teenager might have been the source of the epidemic? We don't know. Tierney claims that Neel suggested that the boy had a *subclinical* case of measles (that is, a case without the characteristic rash). Tierney discounts this suggestion:

> This was within the reach of possibility, but just barely. Subclinical measles is extremely rare, according to a recently written world history of the disease; transmission of measles by a subclinical carrier has never been proven, according to a widely used medical text on vaccination procedures.
>
> I have found only one case of a person suffering from "subclinical" measles where it "simmered" for months. (Tierney p. 66)

First, Neel does not claim that the possible subclinical case simmered "for months." Second, the literature cited by Tierney describes four cases of subclinical measles, not one (see Enders et al. 1959 and Mitus et al. 1962, both cited by Tierney). Third, an article appearing in the same issue of The New England Journal of Medicine as other articles cited by Tierney (Kempe et al. 1960) claims the ratio of subclinical to clinical measles is 1:19, a not insubstantial fraction. Fourth, Tierney claims that "transmission of measles by a subclinical carrier has never been proven, according to a widely used medical text...." Here is what that text (the review article mentioned above) actually says:

> Transmission from exposed immune asymptomatic persons has not been demonstrated but is currently being investigated. (Markowitz and Katz 1994).

(In one of the many ironies of the fact-checking process, the above statement itself cites an article entitled "Failure of vaccinated children to transmit measles." Why didn't Tierney discuss that article?)

What is clear is that at least one article that Tierney cites frequently (Wilson 1962) and the current literature both discuss many cases of apparent subclinical measles. Wilson spends 1/2 page of his five page article laying out the evidence for "latent" (i.e., subclinical) cases of measles.

Searching the more recent literature for information on "subclinical measles," we found many articles reporting extremely high rates of infections by the wild virus that were not associated with the characteristic rash. For example, a serological study of healthy adult Nigerian men (Harry 1981) found that 30.8% of those tested had recently been infected with wild measles virus despite that fact than none had any recent history of clinical measles (that is, did not show the characteristic symptoms). In another serological study among children in Tamil Nadu during a measles epidemic (Charian et al. 1984), 24 children had no history of clinical measles. Surprisingly, 16 of the 24 (67%) had measurable antibody indicating infection with the measles virus, i.e., 67% of these children appear to have had subclinical cases of measles. The authors of this study had earlier found that 20-40% of children in India had subclinical cases of measles (John et al. 1980). Most interesting, here is the abstract of study that appears to have found widespread subclinical measles in a 'virgin' population (Pedersen et al. 1989):

> Measles vaccination was performed in the arctic district of Scoresbysund, Greenland in 1968, which had never been exposed to natural measles. More than 90% of the total population was vaccinated and a 94-100% seroconversion was obtained. During a serological survey to examine the immunity status of the vaccinees, it was discovered that a temporary increase in measles antibodies took place in the majority of the population 2-4 years after the vaccination. This was not accompanied by clinically observed measles. Most likely, it was due to an inapparent measles infection in a population considered highly immune after vaccination.

In other words, a majority of the population suffered subclincial measles! This study also suggests that subclinical measles is contagious (since the majority of the population was infected but there was no clinically observed cases of measles), a view echoed by Harry's 1981 study. Here is the conclusion of that study:

> Wild type measles virus, which causes subclinical infection in adults (parents), may cause clinical measles in the children, and this adds to the problem of measles control in this part of the world. (Harry 1981)

The literature shows that subclinical measles is far from rare. However, we must note that we do not know whether the symptoms described for the Brazilian teenager would qualify as a case of subclinical measles similar to those found in these other studies. We are continuing to investigate this issue. More importantly, we also note that there is not the slightest suggestion in Neel's log that he was worried about having started an epidemic, so why would he have been motivated to cover anything up, especially since he was well aware that many people could have brought measles to Ocamo? Also, Neel wasn't even in Ocamo when the initial decision to vaccinate was made. He was busy vaccinating in another part of the region (the Ventuari river– see the map on p. 31). The decision to vaccinate was made by a French and Venezuelan team of doctors who arrived in Ocamo well before Neel, and who were not part of Neel's team (they were rightly concerned that the Brazilian case might initiate an epidemic). Is it plausible that Neel was covering for doctors that weren't part of his team? Conversely, would he try and set in motion a dangerous experiment when he was busy working elsewhere? Tierney's speculations are absurd.

The Epidemic

'First' Yanomamö death may not have been a Yanomamö

Tierney opens Chapter 5 with a tale of a boy's death of measles:

> Near the juncture of the Orinoco and Ocamo rivers, by a dirt airstrip at a Catholic mission, there lies an unmarked grave. Thirty years ago, a small cross, befitting a child's burial, was erected at this spot, but the tropical weather made a quick casualty of the wooden memorial. With clouds of gnats by day and mosquitoes by night, it is not a pleasant place to live, or to die, or even to be buried. Today nobody except Roberto Balthasar's parents remembers where he was interred or what killed him.
>
> Yet, according to mission records, Roberto Balthasar died of measles, on February 15, 1968. Hundreds, perhaps thousands, of others also died of measles that year on the Upper Orinoco. Two things made Roberto Balthasar's death notable: his was the first clearly diagnosed case of measles among the Venezuelan Yanomami. And, according to the boy's father, Napoleon Chagnon vaccinated him. (Tierney, p. 53)

The interesting thing about this paragraph is that Roberto Balthasar was probably not a Yanomamö. His father was not a Yanomamö, and his mother is not clearly stated to be a Yanomamö either. According to Tierney, the father was "a Brazilian of mixed Indian, African, and Caucasian background, who married an Indian woman on the Orinoco (Tierney, p. 64)." Because the mother is identified only as an "Indian woman on the Orinoco," she could conceivably belong to any one of the many indigenous ethnic groups living along the river. If she was Yanomamö, why doesn't Tierney mention this?

Tierney's major theme in this chapter is that the Yanomamö were uniquely, genetically vulnerable to the measles vaccine used by Neel et al. He starts off this chapter cynically telling the tale of this boy's death as if he were the first Yanomamö casualty of the epidemic, and that his death was somehow caused by Chagnon. He then goes to some length to conceal from the reader that Roberto's father was not a Yanomamö. Tierney retells the story of Roberto's death on p. 64, but does not mention that this is the same death he recounted at the beginning of the chapter! Not only that, when he mentions his interview with the boy's father, he conveniently fails to mention the father's last name:

> One sick child was sent to the Mavaca mission, whose diary for February 15 read, "At 13 hours the little one-year-old boy, the son of the worker Vitalino of the Ocamo residence, breathed his last. He was brought here by his parents in critical condition--measles, bronchopneumonia--he had every medical attention possible."
>
> I spoke to Vitalino, the baby's father, at his small house in the city of Puerto Ayacucho. Vitalino, a small, sturdy man with light brown skin, was the administrator of the Ocamo mission. (Tierney, p. 64)

Only by looking up the footnote for this interview, or by noticing many pages later on p. 70 that "Vitalino Balthasar" was the "former mission administrator" would the reader be able to recognize that these two accounts are one and the same, and that the "first clearly diagnosed case of measles among the Venezuelan Yanomami" was in fact probably not a Yanomamö. Tierney conceals this because he knows that it would seriously call into question the credibility of his claim that a licensed vaccine could cause mortality in a supposedly uniquely, genetically vulnerable population, if he is also claiming that it could cause mortality in anyone. The latter is known to be false: 19 million doses of Edmonston B have been administered to an enormous variety of ethnic groups, including unexposed, rural populations suffering malnutrition, disease, etc., with no mortality beyond a handful of individuals with severely depressed immune systems (i.e., people with leukemia and HIV).

Why did Neel et al. only vaccinate half of the village at Ocamo: was this an experiment?

Tierney strongly implies that the patterns of vaccinations by Neel et al. suggest an experimental procedure, perhaps involving control groups (where one half of the village is vaccinated, and the other half serve as unvaccinated controls):

> At the Ocamo mission, Chagnon and Roche vaccinated forty people. Thirty-six Yanomami at this same village did not receive the vaccine. If they were inoculating in an emergency, as Neel claimed, why only half the village? (Tierney, p. 60)

That Neel was not conducting an experiment with measles vaccine is clear from his field log, as we noted above. The source for the claim that Neel et al. only vaccinated half the village is the Neel et al. article on the epidemic (Tierney cites no other source). Neel et al. state that on January 22, “40 Indians and Brazilians at the mission were vaccinated at once (p. 421)”. On p. 423, Neel et al. state that they vaccinated 31 individuals against measles at Ocamo mission. So, it appears that 31 Yanomamö and 9 Brazilians were vaccinated on the 22nd. Neel et al. make no statement that only half the village was vaccinated.

How does Tierney come to the conclusion that only half the village was inoculated? Well, on p. 426, Neel et al. mention that on February 17 (25 days later), they responded to a call for assistance at the Ocamo mission, where measles had broken out among non-vaccinated villagers. That morning they saw 36 unvaccinated villagers, 17 of whom were in their second day of measles. So, the inference that only half the village was vaccinated comes from the fact that were 36 unvaccinated individuals at Ocamo, compared to the 31 (or 40) who were originally vaccinated over three weeks before.

Tierney speculates:

> There were only two possibilities. Either Chagnon entered the field with only forty doses of virus; or he had more than forty doses. If he had more than forty, he deliberately withheld them while measles spread for fifteen days. If he came to the field with only forty doses, it was to collect data on a small sample of Indians who were meant to receive vaccine without gamma globulin. Ocamo was a good choice because the nuns could look after the sick while Chagnon went on with his demanding work. Dividing villages into two groups, one serving as a control, was common in measles vaccine experiments. (p. 60)

This is pure speculation; there are many more than just two possibilities why only about half the village was vaccinated. Perhaps the Neel team only had 40 doses of vaccine in that village on the 22nd (but this could easily have been a simple accident, and not a preplanned experiment). It is also quite likely that only half the village was in residence. This was the dry season, when Yanomamö frequently visit other villages, and it would be quite normal for about half the village to be off visiting other villages. It could also be that Ocamo itself received visitors between January 22 and February 17. Thus, some or all of the unvaccinated Yanomamö could have come from elsewhere (people in this area generally visit the mission when they’re sick). Finally, because many individuals would have responded to the vaccine with fevers, vaccinating half the village at a time would reduce the burden on care providers to monitor and manage these fevers.

It is interesting that if Tierney felt this was such an important point, he either did not question Neel about it during his interview with him, or he did not report what Neel said about it. He also failed to ask Marcel Roche, the physician who actually administered the first round of vaccines, about this, even though he also interviewed Roche about events that day (Tierney p. 62). Here is what Chagnon says about this issue (personal communication, Oct. 19, 2000; his field notes do not have an entry for 1/22/68):

> We barely had time to do anything---let alone take notes. I don't recall exactly what happened when we got to Ocamo. Neel wasn't with us and I came in with Roche and the French M.D.s. I had planned to proceed upstream, but we were called to attend to some sick people, possibly the night we arrived. Roche wasn't sure if one of the Brazilians had the measles or not, but since the risks were high, we decided to inoculate them immediately, even though we didn't have gamma globulin with us (it was with Neel).
>
> I don't know why we did not inoculate all of them, and I must defer to someone else who might have taken notes on this. It is possible that not all of them were home at that time. Regardless, I believe that all of the Indians survived the epidemic at Ocamo but some of them got wild measles.

Chagnon's field notes do discuss the fate of the vaccinated Yanomamö at Ocamo:

> 2/17/68
>
> Comar sent word up from Ocamo tonight that 30 cases of measles broke out at Ocamo: all those whom we vaccinated three weeks ago are well and have not broken out....The epidemic at Ocamo was not as bad as (1) I thought it would be and (2) as it would have been without the vaccinations we gave (Roche) three weeks ago. None of the vaccinated individuals came down with measles.

Neel et al. confirm that all vaccinated individuals at Ocamo survived: after a discussion of the reaction of these individuals to the vaccine, Neel et al. conclude "No specific complications [e.g., mortality] were observed (p 423)". Whether or not there was mortality among the unvaccinated individuals is not mentioned in this article (Tierney's claim that there were deaths at Ocamo related to the vaccine will be dealt with in a later version of the report).

Did the Neel team fail to provide proper medical care?

Again, Turner and Sponsel manage to exaggerate Tierney:

> Once the measles epidemic took off, closely following the vaccinations with Edmonson B, the members of the research team refused to provide any medical assistance to the sick and dying Yanomami, on explicit orders from Neel . He insisted to his colleagues that they were only there to observe and record the epidemic, and that they must stick strictly to their roles as scientists, not provide medical help . (Turner & Sponsel, original email to Lamphere and Brenneis).

In fact, as Neel's field log documents in numbing detail, the Neel team devoted an enormous amount of time to medical care. Tierney is actually a bit more circumspect; for example:

> Even more curious was the fact that Neel never vaccinated the other half of the Ocamo village, even though he arrived on February 4 with both vaccine and gamma globulin, which he and Roche administered to some of the surrounding villages [according to mission records]. (Tierney, p. 60).

Why did Neel et al. not vaccinate the 36 uninoculated Yanomamö at Ocamo whom they knew had been exposed to measles? What Tierney fails to mention is the treatment Neel et al. *did* provide for these 36 Yanomamö:

> Those who were still well received gamma globulin, whereas the more ill among those with measles were given depot penicillin or Terramycin. (Neel et al. 1970, p. 426)

Providing gamma globulin is the standard treatment for individuals who have already been exposed to measles virus, but are not stricken with the symptoms. If provided within four days of exposure, it actually prevents measles; if provided after four days, it attenuates the illness. For more information, visit the following web site:

http://books.nap.edu/books/0309048958/html/118.html

Thus, Neel et al. provided exactly the right treatment for this group of exposed, but unvaccinated and asymptomatic individuals. For those who had symptoms, the only treatment was antibiotics, which Neel et al. provided. For those who were exposed, but did not yet have symptoms, treatment with gamma globulin, if it was given within four days of exposure (and who knew when these individuals had first been exposed?) would prevent measles; if given after, it would attenuate measles.

Tierney claims that he "devoted months to measles, reading several books and several hundred articles on early vaccination experiments (Tierney, p. 70-71)." After this education, Tierney either still did not know that providing gamma globulin after exposure was the standard treatment, or he fails to inform the reader of this while wondering in print why Neel et al. didn't vaccinate a group of individuals that had already been exposed for an unknown number of days.

Conclusions on Chapters 4 and 5

Tierney misleads the reader in numerous ways:

1. Tierney wrongly implies that the Edmonston B vaccine was dangerous in populations like the Yanomamö when the very literature he cites to support this thesis comes to the opposite conclusion. Tierney cites two studies in Native American, measles inexperienced populations, and two measles experts, in an attempt to question the safety of the vaccine used by Neel et al. However, both articles indicate that the vaccine was safe. The authors of the Panama trial state that "*since even a marked vaccinal reaction was preferable to the risk of the naturally occurring disease in infants, it was thought that the reactions would neither constitute a drawback for use in the Panama area nor prevent the use of measles vaccine in many other areas*." The authors of the Alaskan trial conclude "*Clinical reactions to vaccines were no more severe than those observed in other populations,"* and that the vaccine reactions were *"considerably less than that associated with true measles."* Tierney fails to discuss any of these clearly stated but inconvenient facts. Tierney also claims that one measles expert couldn't believe that Neel et al. would use the Edmonston B vaccine among the Yanomamö, but this same expert advised the Neel team on proper use of the vaccine, and then discussed their data a few years later in a review article without raising any concerns. The other endorsed the use of the vaccine in tropical populations only a few sentences after the ones Tierney quotes.

2. Tierney wrongly implies that the vaccine virus could be transmitted, thus causing an epidemic, when, again, the study he cites in support finds the opposite: the vaccine virus was not transmitted despite months of intimate contact with a susceptible sibling.

3. Tierney fails to coherently explain Neel's theory of the evolution of human intelligence. He wrongly implies that a vaccine experiment would somehow test this theory in an attempt to create a motive for Neel to conduct unethical experiments.

4. Tierney errs by portraying Neel et al.'s view on Native American susceptibility to a measles epidemic as eccentric, when, according to his own expert, it is in fact a mainstream view. Tierney also fails to note that this view would encourage an increased medical response to measles epidemics in vulnerable populations. He also states with no supporting documentation that Neel wanted to observe reactions to measles (or measles vaccine) in order to test this mainstream view, again, in an attempt to demonstrate a motive for conducting unethical experiments.

5. Tierney wrongly attempts to link Neel's views on the evolution of intelligence with Chagnon's views on warfare in non-state societies in an awkward attempt to suggest some kind of conspiracy to commit crimes in the name of science.

6. Tierney attempts to convince the reader that Neel et al. were the likely source of the epidemic by failing to accurately describe simple facts of geography, by failing to note other recent or concurrent outbreaks of measles in the vicinity, and by suggesting that the identified source (the Brazilian teenager) was very unlikely to be the source because he had a subclinical case of measles, even though the literature is full of descriptions of subclinical cases of measles.

7. The 'first' Yanomamö death from measles was probably not a Yanomamö.

8. Tierney fails to note that the vaccination patterns that he claims are evidence of an experiment have many more plausible explanations.

9. Based on information in the Neel et al. account of the epidemic, Tierney insinuates that Neel et al. failed to provide proper medical care when, in fact, the information Tierney cites shows precisely the opposite.

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 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 (Biocca, 1970; Valero, 1984). The sanctions behind the taboo are more complex than just this, for they involve a combination of fear, respect, admiration, political deference, and honor. (Chagnon 1997, p. 19-20).

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. (Tierney, p. 32-33)

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. (Chagnon 1997, p. 25-26; italics in the original)

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 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 1968, p. 12, emphasis added).

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. Chagnon also recounts his elaborate attempt to avoid mentioning the name of a recently deceased woman to her kin, an attempt which backfires on him (see Appendix XIII). In his book Studying the Yanomamö, which Tierney cites frequently (Chagnon 1974), Chagnon details ten suggestions for obtaining accurate genealogies from the Yanomamö. Tierney somehow failed to cite 'Point 6' which states:

> 6. *Do not accept an informant's statements about his own close kinsmen, and do not solicit them.* I make every effort to avoid embarrassing my informants or hurting their feelings by asking them about their close kinsmen. (Chagnon 1974, p. 95; italics in the original)

Once the Yanomamö came to trust Chagnon, they readily provided him information, especially when they were outside their villages:

> Once out of their village and aware that I already knew the names of everyone in their village, most of these informants would cooperate with me. After several such experiences I knew who the best ones were, and used them more than the others. They always knew beforehand what kind of "*Ribromou*" (working with paper) we would be doing, so it was *not* a matter of tricking them into coming a long distance and using them as "captive" informants. Most of the informants were very cooperative outside their village. (Chagnon 1974, p. 91; italics in the original)

Chagnon confirms in a recent email that after about six months in the field, he had little trouble obtaining accurate genealogical information:

> From about the sixth month I was very circumspect and had no difficulty getting genealogies because, by then, the Yanomamö trusted me and revealed the names with no problems--as long as I was circumspect and respectful, and that has been the modus operandi for the vast fraction of my career. The argument that set I into motion "hostilities and antagonisms" between informants and my field methods were therefore provocative is nonsense. The Yanomamö all know that I know their names and know that I am very circumspect about using them. Indeed, for a very long time the first thing they do when I arrive at a village after a long absence is to pull me aside and quickly tell me "don't ask about so and so", "don't ask about so and so's daughter..." etc. That's what they do among themselves as well. (Chagnon, personal communication)

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.

Hamilton Rice

Tierney opens Chapter 3 with a few historical vignettes meant to illustrate the long history of destructive contact between Westerners, including scientists and 'tribal' peoples. Although it is sadly true that indigenous inhabitants of the Americas and elsewhere suffered tremendously from contact, Tierney unnecessarily distorts the historical record. Here is Tierney's retelling of a tragic encounter between an American scientist and the Yanomamö:

> The first American to attempt the Orinoco's origin was the noted geographer Hamilton Rice, on assignment for the Royal Geographical Society. He camped above the turbulent Gauharibo Rapids, considered the border of Yanomamiland, on January 21, 1921 [actually, it was 1920]. There, seeing his abundant supplies, a group of about sixty Yanomami came begging for food and trade goods. This was the Yanomami's typical approach to outsiders, but it startled Rice, who decided to take no chances. He opened fire with his Thompson machine gun and did not bother to count the dead. The Rice expedition fled downriver. He later wrote in Royal Geographical Society's Journal that the Yanomami were cannibals who ate raw flesh and that, given the danger of becoming dinner, it had been "necessary to fire to kill." (Tierney, p. 20-21).

Although Rice did fire on a group of Yanomamö, Tierney has omitted key facts and seems to have fabricated others. First, we did not find any reference to a machine gun, Thompson or otherwise, in Rice's account of his 1919-1920 expedition. According to the Encyclopedia Britannica, the Thompson was patented in 1920, strongly suggesting the gun was not available when Rice departed on his expedition in 1919. According to the following web sites, a prototype was first publicly demonstrated in 1920 (again, after Rice had left), and did not start coming off production lines until the Spring of 1921, one year after the events described here:

> http://www.auto-ordnance.com/ao_ao.html
> http://nfatoys.com/tsmg/web/coltguns.htm
> http://www.rt66.com/~korteng/SmallArms/thompson.htm

Here is the only mention of firearms by Rice, with no mention of a machine gun:

> Fuentes, Ober, and I boarded the falca, whence the scene could be better surveyed, and to get ready if necessary the only firearms we had, a rifle, shot-gun, and revolver in addition to the superannuated muzzle-loader belonging to Filomeno. Andre, machete in hand, stood on some rocks just above the falca; the other men were grouped together in the camp, with faces stoical as they gazed steadfastly at the Guaharibos. Attempts to communicate with them in Spanish, Tupi-Guarani, Maquiritare, and Barb were equally futile, as were signs and the offer to them of some knives, fishhooks, and mirrors (Rice 1921).

We believe it is unlikely that Rice had a Thompson submachine gun on the 1920 expedition.[1]

Contra Tierney, it is crystal clear that Rice made several attempts to establish friendly relations with the Guaharibos (Yanomamö), and didn't fire until he considered himself to be under attack:

> During the attempts at parley before actual hostilities commenced, Chandless's account of his encounter with the Nauas on the upper Jurua came vividly to mind, and every effort was made to conciliate or at least establish neutral relations, before the last expedient of shooting was resorted to. Our attitude however seemed to be interpreted as weakness; for suddenly four Guaharibos [Yanomamö] on the down-river side descended the bank, ran out on a fallen tree-trunk lying in the river in shallow water, and started across, fitting arrows to their bows as they came. They

[1] Rice did report having Thompsons on a later trip to a different region of Yanomamoland [Rice 1928]--Tierney probably lifts the Thompson embellishment from this later report. There is, however, another report, published 30 years after the fact, that Rice had a 'machine gun' on the 1920 expedition: Ybarra 1950. We are not sure whether the account in Ybarra also confuses the 1920 and 1928 expeditions.

> meant to fight, and as the first shot was fired over their heads an arrow sent from up river landed beside me (Rice 1921).

We obviously do not excuse or condone Rice's actions (nor do we judge them at all). However, Tierney has once again withheld critical information from his readers, information that undermines his account. We provide an extended excerpt from Rice's article in Appendix XVII so readers can judge for themselves.

Tierney also quotes Rice in an attempt to discredit Chagnon's depiction of the Yanomamö as relatively healthy:

> In *1924-25,* the geographer Hamilton Rice penetrated to the eastern flanks of the Parima Massif, on Brazil's Parima River, where he noted obvious malnutrition. Rice concluded that these remote Yanomami "are not the fierce and intractable people that legend ascribes them to be, but for the most part poor, undersized, inoffensive creatures who eke out a miserable existence." (Tierney, ch. 16)

This quote comes from an account of a later expedition by Rice and would seem to contradict Chagnon's depiction of the Yanomamö as both healthy and war-like. What Tierney fails to mention is that Rice is talking about a very different group of Yanomamö. In the latter part of the *same* sentence Tierney quotes, Rice portrays the group Chagnon studied in terms that strongly support Chagnon's characterization. Here is Rice's complete quote (material in bold not quoted by Tierney):

> The Shirianas [a group of Yanomamö, probably in Brazil] are not the fierce and intractable people that legend ascribes them to be, but for the most part poor, under-sized, inoffensive creatures who eke out a miserable existence, **barely surviving the privations and diseases of a harsh and exacting environment,** ***a very different nation from the bold and warlike Guaharibos*** [i.e., the Venezuelan Yanomamö Rice originally encountered, and those with whom Chagnon worked]**, on the west side of the Parima serra.** (Rice, 1928; emphasis added)

Rice's account of the 1920 encounter also portrays the Venezuelan Yanomamö as quite healthy: "They were a big, muscular, well-nourished-looking lot, with broad, round faces and shocks of thick black hair" (Rice 1921; see appendix XVII). Rice's description of the Yanomamö supports Chagnon's observations rather than refutes them. Tierney, once again, dishonestly quotes a source in order to discredit Chagnon.

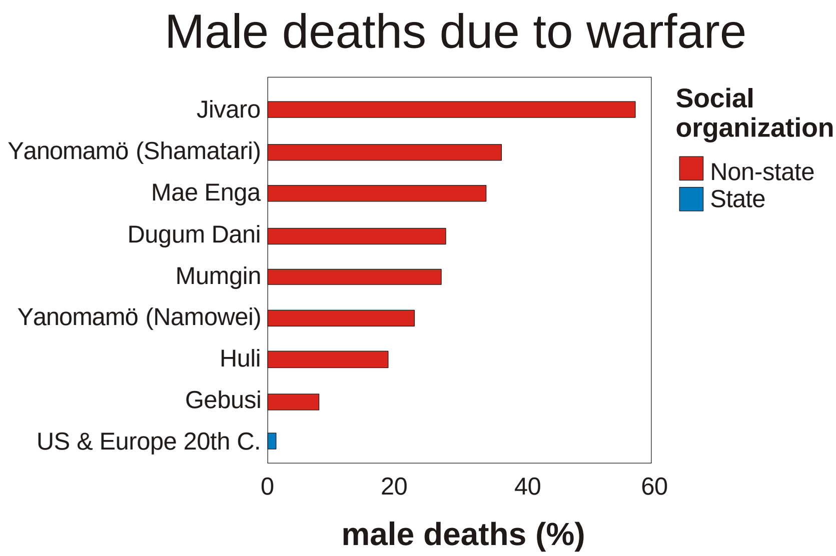
Detailed Evaluation of Chapter 10: To Murder and to Multiply

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" (Chagnon 1988). 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 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. Subsequent research has shown that the Yanomamö are quite typical in this regard, as the following chart shows (data from Keeley 1996):



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. (Tierney, Ch. 10; emphasis added).

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 motivation for a raid). Some of the most dramatic are those of Helena Valero. We have included one of her accounts in Appendix XVI of this report. Here we present Hans Becher's report on Brazilian Yanomamö warfare (based on fieldwork in 1956) from the Human Relations Area Files (HRAF):

> File: Yanoama (Northeastern South America) OWC: SQ18
> Field Date: 1956
> Publication Date: 1960
> Place Coverage: Surára, Pakidái (Brazil)
> Time Coverage: not specified
>
> GUIDE TO THE SQ18 YANOAMA FILE
> The Surara and Pakidai, two Yanoama tribes in northwest Brazil Hans Becher
> VII. Social Organization
> 5. War-- níanyu --
>
> The Yanonámi /Yanoama/, as has been mentioned above (see p. 13), are split into two antagonistic camps, the Xiriána /Shiriana/ and the Waiká /Waica/, with their smaller satellite tribes. There is constant, open hostility between these two "power blocs," on the one hand, and toward outside enemies (Karaiben /Carib/ and Aruaken /Arawak/) on the other. They accuse each other of a propensity for thievery and attribute every sickness and every death to sorcery on the part of members of hostile tribes. Hence blood revenge-- noyú --plays a very great role. The chief cause of all wars, however, is connected with the fact that they feel it necessary to abduct

women and girls of hostile tribes and groups, in order to bring new blood into their own tribal unit. Since many tribes are comparatively small, increasing relationship by marriage already requires exogamy to a certain extent (339), and friendly tribal units are therefore in a connubium. In time, however, strong ties of kinship again develop here, so that finally, and also as a result of the preference for polygyny, there is a real shortage of women, which is felt very painfully by the men as yet unmarried (340). For this reason, as I was told by informants, they now and then make raids on hostile tribes, which take place at dawn without any declaration of war (341). Surára and Pakidái warriors, however, must also take part in raids proposed by the Xiriána. They are always led by the war chief (the chief, his brother, and sons do not take part in military actions (see p. 129), nor do women (342)), and they set out on the day before the proposed raid. All the participants paint horizontal serpentine lines on their bodies and faces with urucú. This is the typical war painting, for in peacetimes the serpentine lines are always applied vertically (343). In the vicinity of the hostile maloka they spend the night quietly, while a few scouts reconnoiter the position. At dawn the warriors blow large quantities of snuff into each other's noses and then, completely unfettered, but in all silence they storm the hostile maloka . Men and boys are killed ruthlessly with arrow shots, and the women and children are dragged off as captives. But the attackers, too, often have to suffer great losses, and it is said that they are sometimes put to flight.

During my stay among the Surára, I heard of an attack by the Xiriána and allied warriors on a maloka of the Waiká on the Rio Mapulau, a left tributary of the upper Demini (see p. 16). On this occasion, as I was told, all the Waiká men were killed, and the women and girls were brought to the Xiriána maloka as captives. The head of the Waiká chief, as the war chief of the Surára added, was then adorned with feathers by the Xiriána and left standing in front of their community house for several days as a trophy; after that it was thrown away (344). The night after the attack there was a big dance festival, in which all the men took part. Before that they took snuff and prayed to the hekurá (see p.199), in order to thank them for the victory.

Footnotes for the Text:

339 Zerries reports the same thing about the Waiká (Zerries, 1956a, p. 186).

340 The fact that for younger, unmarried men sexual intercourse with the wives of their older brothers is permitted may be connected with this.

341 The descriptions of the war expeditions of the Karaiben /Carib/ which Richard Schomburgk (Richard Schomburgh, 1848, II, p. 321) and Appun (Appun, 1871, p. 447) write sound very similar.

342 Richard Schomburgk and Koch-Grünberg report that among a few Karaiben tribes women take part in war expeditions as porters. (Richard Schomburgk, 1848, II, p. 322; Koch-Grünberg, 1923a, p. 103).

343 With reference to the Karaiben, too, Richard Schomburgk points out that their body painting in wartime differs from that of peacetimes. (Richard Schomburgk, 1848, II, page 322).

344 The Hório and Ebidoso in the northeastern Chaco, who occasionally behave in a similar way, are to be referred to as a parallel (Baldus, 1931, p. 73).

Becher’s report on Brazilian Yanomamö warfare is almost identical to Chagnon’s portrayal of Venezuelan Yanomamö warfare one-to-two decades later. Further, Becher’s report is easily available from the HRAF, a standard source of information on cultures worldwide. We are not surprised that Tierney failed to mention this report.

We spend the remainder of this section examining several of Tierney’s claims about Chagnon’s research on war. Tierney has substantially misrepresented his sources in every claim we have investigated.

1. Misrepresentation of data on Jivaro headhunting.

CLAIM: Tierney argues against Chagnon's claim that warriorship and reproductive success are correlated in tribal societies, citing data about the Jivaro:

> Among the Jivaro, head-hunting was a ritual obligation of all males and a required male initiation for teenagers. There, too, most men died in war. Among the Jivaro leaders, however, those who captured the most heads had the fewest wives, and those who had the most wives captured the fewest heads (Tierney, p. 178).

MISREPRESENTATION: In contrast with his normal procedure, Tierney doesn't give a page number reference for this cite, so we had to search through an entire book (Redmond 1994) to find it. The only data we could find that are relevant to Tierney's comment appear on page 126, Table 2. We'll reproduce the relevant portions of the table here:

Tally of Trophy Heads and Wives Acquired by Jivaro Warriors and War Leaders

WARRIOR	HEADS	WIVES
1. Chumbika	4	no data
2. older brother of 1	no data	8
3. an Aquaruna	>2	no data
4. Peruche	>50	4
5. Juanga	"numerous"	4
6. several men	50-60 each	no data
7. Utitiaja	59	>1
8. Juantinga	no data	no data
9. Cucusha	>50	no data
10. Anguasha	>50	no data
11. Tuki (José Grande)	no data	11

Note that firm data about both number of heads and number of wives are included for *none* of the warriors. Consequently, *no* conclusion can be reached about how number of heads correlates with number of wives, and the claim that "those who captured the most heads had the fewest wives, and those who had the most wives captured the fewest heads" is completely unfounded. At best, we can say that most warriors for whom a head tally is provided seem to have a lot of heads, and that most warriors for whom a wife tally is provided seem to have a lot of wives.

Further, the table's author reaches a conclusion about these data that is totally consistent with Chagnon's argument, and totally inconsistent with Tierney's portrayal:

> Yanomamö men who have killed tend to have more wives, which they have acquired either by abducting them from raiding villages, or by the usual marriage alliances in which they are considered more attractive as mates. The same is true of Jivaro war leaders, who might have four to six wives; as a matter of fact, a great war leader on the Upano River in the 1930s by the name of Tuki or José Grande had eleven wives. Distinguished warriors also have more offspring, due mainly to their greater marital success (Redmond 1994, p. 125).

2. 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 (Tierney, p. 178).

MISREPRESENTATION: Here, Tierney omits important information which supports the validity of Chagnon's result. Tierney refers to a recent ethnography of the Waorani (Robarchek & Robarchek 1998) 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 (p. 133). 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.

3. Portrays Chagnon's inclusion of dead and divorced wives as deceptive.

CLAIM: Tierney expresses alarm at Chagnon's claim that 7 men from Mishimishimabowei-teri had 3 or more wives, so he analyzes Chagnon's data himself:

> Thirty-four wives for seven men - 4.8 wives each. I could not believe it. So I decided to take all the information about all the 271 individuals at Mishimishimabowei-teri that was contained in two long appendixes of Chagnon's book *Studying the Yanomamö*, and put them in my own data-base. It was a very tedious and time-consuming task. It took me a week to enter and analyze the information... [I discovered that] only two men out of the whole village actually had more than two wives. One had three; the other had six (Tierney, p. 173).

Tierney goes on to say that the 7 men did not have 3, 3, 3, 5, 6, 6, and 8 wives each (as Chagnon claimed), but rather 1, 1, 1, 2, 2, 2 and 6 wives each. He continues: "In reality, these seven men had 15 wives (2.1 each). The other 'wives' were dead or divorced."

MISREPRESENTATION: Tierney acts as though Chagnon claimed to be only counting current wives in his study, and that he was somehow dishonest in including previous (divorced and deceased) wives. However, in the target article, Chagnon is straightforward about his inclusion of previous wives: "over a lifetime a successful man may have had up to a dozen or more wives, but rarely more than six wives simultaneously. One result is that some men have many children. In the sample considered here, one man (now deceased) had 43 children by 11 wives" (1988, p. 988). Indeed, in order to account for *lifetime* reproductive success of each male, as Chagnon aims to do, he obviously *must* take both previous and current wives into account.

It's also strange for Tierney to claim that in order to 'discover' that some of the wives were divorced or dead, he had to enter and analyze data from *Studying the Yanomamö* (Chagnon

1974), “a tedious and time-consuming task” that took “a week.” It took us approximately 5 minutes to check Appendix B of *Studying the Yanomamö* in order to confirm that Chagnon included both previous and current wives, and to determine how many of each kind of wife each of the seven men had (current wives are coded as 1 in this Appendix, while dead, shared and divorced wives are coded as 2, 3, and 4 respectively). Absolutely no data entry or analysis was required.

4. 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” (Tierney, p. 175).

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” (1988, p. 988). Tierney seems to suggest that Chagnon includes some headmen that he knows not to be unokai. Brian Ferguson (1989), 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 (Chagnon 1989, p. 566). 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.

5. Suggests that he discovered the identities of Chagnon’s villages.

CLAIM: Tierney is critical of Chagnon for not including the specific names of the twelve villages discussed in the target article (Chagnon identifies the villages by ID numbers and population statistics only), but says that he is able to name most of these villages himself:

> It took me quite a while to penetrate Chagnon’s data, but, by combining visits to the villages in the field with GPS locations and mortality statistics, I can identify nine of the twelve villages where all the

> murderers come from in his *Science* article... Chagnon did not invent the twelve villages for *Science*, as Lizot insinuated. Nor was his choice of villages arbitrary. These were the same shabonos where he had spent the great majority of his forty-five months on the Upper Orinoco (Tierney, p. 165).

Tierney then includes a table which includes Chagnon's ID numbers and population counts for nine of his twelve villages, along with what Tierney claims are the actual names and locations of each of the villages. The implication is that Tierney has been able, with considerable effort, to apply names to Chagnon's ID numbers.

MISREPRESENTATION: While it's true that Chagnon doesn't name each village in the target article, he probably doesn't do so because this information isn't particularly relevant to the main subject of the article. There's no evidence that Chagnon ever intentionally obscured the actual names of these villages (scientific journals sometimes *require* that individual and community names be omitted from articles). There's quite a bit of evidence, however, that Tierney wishes to claim credit for discovering - through his dogged, meticulous investigative reporting - information that Chagnon has in fact made widely available in sources that are cited by Tierney himself.

First, Tierney would not have had to discover for himself that "these were the same shabonos where [Chagnon] had spent the great majority of his forty-five months on the Upper Orinoco." Chagnon admits freely in a 1990 article that the data on which the target article is based were collected in the same 12 villages where he did most of the rest of his fieldwork: "During the past 25 years I have made 14 field trips to the Yanomamö. Most of this fieldwork was conducted among the some dozen or so villages described in my 1974 book and in my 1988 article" (Chagnon 1990, p. 49). Tierney cites this article elsewhere, but overlooks it here.

Second, five of the nine village ID numbers that Tierney claims to name are included on the *Yanomamö Interactive* CD (Biella et al. 1997) in the "Garden Locations" excel file: villages #5, 51, 84, 90, and 92. Buried in Tierney's endnotes is the revelation that this excel file identifies village #5 as Bisaasi-teri (Tierney p. 357, note 45), but Tierney fails to convey clearly that this file makes explicit name-number associations for villages #51, 84, 90 and 92 as well, preferring to leave the reader with the impression that making these associations required lots of investigative footwork.

So, if Chagnon himself both stated that these were the same twelve villages where he did most of his fieldwork, and if he also provided names for five of the nine villages that Tierney seems to take credit for naming, the only things left for Tierney to actually 'discover' were the remaining four village names. How did he go about doing this? Retracing his steps is complicated by the sheer sloppiness of his presentation, but we'll give it a shot. In the table on page 165, he says that three villages all have the same name and location (villages #5, 6, and 7 are all referred to as "Bisaasi-teri" and are all located at "Boca Mavaca"); in the text on the same page, he refers to these three villages as "Upper Bisaasi-teri, Lower Bisaasi-teri, and Monou-teri." (Judging the reliability of Tierney's name designations would be easier if one could determine what his name designations actually are). One of these villages (#5) is the one that he admits identifying from data in the *Yanomamö Interactive* CD. His references for identifying the other two (#6 and 7) are seven *printed* sources, five of which are authored or co-authored by Chagnon himself (Tierney, p. 357, notes 45 and 51). To name village #93, which is referred to as "Dakowa's village" in the *Yanomamö Interactive* CD, Tierney says he used two *printed* sources, both authored or co-authored by Chagnon (p. 357, note 50). To name the remaining village (#53), Tierney says he needed five sources, three of which are texts authored or co-authored by

Chagnon. The other two are FUNDAFACI census data and “the journalist Marta Miranda for Venevisión” (p. 358, note 52).

Regardless of how accurate or inaccurate Tierney’s name designations are, it appears that he relied mainly on Chagnon-authored sources in order to make them and that “visits to the villages in the field” were completely unnecessary. Further, the five other name designations, as well as the revelation that the twelve villages were those in which Chagnon did most of his research, could all have been easily and immediately obtained by consulting two Chagnon-authored sources of which Tierney is obviously aware.

6. 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 (Tierney, p. 164).

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” (Chagnon 1988, p. 986). 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’” (Chagnon 1988, p. 986). In his famous ethnography (Chagnon 1992) - 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” (p. 189) 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” (p. 190). 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” (1988, p. 990). 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” (1988, p. 990). This high status of unokais makes them more attractive as mates. In a published response to criticism about the target article, Chagnon (1990) goes into even greater detail about how unokai status makes men more attractive as mates.

Tierney is clearly aware of this publication, as he cites it fairly extensively. Nevertheless, he suggets that Chagnon claims that unokais achieve greater reproductive success only through abductions.

Finally, directly following the block of text from Tierney p. 164 that is quoted above, Tierney quotes Chagnon: “A captured woman is raped by all the men in the raiding party.” He seems to include this quote both as evidence that the image of the Yanomamö as abductionist raiders was something that Chagnon “deftly created,” and also in order to dispute the claim made in the quote. Tierney’s inclusion of this quote is problematic for two reasons: (1) The quote is taken from Chagnon 1990, p. 190, and closely follows the above-quoted lines from Chagnon 1990, p. 189-190, in which Chagnon comments on the relative insignificance of abduction as a motive for raiding. Because Tierney wants to use this quote to argue that Chagnon deftly created the abductionist raiders image, he has to badly misrepresent the context in which this quote was made. In contrast to his normal procedure, Tierney fails to reference this quote, presumably in order to obscure the fact that he has taken it out of context. (2) In support of his argument for the falsity of the claim made by Chagnon in this quote, Tierney cites an unpublished manuscript (co-authored, strangely enough, by Chagnon himself) that does not show up anywhere in his bibliography.

7. Misrepresents a study that he claims refutes Chagnon.

CLAIM: Tierney argues against Chagnon’s claim that warriorship and reproductive success are correlated in tribal societies, citing a study of the Cheyenne: : “...a study of the reproductive success of Cheyenne leaders showed that peaceful leaders had 50 percent more offspring [than war chiefs]” (Tierney, p. 178).

MISREPRESENTATION: The cited study (Moore 1990) does indeed purport to refute the idea that warriorship is correlated with reproductive success. However, Tierney misrepresents the study’s results. Moore begins by explaining that the Cheyenne had two kinds of chiefs, “peace chiefs” and “war chiefs.” He suggests that war chiefs would have been more warlike but less reproductively successful, because of the costs of participating in war (i.e., likelihood of being killed). He acts as if he is going to test this hypothesis, but then shifts gears and starts talking about Cheyenne “war bands” and “peace bands” (Moore says little about how these bands are different or what we are supposed to deduce from the fact that one is called a war band and the other a peace band). Moore announces that rather than compare war chiefs to peace chiefs, he will simply compare all members of war bands to all members of peace bands: “In the demographic analysis that follows, we will contrast all the men of the two groups rather than trying to determine which individuals were actually war chiefs or peace chiefs at any particular time” (p. 326). He then presents some data suggesting that members of peace bands tended to reproduce better than members of war bands. Whatever hypothesis Moore is testing here, he’s not addressing Chagnon’s claim that warriorship and reproductive success should be correlated *within* tribal bands. Contrary to Tierney, this is neither “a study of the reproductive success of Cheyenne leaders,” nor does it show that peaceful leaders outreproduced war leaders, and it is not a relevant test of Chagnon’s model.

The evident distortions uncovered in our preliminary investigation suggest that the reader treat the claims in the rest of the book with the utmost caution.

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—but we must stress that these are only speculations, speculations we ourselves find less than satisfying.

1. Tierney, like hundreds of other journalists and anthropologists, was clearly *extremely* motivated to gain access to the Yanomamö. Permission to Venezuelan Yanomamö territory on the Upper Orinoco is, however, extraordinarily difficult to obtain, and thousands of visitors are turned away every year. Both the Venezuelan government and the Roman Catholic Church restrict access to, by and large, medical personnel and missionaries. We believe that Tierney would not have been able to gain access to areas that he apparently gained access to without the permission and support of the Roman Catholic Church. The Church does not lightly provide such support, so it is possible that Tierney was granted access and support in exchange for something the Church wanted: the discrediting of Chagnon. Chagnon has, in very public forums like the New York Times, accused the Church of putting Yanomamö health and welfare at stake by attracting the Yanomamö to missions with gifts of shotguns that were then used in wars. Chagnon has also accused the Church of introducing diseases to the Yanomamö population via their numerous mission outposts in the region. Further, Chagnon has sided with local, democratically elected indigenous leaders who have lodged complaints against the Church with the US State Department, complaints that accuse the Church of denying the indigenous peoples in this area the right to govern themselves. The Church has angrily denied all these allegations, and has successfully prevented Chagnon from working in the region. Further, Tierney and other Church allies characterize the most prominent indigenous opponents of the Church as criminals. We cannot evaluate these claims, but it is obvious that Tierney's account of the recent history of events in the Upper Orinoco almost exactly matches the version promoted by the Church. Perhaps, by giving the Church what it wanted, Tierney has gotten what he wanted: very rare access to some of the most famous and 'exotic' people on earth. We have also learned that Tierney's family is Roman Catholic. Again, we genuinely do not know if this has any bearing on Tierney's decision to take the side of the Roman Catholic missionaries in their dispute with Chagnon and local indigenous leaders.

2. 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. Margaret Mead, for example, critiqued Western approaches to adolescent conflict and sexuality by comparing them to the supposedly superior practices of Samoans. 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.

Appendices

The appendices contain commentary on *Darkness in El Dorado* by experts. These individuals have not contributed to the body of this report and they cannot vouch for its contents. They are responsible for their comments only. They also have no affiliation with UCSB or the UCSB team investigating the allegations. The contents of these appendices have been posted on public web sites.

Appendix I: Email from Dr. Samuel Katz, measles expert

This is an open email from Dr. Samuel Katz, co-developer of the measles vaccine, that was sent to numerous individuals, including the original recipients of the Turner/Sponsel email.

September 28, 2000

Because I was the co-developer (with John F. Enders, Nobel laureate) of measles vaccine, I have been the recipient over the past 10 days of numerous phone calls and e-mails regarding the Yanomami and Patrick Tierney's accusations (Darkness in El Dorado). I am neither an anthropologist nor a geneticist. I am a pediatrician-vaccinologist who has spent the past 44 years in studies of various vaccines, especially measles.

Among the materials sent me is a memo (undated) from Terry Turner and Leslie Sponsel to Louise Lamphere and Don Brenneis. Their comments regarding Neel's use of measles vaccine are totally incorrect. Edmonston B vaccine which Neel administered at a time when an epidemic of measles was already underway (Amer J Epidemiology, 1970, 91:418-429, Neel et al) was a scientifically established and proven method of attempting to interrupt an outbreak. Nearly 19 million infants and children between 1963 and 1975 in the US and internationally received this licensed (by FDA) vaccine with or without immune globulin. Vaccine virus has never been transmitted to susceptible contacts and cannot cause measles even in intimate contacts. Drs. Turner's and Sponsel's memo indulges in hyperbole as well as errors ("virulent vaccine", "counterindicated by medical experts", "greatly exacerbated and probably started the epidemic of measles", etc.). Who are the unnamed "medical experts" they cite?

Once again, I cannot comment on Neel's style, goals or objectives, but the use of Edmonston B vaccine in an attempt to halt an epidemic was a justifiable, proven and valid approach. In no way could it initiate or exacerbate an epidemic. Continued circulation of these charges is not only unwarranted, but truly egregious.

Yours very truly,

Samuel L. Katz, MD Wilburt C. Davison Professor & Chairman Emeritus Department of Pediatrics

SLK/bc

Appendix II: 'Retraction' by Terence Turner

In the following email, Terence Turner, one of the authors of the original email responsible for widely advertising the Neel/Chagnon allegations, admits that there is no scientific basis for the central allegation that Neel, Chagnon, and others either deliberately or accidentally caused or exacerbated the 1968 Yanomamö epidemic (the email is addressed to Dr. Katz, a measles expert who wrote a strongly worded email to Turner and others decrying the 'egregious' circulation of these allegations--see appendix I):

September 28, 2000

Dear Dr. Katz,

Thank you for your message concerning the Edmonston B vaccine. <u>Now that I have had a chance to research the matter myself, I am in complete agreement with you.</u>

Let me explain something about the memo I and my colleague Leslie Sponsel sent, as a confidential document, to the President and President-elect of the American Anthropological Association, with copy to the chair of the Committee for Human Rights. We were sent advance copies of the galley proofs of Tierney's book, in which he makes the alleegations we describe in our memo. The sole purpose of the memo was to describe these allegations, in order to warn the leaders of the association of the nature of the allegations that were about to be published (the publication of Tierney's long article in the New Yorker, now scheduled for this coming Monday, was supposedly only two weeks away at the time) and the scandal they would probably cause for the whole profession. The purpose was not to describe the actual events to which the allegations referred--a distinction that has been lost by many who have reacted to the memo since it was circulated without our permission. Checking the veracity of the allegations for ourselves was not germane to the immediate, and limited purpose of the memo, which was to warn about what Tierney was about to publish. However, having sent the memo (which was around the world within days) we did set about doing our best to check on its more shocking allegations, particularly those concerning Dr. Neel's vaccination program and his use of the Edmonston vaccine. <u>One of the authorities we consulted was Dr. Peter Aaby, a well-known medical anthropologist and member of the Scandinavian medical team team that has been working on measles in West Africa for some twenty years. He has gone over the claims about the vaccine made by Tierney and refuted them point by point, in very much the same terms that you have used.</u>

We are in the process of preparing a memo that will state our own understanding of this matter, to help correct the confusion that the unauthorized circulation of our earlier memo. Thank you for your message.

Yours sincerely,

Terry Turner

[emphasis added]

Appendix III: Email from Susan Lindee, historian

The following is an open email written by Susan Lindee, a historian of science at the University of Pennsylvania. The notes of Neel's that Lindee refers to are housed in the American Philosophical Society in Philadelphia. Based on information in these notes, she disputes virtually all of Tierney's allegations (as summarized in the Turner/Sponsel email) about the actions of Neel and others during the epidemic.

September 21, 2000

Colleagues:

Today I had the opportunity to read James Neel's entire field notes for the 1968 work in Venezuela. I also read archival materials relating to his consultations with the Centers for Disease Control in late 1967 in preparation for the program in measles immunization he and his colleagues planned to undertake. And I read other correspondence in his papers, including correspondence with missionaries, Venezuelan authorities, Chagnon, and others.

The picture that emerges in these documents is at some variance with that presented in a widely circulated email describing the arguments in a new book by Patrick Tierney.

First, there are explicit matters of fact:

1. Neel had Venezuelan governmental permission to carry out the vaccine program-the telegram providing that permission is in his papers.

2. Neel had consulted a CDC expert on measles about how to administer the vaccine in November 1967, before the field trip which began in January 1968. The correspondence with CDC is in his papers as are records of the trip he made to Atlanta to meet with infectious disease specialists.

3. Neel included gamma globulin with all the vaccines he administered and kept meticulous records of names of persons immunized, and doses given. Apparently some vaccines were administered without gamma globulin by Roche, who was involved in a different project (measuring iodine uptake) with Amazonian populations.

4. Neel heard reports of a measles outbreak at a party on January 20 while he and his team were still in Caracas buying supplies. He did not give any vaccines until January 25, when he vaccinated 14 children under age 5 in a village that had experienced a measles outbreak five years earlier.

5. When the measles problem was identified as an epidemic, on or around February 16, Neel provided penicillin and terramycin not only to those affected in the villages he visited, but also to those who would be able to bring it to persons affected elsewhere. There is no evidence that he attempted to discourage anyone from providing treatment, and indeed for about two weeks he spent much of his own time administering vaccines and antibiotics.

5. Furthermore, Neel himself worked out a plan for controlling the epidemic, from 2 to 4 a.m. on 16 February, after he was awakened by a messenger bearing a frantic note from a colleague at the Ocama Mission, a note which said that

there was a serious outbreak of measles, and asking him to send gamma globulin. His “all Orinoco” plan included controlling movement of people in and through the five primary ports of entry to the region, liberal use of penicillin, vaccination when practical, and gamma globulin when practical.

It is clear from his notes that the epidemic drastically disrupted his field research, making it impossible for him to collect the kinds of data he had intended to collect, and it is clear that he was at times frustrated, even angry, about this situation. A measles outbreak emphatically did not facilitate his research.

I am of course basing the above account on correspondence and field notes in the papers of James V. Neel, and if we wish to adopt an X-files theory of history, we could propose that he planted these records, including the much-scribbled on and often almost illegible field notes, in order to mislead future historians about his actual behavior in the field.

There is one detail that does suggest a certain amount of forethought. All of Neel’s fieldnotes, for his work in Japan, Amazonia, and elsewhere, stayed at his home institution of Ann Arbor after his death earlier this year. He did make one exception. He photocopied his entire field notebook for the 1968 Venezuelan trip, and placed these photocopied pages in a file marked “Yanomama-1968-Insurance.” Having spent a good deal of time with James Neel, and even more time reading his correspondence, I know that he had a shrewd, dry sense of humor. I suspect that by the time he began parceling out his papers, he knew that Tierney was working on this book, and he copied the field notes for APS, where they would be widely available to scholars, as “insurance” against Tierney’s claims.

Of course none of the above addresses what might be considered the real questions. Neel was a Cold Warrior deluxe, and an elitist, who was confident about his hierarchical rankings of races, sexes, civilizations, fields of knowledge production, and forms of social organization. His work drew heavily on the notion of the Yanomama as “primitive” and as a natural population which could be used to understand the “conditions of human evolution.” Furthermore Neel knew--because he had asked the CDC to test antigen responses in his blood samples in 1967--that Yanomama in the very small villages he would be visiting had probably never been exposed to measles, or indeed to many other infectious diseases.

And so I think of Tierney’s book, which I have not seen, and I want to both refute the specifics-I am convinced that Neel’s intentions were benevolent in the classic colonialist sense-and express sympathy for the generalities. Amazonians have in fact been grievously damaged, in many ways, by those who came to them seeking to construct technical knowledge. But the book cannot be right if it does not respect the complexity of that damage, or the tangled human acts and ideas through which it came into being.

I am grateful to Robert Cox for helping me to navigate Neel’s recently accessioned papers so quickly, and to Jonathan Marks, Ricardo Santos, Joel Howell, Rayna Rapp, Gerard Fitzgerald and others who have been participating in this ongoing exploration of a book none of us seems to have read. Please feel free to share this email if you feel it is useful.

Susan Lindee
Department of the History and Sociology of Science
University of Pennsylvania

Appendix IV: Susan Lindee's email to Slate magazine

The New Yorker claimed that Susan Lindee had withdrawn her claim that Neel had permission from the Venezuelan government to vaccinate the Yanomamö. This is an email that Susan Lindee wrote to Slate, commenting on the New Yorker response to John Tooby's article therein.

Subject: Neel and the Venezuelan Government
From: Susan Lindee
Date: 31 Oct 2000 06:21

The New Yorker response to John Tooby's article perpetuates a mistaken claim that appeared earlier in Tierney's essay. I remain convinced that Neel had permission from the Venezuelan government for the vaccination program in the Upper Orinoco in 1968. My reasons for believing this are as follows:

1. Neel requested government permission, in a letter dated December 11, 1967.

2. Neel needed government approval to get the vaccines through customs.

3. Neel was working with a prominent Venezuelan physician, Marcel Roche, and in collaboration with a prominent Venezuelan scientific organization. Roche was in the field with Neel and carried out some of the vaccinations.

4. Neel had government permission later, as evidenced by a telegram sent to him in April 1968, when he had arranged for additional donations of vaccines to be sent to Venezuela, where the epidemic was still underway.

I have not been able to find a letter from the Venezuelan public health authorities dated December 1967 granting permission for the vaccine program, but I have a fairly compelling set of circumstances suggesting that the program was approved. The statement that the New Yorker identified as "erroneous" was my claim in an early email that the April 1968 telegram provided proof of permission--obviously the timing was wrong. But I remain convinced that Neel had permission, based on the archival record.

I must add that I have no particular stake in Neel's reputation. I am a historian who wrote a book about his work in Japan. He disliked my book rather intensely. If I had any evidence that he had behaved in an inhumane or irresponsible manner in Venezuela I would not hesitate to say so. But there is no reason to believe so. There are certainly serious questions raised by the scientific exploitation of the Yanomami. It is unnecessary to make anything up, which is what I think Tierney has done, as a result of having checked many of his footnotes. I find a remarkable pattern of dishonesty in his work and dishonesty serves no one's best interests.

Susan Lindee

Appendix V: Email from VEJA reporter

Chagnon's critics frequently claim that mining or military interests in Brazil use Chagnon's work to justify anti-Yanomamo policies. Euripedes Alcantara, reporter for VEJA (The Brazilian equivalent of Time magazine), has recently investigated this claim. Here is his email to Napoleon Chagnon on what he found:

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Date: Thu, 08 Mar 2001 18:36:16 -0300
From: [Euripedes Alcantara]
Organization: VEJA
To: [Napoleon Chagnon]
Subject: For your web site

I can assure you that I could not find a trace of Brazilian officials use of
your work as a justification of a certain policy towards the Yanomamo.

I have asked people on high ranks of the military, Funai and the Congress.
They have searched on my request almost all documents related to the
Brazilian indians policy. Nobody mentions your work as grounds for treating
the indians one way or another.

Best regards,

Euripedes Alcantara
VEJA Magazine
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Comment by Napoleon Chagnon:

One of the more persistent falsehoods regularly used by my detractors is the unsubstantiated claim that my ethnographic descriptions of the Yanomamö have been used by various groups of Brazilians to justify oppressive or punitive actions against the Yanomamö. For example, the Boston Globe, in a recent story about Tierney's accusations, referred to this 'truth' three different times in the course of a two-page article. At the 'open microphone' session at the 2000 meeting of the American Anthropological Association in San Francisco one Linda Rabben specifically denounced me for never 'repudiating' or 'denouncing' the use to which my ethnographic reports on the Yanomamö have allegedly been put by various nefarious Brazilian groups. I have no evidence that this claim is accurate. A number of my anthropology colleagues who work in Brazil and who are familiar with the Brazilian press and published statements in that press have also assured me that they are unaware of any claim that my ethnographic accounts of the Yanomamö have been appropriated by Brazilian interest groups and used as justifications for harsh or punitive policies against the Yanomamö.

This accusation appears, so far as I know, for the first time in the Brazilian Anthropological Association's (ABA) denunciation of me and my 1988 lead article in Science (239:985-992). This denunciation was published in the Anthropology Newsletter of the AAA in January, 1989 (see Appendix XVIII). It has subsequently grown by a law of it's own being and is now a veritable Anthropological Truth that nobody bothers to question.

I have posted the 1989 ABA denunciation of me and my response to it (Appendix XVIII) to inform the reader about the origins of some of Tierney's claims and how rumors and accusations take on a life of their own and are eventually considered to be Anthropological Truths. Although the then President of the Brazilian Anthropological Association signed the denunciation, it was written by Alcica Ramos and Bruce Albert, who subsequently sent a nearly identical copy of it to Science for possible publication under their own names.

Napoleon A. Chagnon

Appendix VI: Commentary by Dr. Kim Hill

This is a commentary on Darkness in El Dorado by Dr. Kim Hill of the University of New Mexico, one of the world's foremost experts on Native tropical South Americans. Dr. Hill's vitae can be viewed online here: http://www.unm.edu/~anthro/vitae/k_hill.html

I recently became aware that Dr. Leslie Sponsel, Univ. of Hawaii, sent out an unsolicited email mailing to 19 prominent international media organizations coaching them on appropriate "experts" to interview concerning the controversy surrounding Patrick Tierney's book "Darkness in El Dorado". In that mailing my name is included in a list entitled "Among those who have defended Chagnon by criticizing Tierney's book even before reading it are:" That statement is false on two accounts. First I have read the book. I was provided detailed information about the contents of the book in August of this year by a friend of mine who received an unsolicited copy apparently because he was expected to sympathize with the book's goals. At that time I chose not to read the book because I thought it would have no impact in anthropology nor be taken seriously by most informed scholars. After the infamous Sponsel/Turner letter to the president of the American Anthropological Association warning of an impending scandal, I was given a copy of the book by the president of the AAA in order to help advise her on appropriate reaction to the book. I read the entire thing from cover to cover in two days (including all 1599 footnotes) and long before I ever did any press interviews on the topic. I informed all members of the press who interviewed me that I had indeed read the entire book. None of them had seen a copy despite numerous requests to the publisher.

Second, the statement is false because in my interviews (and in my statement below) I have not unconditionally defended Napoleon Chagnon. Instead I have defended him only from obvious ideological persecution and from some specific charges that I know to be false. There are many other charges in the book that Chagnon himself will be in the best position to answer. I have suggested in interviews and in past public forums (some of this is quoted in the Tierney book) that Chagnon may have made some errors in judgement and that I disagreed with some of his actions, specifically during the time period when he was allied with Charles Brewer-Carias, and was making helicopter trips into the Siapa region. I have also mentioned that I was concerned about the negative attitude that many Yanomamo I have met seem to have towards Chagnon, and despite the fact that much of this attitude is clearly due to coaching by Chagnon enemies I do believe that some Yanomamo have sincere and legitimate grievances against Chagnon that should be addressed by him. The strongest complaints that I heard were about his lack of material support for the tribe despite having made an entire career (and a good deal of money) from working with them, and his lack of sensitivity concerning some cultural issues and the use of film portrayals. However, I think most of Chagnon's shortcomings amount to little more than bad judgment and an occasional unwise penchant for self promotion (something which seems to infuriate Yanomamo specialists who are less well known than Chagnon). The main reason he has been targeted by Tierney and his collaborators is clearly related to ideological and theoretical differences which his detractors believe are so immoral that they are prepared to use "whatever means necessary" to discredit him.

I have suggested in interviews about the Tierney book and in a series of documents to the president of the AAA that I think the book raises some important issues about the ethics of fieldwork (see this document at http://www.unm.edu/~kimhill/tierney/ethics.htm), the lack of coherent medical policy about contacts with isolated peoples (see this document at http://www.unm.edu/~kimhill/tierney/contact.htm), and the use of personal smear tactics in anthropological debate. Most importantly I have suggested in some interviews that the book could serve a constructive purpose if it raises awareness about the terrible suffering and precarious situation of native South Americans (see document http://www.unm.edu/~kimhill/tierney/health.htm detailing these current problems). However I am equally concerned that the anti-science message of the book will lead to greater suffering and death among South American Indians rather than a solution (same document).

Although I am not seeking out press interviews concerning this book, I have been motivated to write this document because of Sponsel's attempt to censor my viewpoint from the debate about the value of the book. I have worked with South American Indians for 23 years and have done nearly 120 months of fieldwork with remote Indian tribes. I have published nearly 80 articles and one book containing scientific data about the native groups with whom I worked. In particular my co-authored book (Hill and Hurtado 1996, Ache Life History) represents the most complete demographic analyses ever done of a remote South American tribe and contains a great deal of specific information about contact epidemics and the associated age specific mortality profiles of pre- and post-contact Indians as well as the disastrous virgin soil contact epidemics. I am married to a Venezuelan (Magdalena Hurtado) whose mother was a senior research scientist at IVIC (the Venezuelan Science Institute) and knew personally Neel, Chagnon and all the Venezuelan scientists who collaborated with them during the period of time covered in the book. My wife met both scientists when she was a child and is currently an associate professor of anthropology at the Univ. of New Mexico. She has collaborated in most of my fieldwork and all documents that I have written in the past about Chagnon or the Yanomamo situation (but she is not on Sponsel's list of those who should not be interviewed). I did anthropological fieldwork with my wife in Venezuela between 1982-1991, and we visited the Yanomamo area in 1988. The purpose of that visit was to consider scientific research on Yanomamo health problems, and our host was Jesus Cardozo. We stayed at the Platanal Salesion mission and visited several nearby shabonos providing medical care. We also visited several other downstream Yanomamo communities and Salesian missions, made a short trip with Cardozo and Jacques Lizot to an abandoned Shabono in a more isolated region, and visited the New Tribes settlement of Tama Tama where we talked with some protestant missionaries who worked in remote Yanomamo villages. I have personally met nearly all the main protagonists of the book including Chagnon, who I have known for nearly 20 years, and Neel, who was my colleague at the University of Michigan when I was on the faculty there (1988-1991). I have discussed many scientific issues with both of them at great length including especially some of the major themes of this book: virgin soil epidemics, sexual selection, and warfare. I have read all the primary Yanomamo literature referred to in the Tierney book and I also met and conversed (in Spanish) with some of the Yanomamo "informants" in the Tierney book, including especially Alfredo Awerohe who is mentioned many times in the book. Since Sponsel hopes you do not contact me, below are my reactions to this book.

Tierney book- comments from Kim Hill

After reading the Tierney book I was concerned about a variety of issues, from the truth of specific allegations to the motives behind publishing the myriad of obviously false allegations, and from the ethics of specific fieldwork activities described to the overall impact the book would have on the health and welfare of indigenous peoples. The book is complex and brings up many important issues that have not been well discussed in anthropology. However, unfortunately, the book is also full of false and misleading information, half-truths and deception by omission. As such it constitutes unethical journalism. It does not honestly examine the true causes of the current precarious situation of the Yanomamo and other native South Americans. Specifically, while embellishing a longstanding vendetta and self righteous ideological witch hunt against two prominent anthropologists, Jim Neel and Napoleon Chagnon, and including many highly detailed accounts of their alleged misdeeds, it remains curiously silent on the roll of the Venezuelan/Brazilian governments in failing to provide healthcare assistance and territorial protection to the Yanomamo. The book also ignores entirely, the numerous easily revealed misdeeds of several missionaries and anthropologists who constitute its main source of information against its scientific targets thus rapidly revealing a blatant and powerful bias against only a few individuals in recent Yanomamo history. Finally, it attempts to confuse the reader into believing that some Yanomamo opinions which have been coached for years by bitter enemies of Chagnon and Neel are somehow now independent assessments and representative of the Yanomamo people as a whole.

I make the following observations:

First the book is blatantly anti-science, anti-sociobiology, and anti- a specific view of warfare: the theory that warfare is important in human history and is sometimes related to mate competition. However, the book goes beyond taking a position against certain ideas, it attempts to demonize any who would dare hold ideas contrary to those of the author and his collaborators (some of whom are unfortunately anthropologists who have dishonestly represented their activities in conjunction with this book). It suggests that those who engage in scientific research with native populations are generally evil and uncaring (unlike the engaged "activist" author and his collaborators), that any engagement in general scientific research (rather than pure help) is criminal (p.43), and that sociobiologists are the wickedest of all scientists uniquely capable of anything including sacrificing the lives of their study subjects to prove their theories (p.17). Tierney on the other hand, sees himself as the ally of certain "survival groups, missionaries, and Marxist anthropologists who had opted to help Indians rather than just study them" (p. XXIII). Here his agenda is laid bare. Scientists can't possibly both study and help Indians, therefore they are evil. Only survival groups, missionaries and left leaning anthropologists really care about Indians, all others should be denounced and be punished. Because Tierney knows that he will have a difficult time convincing many readers that dedicated scientists who work in Indian lands and often provide free medical care and a variety of other types of assistance, and who often research topics designed to advance the welfare of all humans on the planet, are instead evil and serve only some military-capitalist-industrial complex and seek to gain secret support for hidden Nazi-like eugenics theories, he engages in a massive exercise of embellishment and deceit–that exercise is this book.

An overriding theme of the book is that anybody who believes that the Yanomamo engage frequently in coalitionary violence is an evil person (because the author engages in the naturalistic fallacy believing that anything which is factual in nature must therefore also be moral or acceptable, or "natural" and that certain scientific findings imply the inability to legislate away competition, p. 14). Even more evil still are those that accept that warfare was common AND entertain the idea that some violent conflicts may represent mate competition between males. The theory of sexual selection is ridiculed in this book (despite the fact that it is virtually accepted as a biological "fact" among modern biologists), and those who would believe that male traits associated with success in male-male competition are favored by natural selection are deemed equivalent to Nazis (never mind the fact that there is no other likely explanation, for example, about why Yanomamo men are larger than women in the first place). Chagnon and Neel are portrayed as genocidal maniacs because of their scientific positions on some of the above themes. The book goes beyond ideological persecution to pure academic McCarthyism (and ironically asserts that Chagnon must be a McCarthy sympathizer because he was raised in rural Michigan, p. 40).

Second, the book is full of false information. It incorrectly ascribes a measles epidemic to the vaccination program by Neel and Chagnon, and then speculates on how this epidemic was intentionally caused in order to test an incoherently presented theory that never was advocated by either Neel or Chagnon. The carelessness of this accusation and the ease with which it has been shown false since pre-publication copies of the book were released, quickly informs the reader about the malicious nature of this entire work. The book claims that certain film scenes were faked when in fact there is an overwhelming body of evidence that they were spontaneous and indeed not even fully understood by the filmmakers. It asserts that Chagnon caused high levels of conflict and warfare through his gift giving and alliance arranging activities, but bases this assertion on a bizarre theory of Yanomamo warfare which claims that steel tools are the ONLY cause of lethal conflict among the Yanomamo. That theory is so incongruent with what is known about primitive warfare worldwide that I refused to waste my time reviewing the book in which it was developed (Ferguson 1995) even after being given a free copy by a prominent anthropological journal. Warfare has been commonly reported among the Yanomamo for centuries, and is obvious in the archeological record of the Americas going back thousands of years. Although it is reasonable that some native peoples in some places and some times may have attacked other groups in order to acquire valuable western tools (just as they may kill to acquire any valuable resource), the theory that all modern native warfare is due to competition for western metal tools is absurd and panglossian. According to the theory in some cases natives attack because they have tools, and in other cases they attack because they do not have them. Still other raids take place where no tools are involved but supposedly represent conflict over hypothetical trade routes of potential access to hypothetical tools that have not yet materialized. Since all modern groups are exposed directly or indirectly to western tools or other groups who may have them or want them, virtually any recent act of violent aggression can be somehow explained as a desire for these tools. This theory however, fails to explain all the pre-European warfare in the Yanomamo, in the Americas, and around the world, and fails to explain why natives would fight for tools which they subsequently trade for wives but not be willing to fight to acquire the wives directly (or any other valuable resource). It also is completely at odds with the best direct sources of Yanomamo ethnography. The two largest ethnographic works that are uninterpreted (without any anthropological theory) storytelling about Yanomamo lifestyles are the testimony of Helena Valero (Yo soy Napeyoma, 1984) and “Jungleman’s” stories taped by Mark Ritchie (Spirit of the Rainforest 1996). Both contain numerous graphic accounts of Yanomamo warriors exterminating enemy villages IN ORDER TO steal their women. In both accounts rape of women captives is common and committed by virtually all warriors (contra Tierney). In both accounts adult men, infants and boys are systematically killed while women and female children are captured. There are accounts of arguments after successful raids on how to divide up the captured women and some of those arguments lead to lethal raiding as well. Nowhere in either book is the theme of fighting for metal tools developed by the narrators. Indeed there are no stories of arguments between raiders over who would get a specific machete or axe, and indeed the material bounty gained from most raids is never even mentioned by native informants, but the fate of captured women is detailed in page after page of narrative. Likewise, Chagnon’s hypothesis that “killers” sometimes enjoy high biological fitness has been tested in only two other South American societies and both found some support for this idea. Specifically the Robarcheck’s study of the Waorani in Ecuador showed that “killers” had more wives, and my own study of the Ache in Paraguay shows that “killers” have high offspring survival. Why does Tierney fail to mention all this evidence in these sources that he cites at times on other points. One can only conclude that he is adamantly committed to his “modern people have caused Yanomamo warfare” worldview and is not an “objective journalist”, but an “advocate” as he himself claims (p.XXIV). If so he has no business stepping onto the turf of academic debate because he is not an honest broker of information.

Third: The book fails to honestly examine the plight of the Yanomamo and the causes of current suffering. The Yanomamo are loosing land and being invaded by gold-miners. This process has happened all over South America many times, beginning 500 years ago (read “Red Gold” by John Hemming for good historical overview). The suggestion that Napoleon Chagnon has had any affect on the process would be laughable if the assertion were not so malicious. Invaders have killed and enslaved Indians regardless of whether they were thought to be warlike or peacelike. Indeed, if anything the “warlike” characterization might help to keep a few timid explorers out of the area. The true responsibility for this tragedy however rests squarely on the institutions that are capable of stopping it. This means primarily the national and departmental governments of the two countries where the events have taken place. A second major cause of Yanomamo suffering is their health situation. If they are anything like other South American native groups they are suffering from high levels of tuberculosis, malaria, respiratory infections, diarrhea, misc. infectious diseases and parasites. This is exacerbated by relatively poor nutrition. Again, no small group of anthropologists could possibly remedy this situation or be held responsible. Why doesn’t Tierney investigate and report on the lack of governmental assistance in this area. Why doesn’t he use his investigative skills to uncover what happened to the millions of dollars that were allocated to the Venezuelan Indian agency (DAI) that never reached the native communities for which they were intended. Why doesn’t he investigate the causes of low monetary allocation to any indigenous assistance programs in Venezuela and Brazil and the rampant corruption that keeps the small amount allocated from ever reaching the target populations. The tenor of his book suggests that he is more interested in “punishing” a few evil scientists (ie. those who hold different ideological or political views from his own) than in uncovering the causes of “Darkness in El Dorado”.

Fourth: The hoax that Tierney and his collaborators have propagated with this book will have serious negative impact on the indigenous populations of South America. To the extent that Tierney's lies are successfully spread (and we can be certain that well known anti-science, anti-sociobiology, and anti-American groups will do their best to publicize the false accusations of this book), native populations may be convinced not to allow scientific research on their communities. This will unfortunately remove them from many of the benefits enjoyed by the rest of the developed world and hinder any attempt to find answers to important questions about native health issues. In this book for example Tierney attempts to denigrate Jim Neel's ideas about disease resistance in native populations. In short Neel believed that much of the disease susceptibility of newly contacted Indians was due to lack of immune system responses that should be developed during childhood exposure rather than genetic susceptibility. Tierney asserts that Neel's ideas are not accepted by scientists working in the area. That is flat out false. Instead there is a good deal of information suggesting that Neel was indeed right. Most isolated Indian groups die from virgin soil epidemics because of the lack of a developed immune response not because of a genetic inability to combat the diseases. This is why native communities have fairly good survival rates from infectious disease epidemics once they engage in long term peaceful interactions with the outside world. It is also congruent with the age-specific mortality patterns during virgin soil epidemics. The only published study of this that I am aware of is in my co-authored book on Ache demography. In that study we showed that mortality was particularly high only among the old and very young during contact epidemics, and that most of the young victims died from lack of parental support (food and care) rather than the effects of the disease. The mortality rate among those with active and developing immune systems who are no longer dependent on parents (ie. young adults) is many fold lower than for other age categories. This was precisely what Jim Neel had predicted would be found, and his ideas about native disease resistance rather than being lunatic fringe (as Tierney implies) are in fact very congruent with all available evidence.

Fifth: The book contains some incredible judgmental hypocrisy. Aside from the false accusations of intentionally causing an epidemic, nearly all other activities of which Chagnon is accused have been committed by Tierney himself or the Chagnon critics cited throughout the book. Chagnon is accused of visiting isolated Yanomamo communities and potentially spreading dangerous infectious diseases. Tierney himself also visited remote villages and endangered the people there (as did many other of the Chagnon critics). Tierney claims to have undergone a period of "quarantine" prior to visiting isolated villages but gives no details about how this was accomplished. I suggest this quarantine was ineffective since it would have required a long enough period to ensure that he carried no slowly incubating infectious diseases and then followed by a complete lack of interaction with mission residents and other outsiders after the quarantine. It would be almost impossible to do this in the environment of the upper Orinoco since one must prepare and obtain supplies etc, just prior to leaving, and social encounters are almost inevitable. More importantly however, Tierney admits to taking a half dozen or so Yanomamo from the Platanal mission with him on his journey to the remote villages. It is quite clear that this group did not undergo quarantine because the original plan was for many of them to return before reaching the isolated villages, but instead they decided to accompany Tierney. Likewise Tierney accuses Chagnon of having caused or exacerbated Yanomamo conflict through his gift giving patterns. However, Tierney too, provided gifts to Yanomamo hosts as he traveled (as have all the Chagnon critics cited in the book). How does Tierney know that HIS gifts caused no conflicts but Chagnon's gifts did? Tierney also accuses Chagnon of not spending enough time effort and resources in treating Yanomamo illnesses that he encountered. I know that Chagnon took medicines with him each time he went to the field. Did Tierney spend more money on medicine than Chagnon during his field trips? Did Tierney ever leave any sick or suffering individuals in a village when he moved on to do his journalist "work" elsewhere? Did the Chagnon critics provide more medical care than Chagnon? I know this is not true for some primary sources in Tierney's book because I was in the field at a Salesian mission where there was no medical care during my entire stay and some Chagnon critics cited in this book that I observed in the field gave no medical treatment to any Yanomamo during my stay (they watched my wife and I do it). Indeed, some had no training that would have allowed them to give treatment. Finally, Tierney accuses Chagnon of profiting from and thus exploiting the Yanomamo. While it may be true that Chagnon obtained important career and economic gain from his relations with the Yanomamo there can be little doubt that this is also exactly what the Tierney book is all about. Why all the hype and media attention for this book? Does Tierney plan to donate his profits to some Yanomamo development fund?

Sixth: The book contains abundant malicious personal information about Neel and Chagnon (including totally unsubstantiated hearsay) but no personal information about Tierney's primary informants who are bitter enemies of Chagnon. It is not hard for anyone who travels in the Yanomamo area to discover dirty little secrets and rumors about several of the anthropologists and missionaries who are sources of Tierney's accusations. I heard a variety of highly detailed accounts from the Yanomamo themselves. I have no doubt that if I returned to the area I could collect tales about Tierney's behavior as well. Indeed any 11 year investigation (as Tierney claims to have carried out on Chagnon) into any normal human being will reveal errors, misjudgments, imperfections, and regrettable behaviors. We may all be perfect in hindsight, but there are no Saints working in the upper Orinoco, and apparently even fewer in investigative journalism. What purpose do these personal smear tactics serve other than to further a nasty political and ideological vendetta. Jim Neel and Napoleon Chagnon are human beings with families. They worked a lifetime to build reputations that Tierney intends to destroy with this book. One would think that to perform such an "execution" of an entire lifetime of work, the judgement should be based on the highest standards of evidence. Tierney has proclaimed himself judge, jury and executioner in this act of career destruction. His evidence far from being "beyond a reasonable doubt" is instead a shoddy collection of distortions, exaggerations, misrepresentations and fabrications.

In summary, although the Tierney book raises important issues about anthropological fieldwork ethics, policies toward remote and isolated indigenous populations and the current state of native South Americans, the false accusations, ideological persecution, and sheer maliciousness of this book undermines much of the good that could have come from reporting about the Yanomamo situation.

Appendix VII: Email from Peter Biella on 'staged' films

The following is an email from Peter Biella regarding allegations that the ethnographic films of Asch and Chagnon were largely staged (with a brief introduction by Ray Hames, another anthropologist who has worked extensively with the Yanomamö).

From: Raymond Hames
Date: Wed Sep 27, 2000 6:51pm
Subject: The Ax Fight a Film Maker's Response

Dear All,

Peter Biella recently sent this around cyberspace and he has given me permission to post it on this list.

Peter is an anthropology film maker who has worked with Tim Asch. In addition, he is the one who put together "Yanomamo Interactive", a CD-ROM that is available with Chagnon's fifth edition of the "Yanomamo". As you may know, Tierney in "Darkness .." claims that the footage to the famous "Ax Fight" film was staged. Below is Biella's evaluation of the claim. (Realize that he has not read the book, only the Sponsel/Turner characterization of it).

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From: Peter Biella

Dear Colleagues,

Many people have asked me about the recent email-borne Chagnon-Asch scandal, concerning measles, concubines and faking data in the Yanomamo films. I want to send out a preliminary response. I intend to publish a more complete version of these arguments - coauthored with Gary Seaman - in Anthropology News, the AAA's newsletter. I can only speak about the Ax Fight film - having studied it and documents concerning its history for several years. The other aspects of the email scandal do not concern Asch or The Ax Fight.

To begin, it should be remembered that during, and for more than 20 years after, the Yanomamo collaboration, Asch expressed considerable animosity toward Chagnon and his "fierce people" hypothesis. He lectured publicly decrying Chagnon's apparently univocal depictions, privately spoke to generations of students about Chagnon's selective blindness to other aspects of Yanomamo. At no time to my knowledge did Asch ever suggest that data was faked: his criticism was that the sampling was biased (that there was not enough data adequately to reveal the other side of the story. He had been unable to create a memorable depiction of Yanomamo: The Ironic and Gentle People). Sample bias and faked data are very different matters.

Although the disseminated scandal letter does not name it, apparently it is the violence depicted in the Ax Fight film that is criticized.
(As I write this letter, I have not yet seen the critique verbatim.) I cannot believe that Asch would remain silent on the essential matter of "faking data in order to film it" since he would have liked nothing better than to repudiate Chagnon's fierceness hypothesis (even if by

doing so Asch might also implicate himself either for unknowing cooperation or cupidity). Asch had repudiated the impression of fierceness given by the film long since.

The film's structure, as I argue in my introduction to the Yanomamo Interactive CD (a study of The Ax Fight film), bends over backwards to qualify and reject stereotypic impressions of irrepressible Yanomamo violence. The film is about ways that violence is muted, restrained, and non-fatal. Essentially it argues that without police, Yanomamo manage to make their system of dispute settlement work pretty well, with nobody in in this case getting very hurt. Why would the filmmakers go to the trouble of starting a fight in order to prove the existence of outrageous, uncontrolled Yanomamo violence if their purpose were to argue that the fight is restrained and relatively peaceable? Why would they include footage of the injured Torowa getting up and walking away, unsteadily but with some pride intact, relatively unharmed? Why wouldn't they cut out those three feet of film and have the narration say, "He spent the rest of his days permanently crippled by the wounds inflicted"? Faking data in a film is not difficult when all one needs to do is leave out what is inconvenient, and then add misleading narration to cover the rest.

I know a great deal about the Ax Fight film and its creation -- about all the fits and starts the filmmakers had in understanding the footage, about what happened on the filming day in Mishimishimabowa-teri, about why the fight started, about the filmmakers' false theories on its origin. I cannot conceive of making a film in which a main feature is the anthropologists' confession of confusion, when, by hypothesis, there was never any confusion at all. I have published transcripts of tape recordings that Chagnon made six months after filming (late 1971), looking at the Ax Fight rushes with the other filmmakers, still trying to figure the thing out, going back two weeks later and looking at the rushes again, taping everything he said. Knowing all this, I simply don't believe Chagnon would have gone to all the trouble of faking ignorance in the presence of his fellow filmmakers, creating a back-trail as it were for people 25 years later to discover [!?], pretending to figure out the fight, if all the time that he had actually instigated it himself - and therefore knew why the fight started from the beginning. Chagnon in particular could not possibly have anticipated how famous the film would become, and yet we would have to believe on this email hypothesis that he created obscure evidence to the contrary in 1971. It doesn't make sense. To my mind, the 1971 taped evidence confirms that at first Chagnon knew virtually nothing about the origins of the fight.

Moreover, Asch and Chagnon let the footage sit on a shelf for four years before they edited it together, released the film. Asch and Chagnon were profoundly confused (and possibly even mortified) by the misunderstandings that the footage revealed and continued to create. If the fight had been an anthropolgist-provoked concoction from the beginning, why would the filmmakers have experienced any uncertainty about going to press? Why wouldn't they simply tell any story they wished from the beginning?

For the above reasons, the criticism that the ax fight was staged for the camera strikes me as obviously and manifestly untrue.

Finally, Zandy Moore, Chair of the USC Anthropology Department, points out a problem concerning the claim in the email letter that an entire shabono was built for the filming. Such a travesty did occur, Moore says, but it was done for a Nova television production in which neither Asch nor Chagnon were involved.

That Asch is not alive to defend himself, that Chagnon's word is sure to be doubted, that Patsy Asch's protestations would at best be heresay, makes it seem important for me to air the above information and arguments.

Peter Biella
Department of Anthropology
San Francisco State University
September 19, 2000

Appendix VIII: Email from Jay Ruby on 'staged' films, etc.

The following is an email from Jay Ruby, another expert on ethnographic filmmaking and Tim Asch.

Some Hurried Thoughts about Tim Asch and Patrick Tierney

By Jay Ruby

By now readers of AAA News have seen preliminary rebuttals to Patrick Tierney's claim of misconduct on the part of Neel and Chagnon. I am interested in a minor part of this story - Tierney's critique of Timothy Asch' films.

Let me position my comments. Asch and I were friends and colleagues. I saw The Feast while in process. I was a supportive reader for his NSF grant. I have seen all his films and have taught with them for decades. I published "Out of Sync: The Cinema of Tim Asch " in Visual Anthropology Review (1995, vol. 11, no. 1:19-37) and revised the essay in Picturing Culture (University of Chicago Press, 2000). My position is that since Tierney cites the article he knows its content. Therefore the errors of fact and interpretation I found are deliberate and calculated to further his dubious assertions and not based on ignorance.

I obtained uncorrected page proofs of Tierney's Chapter 6 and 7. Recently a W. W. Norton representative has stated that "corrections" will be made prior to publication in mid-November. As the points I take issue with are minor in comparison to the accusations of massive misconduct by Neel and Chagnon, I doubt the errors discussed below will be corrected.

1. "...cinema veritè became the principal source of income for many Yamomami along the Orinoco." Page 84.

To suggest that Yamomani made a living from the "income" they received from visiting filmmakers is absurd. Chagnon and Asch distributed trade goods -metal pots, soap, machetes, etc. - hardly enough to live off of. At the time Asch filmed, the Yamomani did not have a cash economy. I doubt they do today. So exactly what filmmakers could give them that could be considered their "principal source of income" is beyond me to imagine. Overstatement is commonly used by Tierney.

2. "Napoleon Chagnon was a pioneer in this frontier of film..." Page 84.

Tierney constantly overstates Chagnon's role in the making of the Yanomami films because he wants to demonstrate that the films were part of a sinister plot against the Yanomami cooked up by Neel and Chagnon. Read the interviews with Asch in "Out of Sync" to see Asch's view of Chagnon's role. Apart from A Man Called Bee, Chagnon's role was primarily one of providing intellectual direction for these films. Asch did most of the editing without him. Tierney only discusses four of these films. A glance at Documentary Education Resources' catalog reveals there are twenty some Yanomami films by Asch. None deal with war or violence. Tierney's assumption that the motivation for making the films was to put Neel's work in a good light and to show Yanomami violence and warfare is easily disputed when one looks at the entire corpus of films.

3. "Dead Birds was Chagnon's model and he took his first footage to Harvard's Gardner for advice." Page 85.

In truth Chagnon only ask Gardner to help him locate a filmmaker and was introduced to Asch. All Tierney had to do was read Asch's interviews in "Out of Sync" to know this.

4. "Doctors at the University of Michigan who did not consider his anthropological studies to be real science constantly taunted him. He had found the Fierce People but no proof they actually fought." Page 85.

If you bother to read Tierney's own footnote (No. 17, page 342), it is clear that those doctors were critical of ethnographic methods and not Chagnon's lack of evidence about violence. These scientists were simply voicing an antipathy toward qualitative research.

5. "What ensued was a formula for Yamomami filmmaking. The way to make a successful Yanomami movie was to build a new shabono, sponsor a feast, create a new military alliance, and record a raid by the newly created power. A frequent sequel to this stock sequence was an epidemic, which might kill a quarter of the Yanomami actors." Page 88.

If this was a "formula" for making Yanomami films why doesn't Tierney cite some examples. He could have gotten a list of Yanomami films shown at the conference Rouch organized to compare Yanomami films to support this notion. He is describing the Feast and The Multidisciplinary film but not the dozens of other Yanamami films.

6. Talking about The Feast Tierney claims that "They wanted to illustrate feasting as a dangerous political-military event..." Page 88.

Tierney implies that making a film about warfare was primary to Chagnon. Chagnon, like Asch, was interested in a film that would illustrate Mauss' notion of reciprocity not violence. Asch's "Out of Sync" interviews make that quite clear.

7. Once in the field with Chagnon, Tierney claims that Asch felt that "...he was alone in the jungle with aliens." Page 88.

The quote Tierney uses to support this contention actually says "He had, it seemed to me, begun to change in the last few hours. I felt he was taking on attributes of the people he had studies (sic) so long, and it seemed I was all the more alone...[T]hey looked like a very grim bunch of friends indeed, painted black and charcoal." (Footnote 42, page 334.) Why did Tierney use the word "alien" except that it is useful as further evidence of Chagnon's character.

8. "...Asch's memoir prompted scholars in recent years to politely

question the authenticity of The Feast as the film scholar Jay Ruby did in an issue the Visual Anthropology Review dedicated to Timothy Asch." Page 91.

I know of no Asch memoir. Nowhere in my article do I "question the authenticity of The Feast." Calling me a "film scholar" and not an anthropologist is a minor mistake but indicative of Tierney's lack of interest in accuracy.

9. "It was violence and the expectation of violence that appealed to film juries and students and that gave The Feast its edge." Page 102.

There is no violence in The Feast, only a final title card stating that after the feast the two villages raided another village together. Tierney offers no evidence to support this statement. Which juries? As film festivals often circulate a statement about why a particular film is awarded a prize, it would have been possible to support of this contention, none are offered. Has Tierney taught with The Feast or discussed it with teachers? How does he know what students think? He offers no evidence to support this contention. I have taught with this film since its release in dozens of courses with hundreds of students. I have been in numerous academic settings in which the film was discussed and not once have I heard any references to "violence and the expectation of violence."

10. In talking about Asch's second trip to the Yamomani, Tierney states that he had "...orders to record a war." Page 105.

Whose orders? NSF? Considering the sometime nature of Yanomami warfare such an order is impossible to fulfill. Being able to shoot the Ax Fight was an accident of being someplace at the right time. It is just silly to think that some anonymous person or agency "ordered" them to make a film about war.

There are other inaccurate statements by Tierney - like calling anthropologists, Peter Biella and Gary Seaman "two USC film professors" (Page 117) - but with the few quotations I have disputed, it is clear that Tierney has chosen to systematically misrepresent the work of Asch to further his character assassination of Chagnon. Too bad the publishers did not bother to employ a reader or fact checker who knew something about these matters.

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http://astro.ocis.temple.edu/~ruby/opp

Appendix VIX: Letter from Professor Jane Lancaster

Darkness in El Dorado by Patrick Tierney: A case of highly selective investigative journalism

By Jane B. Lancaster, Department of Anthropology, University of New Mexico

The book, Darkness in El Dorado, by Patrick Tierney is filled with a series of accusations ranging from misconduct, unprofessional conduct, to downright illegal and immoral acts. The book is also impressively documented with 58 pages of footnotes, another 10 pages of references cited, and 90 personal interviews. The average reader is put in the position of having to accept such massive documentation as being as accurate and unbiased as such lengthy referencing would imply. However, when specific sources relied upon by Tierney are compared with the way Tierney uses them, a very different pattern emerges: one of highly selective use of sources in ways that support Tierney's main arguments and the omission of much more substantive materials which contradict him. I give only two examples from first person accounts that he relies upon heavily for particular points but ignores when they speak to others.

Spirit of the Rainforest: A Yanomamo shaman's story by Mark Andrew Ritchie Chicago, Island Lake Press, 1996, 271 pages. Tierney relies heavily on Chapter 9 of this book for the materials on Jacque Lizot in his Chapter 8, "Erotic Indians". Tierney cites Ritchie's book 12 times in support of his accusations of sexual abuse of young boys by Lizot. However, Jacques Lizot is not the main subject of Ritchie's book. Rather the book is the life story of a Yanomamo shaman, Jungleman, as told to Richie. The text of Jungleman's life story is about the behavior of the shaman as a Yanomamo warrior and his relationship to his guardian spirits. The book describes approximately 45 years of inter-village raiding beginning in 1950 and continuing to the present. These raids center on the capture of women who are repeatedly gang raped before being divvied up among their captors. These gang rapes continue even during the time period when men who have killed on the raid are going through a process of purification before reentering normal Yanomamo society. The raids lead to many deaths, especially of male defenders, and infants and male children who are murdered by the victors before their mothers are taken off. These murders include grizzly descriptions of house poles covered with babies' brains and of a toddler rectally impaled on the end of a sharp bow. It seems hard to believe that Tierney, who used this source so extensively to indict Lizot, could not have read the rest of the book about stealing women. Further stories tell about the murder or mutilation of women who try to flee abusive husbands or return to their original villages. In the 45 years of raiding described by Jungleman, the raids are motivated to gaining access to women or retrieving women who have been stolen or have run away, or for revenge for previous raids. These stories are completely opposite to the claim by Tierney in Chapter 10 ("To murder and to multiply") that Yanomamo inter-village warfare is a response to the introduction of trade goods by whites and most especially by the Neel and Chagnon expeditions. It is particularly astonishing that Jungleman's interviews are never referenced in this chapter which has 157 footnotes.

Yanoama: The narrative of a white girl kidnapped by Amazonian Indians as told to Ettore Biocca. New York: Dutton, 1971, 382 pages. This life story by Helena Valero describes her 17 years among the Yanomamo after she was captured in 1937 at the age of eleven. During her first year among the Yanomamo she changed hands three times as she was captured in raids by different villages. One of the raids included 50 women captured by the Karawatari warriors following days of flight with the Kohoroshiwetari, her first captors. When the women are finally taken by over 100 warriors, there is a slaughter of infants and male children and their mothers and female children are split up among the victors. In spite of this narrative, Tierney uses Helena Valero's story to support his claim that Yanomamo raided for trade goods not women. He states (pp. 28-29) that prior to the arrival of missionaries in the 1940's, the Namowei Yanomami had lived in peace for a generation. Their only raiding parties had gone out searching for whites in order to steal machetes. He quotes Valero in describing her first capture that "the Indians did not want to capture women, just madohe (stuff)." This quote, although used very differently by Tierney, was really her explanation that, when raiding whites, Yanomamo were seeking goods. Furthermore, Tierney neglects to recount the second and third capture of Valero when Yanomamo men were raiding strictly to capture women. Although the capture of these women and the scene of babies heads being bashed against rocks would seem to be unforgettable to the average reader, Tierney did not seem to remember them, only stories by Helena after her final capture by a famous warrior with whom she lived for many years before her escape.

Tierney's use of the Jungleman and Valero life histories to support his thesis and then totally ignoring in these same sources major eyewitness and participant accounts of women capture, rape and the murder of children is not an example of investigative journalism that we can trust. Although his documentation with footnotes and citation of sources would seem impressive, it does not hold up to straightforward tests of what the sources really say and what Tierney reports them to say. These distortions give testament to a lack of journalistic responsibility and ethics.

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Appendix X: Letter to the New Yorker from Bill Oliver, Chairman of Pediatrics, U. Michigan

RESPONSE TO PATRICK TIERNEY'S ARTICLE ENTITLED "THE FIERCE ANTHROPOLOGIST" WHICH APPEARED IN 'THE NEW YORKER' OF OCTOBER 9, 2000

The narrative description of the measles epidemic among the Yanomamö Indians by Tierney warrants careful re-appraisal against correct facts regarding the vaccine (detailed in a separate Letter to the Editor by Samuel L. Katz, M.D. [co-developer of the vaccine with John F. Enders, Nobel laureate]) and true, first hand accounts of the events. The blatant inaccuracies of fact and use of material out of context are easily evident.

The primary sources of the correct accounts were published by Centerwall (1968) and Neel (1970), augmented by entries in Neel's and Chagnon's personal field journals. These echo earlier descriptions of the devastation incurred by introduction of a highly contagious, virulent disease to a population of nearly 100 per cent susceptible individuals. Efforts to abort the epidemic by active immunization are carefully detailed. The reports of Centerwall and Neel also document, as have others, the reduction of morbidity and mortality by aggressive antibiotic treatment and skillful nursing care. It is highly pertinent to note that these publications of some thirty years ago certainly did not anticipate the current vicious attack by Tierney on the actions of these same researchers. The facts were cleanly presented without embellishments or omissions.

The true sequence of events can be best considered chronologically:

1. PROCUREMENT OF MEASLES VACCINE FOR THE YANOMAMO:

Previous studies in 1966 of the Yanomamö of Venezuela indicated a few had antibodies to measles but most had none. Accordingly, in the fall of 1967, in anticipation of the January 1968 expedition, Dr. Neel initiated requests to pharmaceutical companies and obtained 2,000 doses of Edmonston B measles vaccine plus equivalent doses of human gamma globulin from the Michigan Department of Health. He also consulted with the experts at the Center for Disease Control on the best way to administer the vaccine. His goal was to vaccinate as many as possible to prevent or, at least, blunt future epidemics among this highly vulnerable population.

2. MEASLES OUTBREAK IN BRAZIL:

In November 1967, an outbreak of measles began in the Yanomamö of Brazil. To aid in stopping the epidemic, Neel diverted 1000 doses of measles vaccine to Brazil. These were given to the Indians by physicians and missionaries working in that country.

3. MEASLES OUTBREAK IN BRAZIL:

By chance, as Neel's group arrived in Venezuela, in January 1968, measles was introduced by a young Brazilian boy to the Yanomamö at the Salesian Mission of Santa Maria del Ocamo in

Venezuela. Exposed susceptible individuals included both those Indians resident at the mission and those visiting from outlying villages. A French team of doctors with the Venezuelan physician Marcel Roche were at the mission at the time the ill youth appeared on January 23, 1968. Roche made a tentative diagnosis of measles in the boy. Two facts were well known to Roche and the French team. First, measles can be a devastating disease in a virgin population; second, administration of the vaccine within 72 hours of exposure can protect from the wild disease. Vaccine was available but gamma globulin would arrive with Neel two weeks later. It was concluded that the wisest course was to give the vaccine. The doctors vaccinated 31 Yanomamö plus nine Brazilians (page 57 of Tierney's article). Of the 21 immunized Indian children, ages two to 12 years, 17 were brought to sick call when Neel and the full team arrived. Febrile response to the vaccine was high and, as noted by Neel, "a few had a reaction indistinguishable from moderately severe measles". Importantly, he observed no significant complications and no deaths. At the mission, new cases of wild measles developed in 15 days and also appeared in surrounding villages.

4. NEEL'S ATTEMPT TO STOP OR MINIMIZE THE EPIDEMIC:

Neel arrived at the mission on February 5, 1968, He and members of his team responded quickly and responsibility in an attempt to halt the epidemic. Several teams including members of Neel's group, missionaries, and medical auxiliaries of the Venezuelan Government were dispersed to villages in the surrounding territories. The immunizing program used Edmonston B measles vaccine with simultaneous administration of human gamma globulin. The vaccine Neel brought was later augmented by additional quantities from the Venezuelan Government. There were no deaths or serious untoward events. This observation was expected from the known world-wide experience with the vaccine. Deaths occurred only in Indians suffering from wild measles. Fatalities were usually due to complications of bronchopneumonia in the absence of early and aggressive antibiotic therapy.

The orderly program of vaccination was abruptly interrupted by development of a serious outbreak of measles at Ocamo Mission. In his entry of February 17, 1968 written at Mavacca village, Dr. Neel describes an urgent request at 2:00 a.m. for help with the emergent situation. Neel and the team promptly returned to the mission that same morning. His notes detail thoughtful deliberations for developing an optimal plan for preventing or minimizing the disease and its complications. Indians not yet ill but late in their incubation phase were given gamma globulin; others given vaccine and gamma globulin. Those ill with measles and its complications were aggressively treated with antibiotics and nursing care. Additional teams were dispatched to other villages to augment those already giving immunizations plus bringing antibiotics for those already ill.

The priority given by Neel for humanitarian assistance is additionally given in his schedule for the village of Patanowa-tedi. His log notes that the first activity planned for the Indians of the village is "immunize for measles". Biomedical and anthropological studies were listed for subsequent days.

In his entry of February 25, 1968, Neel gives a summary of the measles vaccination program. Nine hundred and ninety-three doses of vaccine were given simultaneously with gamma globulin to Indians in 12 different villages. Vaccine was administered without gamma globulin only to the first forty recipients as noted previously. The logistics of getting viable vaccine in a tropical environment to villages widely separated by dense jungle and rivers with varying degrees of navigability to unsophisticated natives with high suspicion regarding foreign medicines should not be underestimated. The accomplishments in face of these difficult field conditions should not be minimized. Again, there were no deaths or serious complications associated with the immunization program, with or without gamma globulin.

5. DEATH OF AN INFANT:

In his article (page 57), Tierney employs a highly dramatic introduction to his perception of events ending in the death of a one-year old infant. His report is factually incorrect. Dr. James Neel's meticulously comprehensive entries in his personal field journal for February 6, 1968 and February 17, 1968, written 32 years ago, give the true sequence. These are his on site observations. The summary segment of the entry for February 17, 1968 is titled "Measles at Ocamo" and ends with the phrase: "Story put together with French group at Ocamo on 16 February 1968."

First, it was noted that Vitalino Baltasar was a 21-year old Brazilian, a friend of the boy with the first case of measles, not a Yanomamö Indian. In his formal report of the measles epidemic (Neel [1970], reference 14), he wrote "Both Brazilians (i.e., the boy and Baltasar) were typical 'caboclos', probably of mixed Indian, Negro and Caucasian ancestry."

In the entry for February 6th, Neel wrote that Vitalino Baltasar and a 30-year old Yanomamö male sought medical care on the night of February 5th. Dr. Neel and the second physician, Dr. Willard Centerwall, described both to be very febrile (39-40º C.), with intense conjunctival injection (red eyes), and rashes atypical for measles. The diagnosis was not thought to be measles. Both given penicillin by injection. Baltasar was seen two days later by Dr. Poiviere, a French physician working at the mission, still with injected eyes but also with signs of pneumonia. The antibiotic, terramycin, was given.

On February 13, 1968, Baltasar brought his one-year old son, Roberto, to the Ocamo Mission for treatment. Neel wrote that the infant had a very high fever, intense conjunctival injection, extreme shortness of breath and findings of pneumonia but no rash. He was given penicillin, terramycin, a cardiac stimulant and quarantined in the infirmary. Following a short phase of improvement, his condition deteriorated. He died on February 15, 1968.

There is no record of Vitalino Baltasar or his son receiving measles vaccine. In retrospect, it is likely that both had wild measles, but atypical for absence of a classical rash.

The Mission was not the only site of wild measles at that time. Chagnon in his entry of January 31, 1968 recorded that he arrived at Mavaca and the missionary, Danny Shaylor, was absent. He had become involved in taking the remains (ashes) of a Yanomamö boy, age 17 years, who had died of measles in the village of Tamatama, near Ocomo mission, back to the boy's home village.

6. CONFUSION BETWEEN WILD MEASLES AND TOXIC REACTIONS TO VACCINE:

In many villages, the immunizing teams were preceded by Indians returning to the village after exposure to wild measles. The long incubation of measles (10-12 days) resulted in asymptomatic travelers returning home and only then breaking out with the disease. Thus, in villages distant from the mission, simultaneously there could be the presence of wild, virulent measles disease and the milder but toxic reaction to the vaccine. An understanding of the distinction between the two clinical pictures might not be apparent to trained observers much less to these frightened Indians. Characteristics of measles, including its transmission by droplet spray, its relatively long incubation period plus appearing as a new disease in the experience of the Indians, all contributed to misconceptions. Far easier to incriminate those giving the vaccine and the vaccine itself as the causes of illness and death than to believe that seemingly healthy Indians could bring a severe and often fatal disease back to the village. This erroneous interpretation would clearly explain the entries in the mission journals of an association between visits of Neel's immunizing teams and outbreaks of wild measles (page 57 of Tierney's article).

The total absence of communicability of the vaccine appears to have escaped recognition by Tierney and those whom he quotes.

7. TREATMENT OF COMPLICATIONS OF MEASLES:

Dr. Neel's expedition brought in a large quantity of medicines for dispensing to sick Indians. This was Neel's standard operating protocol. 'Sick call' occurred daily. Illnesses were always treated prior to any biomedical studies. In this instance, the aggressive treatment of the Indians with bronchopneumonia complicating the wild measles was successful. However, the number of cases of pneumonia, exceeding 35 per cent of those with measles, rapidly depleted the antibiotic supplies of the team. Thus, the request to Caracas by the radio operator, Rousseau, was logically for additional antibiotics to treat the secondary pneumonia, not for drugs to treat the primary measles (page 58 of Tierney's article).

8. REDUCTION OF DEATHS FROM MEASLES:

The fatality rate for measles among all the Yanomamö was 8.8 per cent. This is high by standards of civilized societies, but low in comparison to the usual death rate observed in Indians. The lower rate most likely could be attributed to the intensive antibiotic therapy and nursing care given by missionaries, government auxiliaries and members of Neel's team. Fatality rates over 25 per cent have occurred in similar epidemics when care was unavailable or given late. In the majority of cases, deaths were due to the secondary pneumonia. In contrast, fatal complications do not occur in association with measles vaccine.

9. CONVERSATIONS DURING FILMING OF MEASLES VICTIMS:

The described exchange between Timothy Asch, the expedition photographer, and Neel is one blatant example of material taken out of context by Tierney (page 58 of Tierney's article). This was not a callous comment by an uncaring investigator. It was made in the course of taking movies to document the impact of a formerly termed 'childhood' disease' of acculturated societies on all generations of a previously inexperienced group. In this instance, the conversation focused on Neel's efforts to confirm extreme examples of the disease occurring simultaneously in three generations plus the range of illness from extreme to mild. Neel's comment, "We're going to document the whole gamut of measles in this group" reflects this goal. Importantly, these films also illustrate the clinical picture confronting those natives not yet ill with the disease. The magnitude of physical misery recorded in these movies helps to explain the Indians usually ill-fated attempts to escape disease by retreating to the jungle.

In summary, the above comments focus on a scant few of the incorrect and distorted statements which characterize the article in the New Yorker by Tierney. Only a longer response could permit a complete detailing of these blatant untruths which unfairly damage the reputation of James V. Neel and his colleagues.

(A full list of supporting references and sources is detailed on the University of Michigan web site http://www.umich.edu/~urel/darkness.html)

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Appendix XI: Original email from Dr. Samuel Katz to Bill Oliver

The follwing is an email written by Dr. Samuel Katz to Dr. Bill Oliver (a pediatrician who worked with Neel on Yanomamo health projects–see Appendix VII), after Dr. Oliver asked him to review the original published report on the 1968 epidemic.

Bill: I was able to locate James Neel's paper in the American Journal of Epidemiology (1970; 91: 418-429). Not having previously read it, I found it very interesting. The reported results are not unexpected. He obviously was trying to abort a measles epidemic already in progress by administration of vaccine. A number of comments are due.

First of all, he was using two different Edmonston B vaccines, one grown in chick embryo cell cultures, the other in canine renal cell cultures. The latter was later abandoned as it was more reactogenic than the chick cell material, but it was licensed by FDA.

A number of studies had shown and have subsequently been reaffirmed that if vaccine is administered within 72 hours of exposure, one can obtain a vaccine response and abort the wild virus illness. Thus he was undoubtedly dealing with a mixture of natural measles and vaccine-induced responses. In the absence of virus isolations and (then unavailable) genomic characterization it would be difficult to segregate the two.

"We" and other investigators had studied previously the responses to Edmonston B vaccine in children in developed nations as well as those in developing lands (Haute Volta--now Burkina Faso, Nigeria, among others) in infants and children with malnutrition, protein depletion, malaria and other underlying problems. Several results were consistently observed: the children responded with excellent antibody levels (often greater than their more fortunate contemporaries in developed nations), although they had febrile responses they remained well and active, there was never any transmission of vaccine virus to susceptible contacts who were controls receiving placebos. Despite every attempt to domonstrate communicability of the vaccine virus, it has never occurred in any populations of the many studied.

Although there was the morbidity described with Edmonston B vaccine (especially when used without gamma globulin)--fever, occasional URI symptoms, evanescent rash--there were never any severe complications such as those observed with natural measles (especially bronchopneumonia, gastroenteritis, croup, otitis media, encephalitis, etc.).

Despite the administration of millions of doses of vaccine to children throughout the world, the only deaths known to have occurred were in several youngsters who were under intense therapy for their leukemias and more recently a young adult with AIDS. These patients developed the giant cell pneumonia that has been seen with natural measles.

In summary measles vaccine viruses (Edmonston B, Moraten, Edmonston Zagreb,and any other descendents of Edmonston) have never been shown to be transmissible from a vaccine recipient to a susceptible contact. Except for the rare instances noted above they have not been responsible for deaths despite the administration of hundreds of millions of doses throughout the world. Before the availability of vaccine, WHO estimates there were 6 million measles deaths annually among infants and children. WHO's estimate for 1999 with increasingly widespread use of vaccine globally was 800,000 deaths. After the successful elimination of polio, measles is next on WHO's agenda for attempted eradication.

In hopes these lengthy comments assist you in your current endeavors, and please feel free to contact me if there are further questions--Cheers, Sam

Appendix XII: Statement read by Professor A. Magdalena Hurtado at the AAA meetings

Statement read by Professor A. Magdalena Hurtado at the "Research Among the Yanomami" panel, American Anthropological Association meeting, San Francisco, November 16, 2000

The epidemiology of infectious diseases among South American Indians

As we speak, many Yanomamo and other South American Indians are ill and dying from preventable diseases. At the same time, many other indigenous groups await contact with outsiders, and when it finally occurs, a huge fraction of them will die, again, from preventable causes. Why is this epidemiological profile so bleak at a time when the causative organisms of this suffering are well known, and when effective drugs and vaccines are available to prevent it? What forces continue to deprive South American Indians of the scientific knowledge and technology that protects many other people from pathogens including everyone in this audience? I suggest that these forces are complex, that to understand them and influence their course we need to do more, rather than less research, and that the anti-science views that Patrick Tierney promotes in his new book have the potential to unjustly deny indigenous people of South America the right to combat health problems through scientific research and interventions.

Tierney's book promotes anti-science views by falsely accusing scientists of unethical experimentation among the Yanomamo. His charges have been refuted by National Academy of Science specialists and the University of Michigan in widely distributed documents, and I will not discuss them here. Instead I will focus on how Tierney's book promotes anti-science views by giving the false impression that the causes of poor health status of the Yanomamo and other indigenous people are simple and easy to identify by anyone without carefully designed epidemiological and medical research. Tierney suggests that only treatment but not research is justifiable with indian populations. To the contrary, I will argue that in order to intervene effectively, and in ways that truly benefit South American Indians, a great deal of research needs to be done on relationships between social inequities and the uniqueness of the indigenous immune response to infectious diseases. Indeed, the book that needs to be written in order to help the Yanomamo and other Amerindians should be entitled Darkness in El Dorado: how governments, international agencies and scientists can help reverse the devastation of the Amazon.

Scientists can help by identifying the social causes of such devastation and by collaborating with international teams in efforts to counter their effects. One of the most important and well-known causes of indigenous suffering is governmental unresponsiveness to land rights violations and the increasingly precarious health status of native populations. Throughout South America, local governments allocate meager and inadequate resources to indigenous help programs and only a fraction of these resources is ever seen by native communities due to rampant corruption and embezzlement. Furthermore, laws that protect indigenous rights are infrequently implemented. For example, in 1986, I reported to the Direcccion de Asuntos Indigenas of Venezuela that only 1 indigenous land title was legitimate out of 152 that had been initially decreed by President Rafael Caldera in 1972. Several months later, Peruvian government officials threatened to expel my husband and I from our field site because we had treated the sick during a massive respiratory epidemic in a remote Machiguenga village. In 1991 several of our Hiwi Indian collaborators were murdered without cause by Venezuelan nationals. All these events were reported to government officials with no response. They were ignored along with many other instances of wrongdoing observed by us and countless other anthropologists. Darkness in El Dorado did not come from actions of a geneticist, a sociobiologist and a filmmaker in one tiny corner of the Amazon. It has been produced through hundred of years of racist colonialism and neglect. The devastation of the Amazon will only stop when governments and international agencies respond to human rights violations in an effective manner with the help of scientists.

Scientists in fact have a special role in this process–they can also help by isolating biological causes of poor health status. Biological anthropologists have repeatedly shown that South American Indians are highly susceptible to diseases of contact. In fact, the mortality rate of Amerindians at contact due to the exposure to measles is more than a hundredfold higher than in other populations even when medical help is provided to them. Much less attention has been given to the susceptibility that lingers on for many generations after contact. Recent large surveys of rural populations of South America show that natives invariably suffer from worse health than do their neighbors of European and African descent. Biological influences may account for these poor outcomes during and after contact. Inefficient immune response due to high genetic homozygosity and extensive macroparasitic infection are potentially two of the more important contributors. Unlike other populations, South American natives have much less heterogeneity in loci that control the immune system, and this low heterogeneity increases the risk of many infectious diseases as well as their severity. In addition, immune responses against hookworm and ascaris, two very prevalent macroparasites, undermine immune responses against introduced diseases like malaria and tuberculosis. This is because the exposure to macroparasites activates the T-helper 2 cell pathway which is less effective than, and antagonistic to the Th1 pathway which stimulates macrophage production to combat mycobacteria and plasmodia. The effects of these persistent and ubiquitous causes are not augmented by the presence of a few scientists in indigenous communities as Tierney claims, but rather by chronic social inequities.

In populations whose adults have never been exposed to nonindigenous pathogens, and who have therefore never developed immunity to them, homozygosity and macroparasitic infection probably exacerbate the effects of infectious diseases on the high mortality that natives experience at contact. To this day, first contacts result in the deaths of between one-third to one-half of native populations within the first five years. Half the Xikrin Indians of Brazil and the Ache of Paraguay died at contact in the 1960s and 1970s. In the state of Rondonia in Brazil, 600 out of 800 Surui died within 6 years of contact in the 1980s. In spite of these well documented examples, in October of 1996, FUNAI officials set out to contact the Korubo, one of 50 groups who still live in isolation in Brazil, with a team of 26 individuals none of whom were medical personnel. But the contact team did include 8 journalists including representatives from National Geographic Magazine. The next year the Korubo attacked a FUNAI follow-up team and they have not been seen since. All the Korubo at that contact could now be dead from diseases introduced by the contact team—all because the scientific literature on contact epidemics was ignored.

After contact is made, biological influences interact with social factors such as sedentism, poverty, and poor access to health care to produce an ever increasing deterioration in health status. Without scientific research on these populations we will never know the full extent of this deterioration, nor how to prevent it. Among the Ache of Paraguay, infant mortality increased over a period of 10 years after contact in the late 1970s. Over that time period, the Ache also became exposed to tuberculosis possibly for the first time in their history. Within fewer than 10 years, the prevalence of active tuberculosis increased from less than 1% to 16% - one of the highest prevalences ever reported for any human population. Some 1800 miles away in Brazil, the Yanomamo are experiencing a similar fate. Within fewer than 15 years, over 6% of the population in some villages became very ill with active tuberculosis during the late 1980s. Because of scientific research, we now have some clues as to why the Yanomamo experience such high rates. A study shows that unlike their Brazilian neighbors, the Yanomamo mount unusually high antibody responses instead of the cell-mediated defense that is most effective in containing mycobacterial infection. This could also be part of the explanation for why the measles epidemic that Neel observed among the Yanomamo in the 1960s was so devastating, and why so many other indigenous groups are similarly affected.

Recent tuberculosis epidemics clearly illustrate that more, rather than less, medical and epidemiological research is imperative to save the lives of indigenous people over the next century. Along with the Ache and the Yanomamo, the Cuna of Panama, the Taraumara of Mexico, the Shuar of Ecuador, the Maka of Paraguay, and many other groups are now becoming breeding grounds of drug resistant Mycobacterium tuberculosis. South American Indians are reliving the experiences of North American natives in the early 1900s. The difference is that we now have ways to cure tuberculosis, and yet the outcomes are currently just as devastating. Indigenous susceptibility to mycobacteria in combination with resource-poor and inadequately managed Word Health Organization-sponsored programs are exactly the sort of conditions that promote the emergence of drug resistance tuberculosis, a sure death sentence in developing countries where specialized treatment costs are beyond anyone's means. Indigenous people will not survive drug resistance tuberculosis without research designed to find resistant bacilli and health care interventions designed to eradicate them.

Thus, I conclude that international and national guidelines for research among native peoples should be based on scientific understanding of infectious disease epidemiology to serve humanitarian ends. To do so, the field of anthropology needs to denounce anti-science propaganda that attempts to convince indigenous people and Latin Americans to ban scientific research in their communities. These views seem motivated by a desire to control indigenous populations by keeping information and alternative viewpoints from them. That is, these views constitute "colonialism by deception." Adherence to these anti-science views will tragically single out South American Indians as the one population on earth that would give up entirely the right to medical research and intervention, a benefit that is amply enjoyed by people of the First World and its enclaves overseas. This is already taking place. In the 1980s, in the country of Peru we were denied permission to obtain medicines for a remote Machiguenga village because an anti-science local anthropologist who controlled scientific research permits believed that Indians should be allowed to die without modern medical treatment. According to this anthropologist, to do otherwise would constitute meddling with natural and harmonious forms of population control.

I reject the subtext of the Tierney book that indigenous groups and national governments should severely limit scientific activity in native communities. Instead, the field of anthropology needs to make a concerted effort to put First World scientific research to good use by serving humanitarian ends among indigenous people. This can be done by encouraging interdisciplinary research and partnerships with governments in order to end human rights violations and to promote larger investments in native communities. Reporting mechanisms need to be established with international and national authorities that will be responsive to human rights abuses. Scientific partnerships with indigenous people should also be advocated as the Escola Paolista do Medicina has done in Xingu Park over the past 30 years, and research guidelines should ensure that natives can clearly identify fieldwork activities as positive for their communities. Anthropologists and medical scientists should not continue to respond to the plight of indigenous people with silence and complacency but rather with proactive plans of action that include carefully though-out research on indigenous health.

A. Magdalena Hurtado
Kim Hill
Hillard Kaplan
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Appendix XIII: Excerpt from Chagnon's monograph on collecting genealogies

From *Yanomamö* by Napoleon Chagnon (5th Edition, p. 19-27)

(note: the following was scanned; there are probably typos. Chagnon tells essentially the same story in all five editions of his monograph. The later versions are the most detailed.)

Collecting Yanomamö Genealogies and Reproductive Histories

My purpose for living among the Yanomamö was to systematically collect certain kinds of information on genealogy, reproduction, marriage practices, kinship, settlement patterns, migrations, and politics. Much of the fundamental data was genealogical-who was the parent of whom, tracing these connections as for back in time as Yanomamö knowledge and memory permitted. Since 'primitive' society is organized largely by kinship relationships, figuring out the social organization of the Yanomamö essentially meant collecting extensive data on genealogies, marriage, and reproduction. This turned out to be a staggering and very frustrating problem. I could not have deliberately picked a more difficult people to work with in this regard. They have very stringent name taboos and eschew mentioning the names of prominent living people was well as all deceased friends and relatives. They attempt to name people in such a way that when the person dies and they can no longer use his or her name, the loss of the word in their language is not inconvenient. Hence, they name people for specific and minute parts of things, such as 'toenail of sloth,' 'whisker of howler monkey,' and so on, thereby being able to retain the words 'toenail' or 'whisker' but somewhat handicapped in referring to these anatomical parts of sloths and monkeys respectively. 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 (Biocca, 1970; Valero, 1984). The sanctions behind the taboo are more complex than just this, for they involve a combination of fear, respect, admiration, political deference, and honor.

At first I tried to use kinship terms alone to collect genealogies, but Yanomamö kinship terms, like the kinship terms in all systems, are ambiguous at some point because they include so many possible relatives (as the term 'uncle' does in our own kinship system). Again, their system of kin classification merges many relatives that we 'separate' by using different terms: They call both their actual father and their father's brother by a single term, whereas we call one 'father' and the other 'uncle.' I was forced, therefore, to resort to personal names to collect unambiguous genealogies or 'pedigrees'. They quickly grasped what I was up to and that I was determined to learn everyone's 'true name', which amounted to an invasion of their system of prestige and etiquette, if not a flagrant violation of it. They reacted to this in a brilliant but devastating manner: They invented false names for everybody in the village and systematically learned them, freely revealing to me the 'true' identities of everyone. I smugly thought I had cracked the system and enthusiastically constructed elaborate genealogies over a period of some five months. They enjoyed watching me learn their names and kinship relationships. I naively assumed that I would get the 'truth' to each question and the best information by working in public. This set the stage for converting my serious project into an amusing hoax of the grandest proportions. Each 'informant' would try to outdo his peers by inventing a name even more preposterous or ridiculous than what I had been given by someone earlier, the explanations for discrepancies being 'Well, he has two names and this is the other one.' They even fabricated devilishly improbable genealogical relationships, such as someone being married to his grandmother, or worse yet, to his mother-in-law, a grotesque and horrifying prospect to the Yanomamö. I would collect the desired names and relationships by having my informant whisper the name of the person softly into my ear, noting that he or she was the parent of such and such or the child of such and such, and so on. Everyone who was observing my work would then insist that I repeat the name aloud, roaring in hysterical laughter as I clumsily pronounced the name, sometimes laughing until tears streamed down their faces. The 'named' person would usually react with annoyance and hiss some untranslatable epithet at me, which served to reassure me that I had the 'true' name. I conscientiously checked and rechecked the names and relationships with multiple informants, pleased to see the inconsistencies disappear as my genealogy sheets filled with those desirable little triangles and circles, thousands of them.

My anthropological bubble was burst when I visited a village about 10 hours' walk to the southwest of Bisaasi-teri some five months after I had begun collecting genealogies on the Bisaasi-teri. I was chatting with the local headman of this village and happened to casually drop the name of the wife of the Bisaasi-teri headman. A stunned silence followed, and then a village-wide roar of uncontrollable laughter, choking, gasping, and howling followed. It seems that I thought the Bisaasi-teri headman was married to a woman named "hairy cunt." It also seems that the Bisaasi-teri headman was called 'long dong' and his brother 'eagle shit.' The Bisaasi-teri headman had a son called "asshole" and a daughter called 'fart breath.' And so on. Blood welled up to my temples as I realized that I had nothing but nonsense to show for my five months' of dedicated genealogical effort, and I had to throw away almost all the information I had collected on this the most basic set of data I had come there to get. I understood at that point why the Bisaasi-teri laughed so hard when they made me repeat the names of their covillagers, and why the 'named' person would react with anger and annoyance as I pronounced his 'name' aloud.

I was forced to change research strategy-to make an understatement to describe this serious situation. The first thing I did was to begin working in private with my informants to eliminate the horseplay and distraction that attended public sessions. Once I did this, my informants, who did not know what others were telling me, began to agree with each other and I managed to begin learning the 'real' names, starting first with children and gradually moving to adult women and then, cautiously, adult men, a sequence that reflected the relative degree of intransigence at revealing names of people. As I built up a core of accurate genealogies and relationships-a core that all independent informants had verified repetitiously-I could 'test' any new informant by soliciting his or her opinion and knowledge about these 'core' people whose names and relationships I was confident were accurate. I was, in this fashion, able to immediately weed out the mischievous informants who persisted in trying to deceive me. Still, I had great difficulty getting the names of dead kinsmen, the only accurate way to extend genealogies back in time. Even my best informants continued to falsify names of the deceased, especially closely related deceased. The falsifications at this point were not serious and turned out to be readily corrected as my interviewing methods improved (see below). Most of the deceptions were of the sort where the informant would give me the name of a living man as the father of some child whose actual father was dead, a response that enabled the informant to avoid using the name of a deceased kinsman or friend.

The quality of a genealogy depends in part on the number of generations it embraces, and the name taboo prevented me from making any substantial progress in learning about the deceased ancestors of the present population. Without this information, I could not, for example, document marriage patterns and interfamilial alliances through time. I had to rely on older informants for this information, but these were the most reluctant informants of all for this data. As I became more proficient in the language and more skilled at detecting fabrications, my informants became better at deception. One old man was particularly cunning and persuasive, following a sort of Mark Twain policy that the most effective lie is a sincere lie. He specialized in making a ceremony out of false names for dead ancestors. He would look around nervously to make sure nobody was listening outside my hut, enjoin me never to mention the name again, become very anxious and spooky, and grab me by the head to whisper a secret name into my ear. I was always elated after a session with him, because I managed to add several generations of ancestors for particular members of the village. Others steadfastly refused to give me such information. To show my gratitude, I paid him quadruple the rate that I had been paying the others. When word got around that I had increased the pay for genealogical and demographic information, volunteers began pouring into my hut to ‘work’ for me, assuring me of their changed ways and keen desire to divest themselves of the ‘truth’.

Enter Rerebawa: Inmarried Tough Guy

I discovered that the old man was lying quite by accident. A club fight broke out in the village one day, the result of a dispute over the possession of a woman. She had been promised to a young man in the village, a man named Rerebawa, who was particularly aggressive. He had married into Bisaasi-teri and was doing his ‘bride service’-a period of several years during which he had to provide game for his wife’s father and mother, provide them with wild foods he might collect, and help them in certain gardening and other tasks. Rerebawa had already been given one of the daughters in marriage and was promised her younger sister as his second wife. He was enraged when the younger sister, then about 16 years old, began having an affair with another young man in the village, Bakotawa, making no attempt to conceal it. Rerebawa challenged Bakotawa to a club fight. He swaggered boisterously out to the duel with his 10-foot-long club, a roof-pole he had cut from the house on the spur of the moment, as is the usual procedure. He hurled insult after insult at both Bakotawa and his father, trying to goad them into a fight. His insults were bitter and nasty. They tolerated them for a few moments, but Rerebawa’s biting insults provoked them to rage. Finally, they stormed angrily out of their hammocks and ripped out roof-poles, now returning the insults verbally, and rushed to the village clearing. Rerebawa continued to insult them, goading them into striking him on the head with their equally long clubs. Had either of them struck his head-which he held out conspicuously for them to swing at-he would then have the right to take his turn on their heads with his club. His opponents were intimidated by his fury, and simply backed down, refusing to strike him, and the argument ended. He had intimidated them into submission. All three retired pompously to their respective hammocks, exchanging nasty insults as they departed. But Rerebawa had won the showdown and thereafter swaggered around the village, insulting the two men behind their backs at every opportunity. He was genuinely angry with them, to the point of calling the older man by the name of his long-deceased father. I quickly seized on this incident as an opportunity to collect an accurate genealogy and confidentially asked Rerebawa about his adversary's ancestors. Rerebawa had been particularly 'pushy' with me up to this point, but we soon became warm friends and staunch allies: We were both 'outsiders' in Bisaasiteri and, although he was a Yanomamo, he nevertheless had to put up with some considerable amount of pointed teasing and scorn from the locals, as all inmarried 'sons-in-law' must (Figure 1.5). He gave me the information I requested of his adversary's deceased ancestors, almost with devilish glee. I asked about dead ancestors of other people in the village and got prompt, unequivocal answers: He was angry with everyone in the village. When I compared his answers to those of the old man, it was obvious that one of them was lying. I then challenged his answers. He explained, in a sort of 'you damned fool, don't you know better?' tone of voice that everyone in the village knew the old man was lying to me and gloating over it when I was out of earshot. The names the old man had given to me were names of dead ancestors of the members of a village so far away that he thought I would never have occasion to check them out authoritatively. As it turned out, Rerebawa knew most of the people in that distant village and recognized the names given by the old man. I then went over all my Bisaasi-teri genealogies with Rerebawa, genealogies I had presumed to be close to their final form. I had to revise them all because of the numerous lies and falsifications they contained, much of it provided by the sly old man. Once again, after months of work, I had to recheck everything with Rerebawa's aid. Only the living members of the nuclear families turned out to be accurate; the deceased ancestors were mostly fabrications.

Discouraging as it was to have to recheck everything all over again, it was a major turning point in my fieldwork. Thereafter, I began taking advantage of local arguments and animosities in selecting my informants, and used more extensively informants who had married into the village in the recent past. I also began traveling more regularly to other villages at this time to check on genealogies, seeking out villages whose members were on strained terms with the people about whom I wanted information. I would then return to my base in the village of Bisaasi-teri and check with local informants the accuracy of the new information. I had to be careful in this work and scrupulously select my local informants in such a way that I would not be inquiring about *their* closely related kin. Thus, for each of my local informants, I had to make lists of names of certain deceased people that I dared not mention in their presence. But despite this precaution, I would occasionally hit a new name that would put some informants into a rage, or into a surly mood, such as that of a dead 'brother' or 'sister'[2] whose existence had not been indicated to me by other informants. This usually terminated my day's work with that informant, for he or she would be too touchy or upset to continue any further, and I would be reluctant to take a chance on accidentally discovering another dead close kinsman soon after discovering the first.

These were unpleasant experiences, and occasionally dangerous as well, depending on the temperament of my informant. On one occasion I was planning to visit a village that had been raided recently by one of their enemies. A woman, whose name I had on my census list for that village, had been killed by the raiders. Killing women is considered to be bad form in Yanomamö warfare, but this woman was deliberately killed for revenge. The raiders were unable to bushwhack some man who stepped out of the village at dawn to urinate, so they shot a volley of arrows over the roof into the village and beat a hasty retreat. Unfortunately, one of the arrows struck and killed a woman, an accident. For that reason, her village's raiders *deliberately* sought out and killed a woman in retaliation-whose name was on my list. My reason for going to the

[2] Rarely were these actual brothers or sisters. In Yanomamö kinship classifications, certain kinds of cousins are classified as siblings. See Chapter 4.

village was to update my census data on a name-by-name basis and estimate the ages of all the residents. I knew I had the name of the dead woman in my list, but nobody would dare to utter her name so I could remove it. I knew that I would be in very serious trouble if I got to the village and said her name aloud, and I desperately wanted to remove it from my list. I called on one of my regular and usually cooperative informants and asked him to tell me the woman's name. He refused adamantly, explaining that she was a close relative-and was angry that I even raised the topic with him. I then asked him if he would let me whisper the names of *all* the women of that village in his ear, and he would simply have to nod when I hit the right name. We had been 'friends' for some time, and I thought I was able to predict his reaction, and thought that our friendship was good enough to use this procedure. He agreed to the procedure, and I began whispering the names of the women, one by one. We were alone in my but so that nobody would know what we were doing and nobody could hear us. I read the names softly, continuing to the next when his response was a negative. When I ultimately hit the dead woman's name, he flew out of his chair, enraged and trembling violently, his arm raised to strike me: 'You son-of-a-bitch!' he screamed. 'If you say her name in my presence again, I'll kill you in an instant!' I sat there, bewildered, shocked, and confused. And frightened, as much because of his reaction, but also because I could imagine what might happen to me should I unknowingly visit a village to check genealogy accuracy without knowing that someone had just died there or had been shot by raiders since my last visit. I reflected on the several articles I had read as a graduate student that explained the 'genealogical method,' but could not recall anything about its being a potentially lethal undertaking. My furious informant left my hut, never again to be invited back to be an informant. I had other similar experiences in different villages, but I was always fortunate in that the dead person had been dead for some time, or was not very closely related to the individual into whose ear I whispered the forbidden name. I was usually cautioned by one of the men to desist from saying any more names lest I get people 'angry'.[3]

Kaobawa: The Bisaasi-teri Headman Volunteers to Help Me

I had been working on the genealogies for nearly a year when another individual came to my aid. It was Kaobawa, the headman of Upper Bisaasi-teri. The village of Bisaasi-teri was split into two components, each with its own garden and own circular house. Both were in sight of each other. However, the intensity and frequency of internal bickering and argumentation was so high that they decided to split into two separate groups but remain close to each other for protection in case they were raided. One group was downstream from the other; I refer to that group as the 'Lower' Bisaasi-teri and call Kaobawa's group 'Upper' (upstream) Bisaasi-teri, a convenience they themselves adopted after separating from each other. I spent most of my time with the members of Kaobawa's group, some 200 people when I first arrived there. I did not have much contact with Kaobawa, during the early months of my work. He was a somewhat retiring, quiet man, and among the Yanomamö the outsider has little time to notice the rare quiet ones when most everyone else is in the front row, pushing and demanding attention. He showed up at my but one day after all the others had left. He had come to volunteer to help me with the genealogies. He was 'poor,' he explained, and needed a machete. He would work only on the

[3] Over time, as I became more and more 'accepted' by the Yanomamo they became less and less concerned about my genealogical inquiries and, now, provide me with this information quite willingly because I have been very discrete with it. Now, when I revisit familiar villages I am called aside by someone who whispers to me things like, "Don't ask about so-and-so's father."

condition that I did not ask him about his own parents and other very close kinsmen who had died. He also added that he would not lie to me as the others had done in the past.

This was perhaps the single most important event in my first 15 months of field research, for out of this fortuitous circumstance evolved a very warm friendship, and among the many things following from it was a wealth of accurate information on the political history of Kaobawa's village and related villages, highly detailed genealogical information, sincere and useful advise to me, and hundreds of valuable insights into the Yanomamö way of life. Kaobawa's familiarity with his group's history and his candidness were remarkable (Figure 1.6). His 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. If there were subtle details he could not recite on the spot, he would advise me to wait until he could check things out with someone else in the village. He would often do this clandestinely, giving me a report the next day, telling me who revealed the new information and whether or not he thought they were in a position to know it. With the information provided by Kaobawa, and Rerebawa, 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.

Once again I went over the genealogies with Kaobawa to recheck them, a considerable task by this time. They included about two thousand names, representing several generations of individuals from four different villages. Rerebawa's information was very accurate, and Kaobawa's contribution enabled me to trace the genealogies further back in time. Thus, after nearly a year of intensive effort on genealogies, Yanomamö demographic patterns and social organization began to make a good deal of sense to me. Only at this point did the patterns through time begin to emerge in the data, and I could begin to understand how kinship groups took form, exchanged women in marriage over several generations, and only then did the fissioning of larger villages into smaller ones emerge as a chronic and important feature of Yanomamö social, political, demographic, economic, and ecological adaptation. At this point I was able to begin formulating more sophisticated questions, for there was now a pattern to work from and one to flesh out. Without the help of Rerebawa and Kaobawa it would have taken much longer to make sense of the plethora of details I had collected from not only them, but dozens of other informants as well.

I spent a good deal of time with these two men and their families, and got to know them much better than I knew most Yanomamö. They frequently gave their information in a way which related themselves to the topic under discussion. We became warm friends as time passed, and the formal 'informant/anthropologist' relationship faded into the background. Eventually, we simply stopped 'keeping track' of work and pay. They would both spend hours talking with me, leaving without asking for anything. When they wanted something, they would ask for it no matter what the relative balance of reciprocity between us might have been at that point. I will speak of both of them-and their respective families-frequently in the following chapters, using them as 'examples' of life in Yanomamö culture. For many of the customary things that anthropologists try to communicate about another culture, these two men and their families might be considered to be 'exemplary' or 'typical'. For other things, they are exceptional in many regards, but the reader will, even knowing some of the exceptions, understand Yanomamö culture more intimately by being familiar with a few examples.

Kaobawa, was about 40 years old when I first came to his village in 1964. I say “about 40” because the Yanomamö numeration system has only three numbers: one, two, and more-than-two. It is hard to give accurate ages or dates for events when the informants have no means in their language to reveal such detail. Kaobawa, is the headman of his village, meaning that he has somewhat more responsibility in political dealings with other Yanomamö. groups, and very little control over those who live in his group except when the village is being raided by enemies. We will learn more about political leadership and warfare in a later chapter, but most of the time men like Kaobawa are like the North American Indian ‘chief’ whose authority was characterized in the following fashion: “One word from the chief, and each man does as he pleases.” There are different ‘styles’ of political leadership among the Yanomamö. Some leaders are mild, quiet, inconspicuous most of the time, but intensely competent. They act parsimoniously, but when they do, people listen and conform. Other men are more tyrannical, despotic, pushy, flamboyant, and unpleasant to all around them. They shout orders frequently, are prone to beat their wives, or pick on weaker men. Some are very violent. I have met headmen who run the entire spectrum between these polar types, for I have visited some 60 Yanomamö villages. Kaobawa, stands at the mild, quietly competent end of the spectrum. He has had six wives thus far-and temporary affairs with as many more, at least one of which resulted in a child that is publicly acknowledged as his child. When I first met him he had just two wives: Bahimi and Koamashima. Bahimi had two living children when I first met her; many others had died. She was the older and enduring wife, as much a friend to him as a mate. Their relationship was as close to what we think of as ‘love’ in our culture as I have seen among the Yanomamö.

Appendix XIV: Chagnon's recent statement on his genealogical methods

Tierney charges that Chagnon's genealogical research among the Yanomamö was a source of conflict. In fact, the material Tierney cites shows no such thing (see the section of this report on 'Naming the Dead'). Below is a recent note from Chagnon updating the account in his monograph, an account that was obviously meant to illustrate some of the problems he encountered early in his fieldwork, and was not intended to describe what he subsequently did.

December 10

During almost the entire period of my field research I used Polaroid photos in my census work. This made it very easy to avoid using the names of people, although very few of them objected when I did use their real names so long as I was discreet and whispered them in someone's ear to verify the identity of someone in the photographs. In general, the use of personal names follows a pattern. Almost everyone openly and publicly uses the names of children. The same is true of women, although one should be somewhat more circumspect and, when possible, whisper them. Boys begin to be conscious of their "status" as they approach puberty, and regard the avoidance of personal name use as a kind of measure of their importance. For example, you can flatter a young man by conspicuously avoiding the use of his personal name through a circumlocution, like "so and so's older brother" (using the name of some young child). This is a public acknowledgement that you show him some deference and courtesy, which pleases him. The more you get to know the person, however, the more acceptable it is to use his personal name...or a short version of it: "Waka" for "Wakarabewä" for example. The people whose names you generally should avoid using are people who are sick, for they do not want to have unnecessary attention drawn to them during this time. Finally, one should avoid using the names of prominent men, again for reasons of status, not simply because it is a "sacred taboo" the violation of which is "reprehensible"–it has more to do with status recognition than anything else. Indeed, some men are so confident of their superior status that they themselves use their OWN names publicly, but usually only to make some important political point or to announce some important decision.

The Polaroid photographs turned out to be a big hit. I usually had Polaroid censuses of several villages with me at any given time. The Yanomamö would borrow these field books into which I scotch taped the photos, one to a page, along with the names of the individuals plus incidental information–estimated age, sex, and comments like "...captured from Reyaboböwei-teri.." or "...promised in marriage to..." They particularly enjoyed going through the photos I had taken in other villages, especially ones to which they were closely related...and openly used the personal names of the people they were discussing. Sometimes they couldn't recognize individuals because the photos were either blurry, or there were so many people in the "family" that I had to stand further back to get everyone in the photo. They would sometimes ask me the name of the person and, looking at what I had written down next to the photo, I would whisper the name to them. There would be "Ahhs and Ohhs" of approval, indicating they recognized the person's name whether or not the image was clear. They would even correct me from time to time–"That's only one of his names: his real name is Botomawä!" And sometimes, their tone of voice was somewhat contemptuous–they seemed to resent the fact that whoever gave me the "false" name was derelict in honesty and self-righteously made it clear that I had been duped by my original source, and felt compelled to correct the error and make sure that I had the correct information. This is hardly the reaction of people who allegedly have the view that I violated a "sacred" taboo, yet Tierney labors at the argument that I "turned informant against informant" by documenting their true identities, sweetening the pie by giving machetes away to get them to "rat" on their neighbors....or that they revealed the allegedly "sacred" names only because I promised to "pay" them.

Many of the names of dead ancestors came from other kinds of interviews, particularly the long interviews focused on the political histories of the villages. In these contexts, my informants would become extremely excited about how, for example, a particular war got started, or why a particular village either fissioned or relocated. They occasionally made reference to relatives like "my uncle who was the headman there..." but their reference could have been to any one of a large number of relatives–"uncles" on both sides of the family, and "grandfathers" on both sides. They were quite candid when I would ask "...which shoabe do you mean?" They promptly whispered the name of the specific individual, and continued with their account, unperturbed by this distraction, caught up in the saga they were revealing to me. In fact, they quickly realized that their allusions to deceased ancestors in specific incidents was very ambiguous, and most of them would make clarifications like "....my 'true' shoabe...." and then whisper the man's name to make sure I knew which "shoabe" he was talking about. There was one particularly prominent man whose name was "Shinbone", and every time he came up as an ancestor or father of someone (he had 43 children and something like 400 grandchildren), my informants would simply tap their shin (or my shin) to let me know who they were talking about, avoiding the mention of his name in this way. Other people were also referred to this way, like the man whose name was "nose"–they would tap their nose to indicate who they meant.

My sets of Polaroid ID photos gave the Yanomamö many delightful hours of entertainment, and they knew that I knew the names of everyone of the people in them–and were not disturbed, let alone angered, by this. By that time, they trusted me and assumed that it was quite natural for me to know the identities of all these people.

The only "complaints" about my knowing their names comes now from Yanomamö at Salesian Missions, who coach them and encourage them (the acculturated ones among them) to "complain" that I have "violated a sacred taboo" and to justify why I should be prevented from returning to work among them. They have been made aware that "white people" will look dimly on this. Most of the complaints come from highly acculturated Yanomamö whose names I learned 35 years ago and have not used publicly since I first learned them. Indeed, I'm surprised that they even know that I know their names, but they DO know that other white people ("anthros") will find this "outrageous" and condemn me for this knowledge.

There is a good reason for this. The Salesians do not want me to ask about "dead people" and deliberately get the Yanomamö at their missions upset about this. The reason is very clear, because I can accurately document mortality rates with this information, something the Salesians would prefer that I not do. It implicates many of their destructive policies, like recent deaths caused by the shotguns and ammunition they have, for 35 years, dispensed, or specific recent epidemics that could have been avoided had the Salesians taken seriously the "rumors" that everyone in the village they recently "attracted and reduced" was sick, but never sent someone to investigate these rumors. In 1988 I visited the Salesian Mission at Mavaca with the intent of developing a collaboration with the Salesians for assigning "Christian" names, using my genealogies instead of their current haphazard way of sometimes separating all five brothers in a family by giving each one a different “last name”. While I was there, I happened to ask one of my regular informants a question about someone who had, unknown to me, recently died. He immediately clammed up because one of the “Salesianized” Yanomamö was within earshot. He later, secretly, explained to me that the Salesians had forbidden anyone to tell me about the dead–and specifically what they died from. Not long after that the Bishop of Amazonas declared that there had been enough “anthropological research” among the Yanomamö and recommended that henceforth there should be no more. We already knew enough about the Yanomamö.

Appendix XV: Chagnon's response to various allegations

This is Chagnon's response to allegations in Tierney's book regarding Chagnon's use of hallucinogens, his participation in shamanistic rituals, and the landing of helicopters in villages.

Let's now look at another tactic Mr. Tierney uses, again, abundantly. It amounts to having *others* say what he wants you to believe. Again, this gets him and his fact-checking publisher off the libel hook: he's just citing a "statement" by, for example, a Yanomamö "eye witness" to one of my alleged crimes or misdemeanors. The testimony of one "Pablo Mejia" is a typical, but not the only, use of this deceptive, dishonest tactic.

He cites a long account by this man, apparently translated for him by a member of the Dawson mission. He claims that "Pablo" is fluent in Spanish, so it might be his own translation. It should be noted, before reading what Pablo Mejia allegedly said, that the Dawson family has long since been officially separated from the New Tribes Mission because of what I take to be an argument about policy and procedures. Joe Dawson, the "patriarch" of the mission, and I had a serious disagreement in the 1970s about "tactics" he was using to convert the Yanomamö, I'll explain this after we hear from "Pablo", who Tierney says was a resident of Dawson's mission when he interviewed him…and later accompanied him on his journey to partially retrace my recent steps.

> Pablo Mejía, who at age forty-five is now literate and fluent in Spanish, first met Chagnon when Mejía was about twelve years old. "I was in Momaribowei-teri. That's the first village where Chagnon arrived after he established himself at Bisaasi-teri. He thought he would become a sorcerer [*brujo*]. In order to be a sorcerer, he asked the other *brujos* to teach him. When he arrived at the village, he had his bird feathers adorning his arms. He had red *onoto* dye paint all over his body. He used a loincloth like the Yanomami. He sang with the chant of his shamanism and took *yopo* [a powerful hallucinogen used by Yanomami shamans that alters vision and self-awareness]. He took a *lot* of *yopo*. I was terrified of him. He always fired off his pistol when he entered the village, to prove that he was fiercer than the Yanomami. Everybody was afraid of him because no one had ever seen a *nabah* [white man, outsider] acting like a shaman. He would, say, ask, 'Who was your dead father?' He said to my brother Samuel, who was the headman, 'What is your mother's name?' My brother answered, 'I don't want to say her name. We Yanomami do not speak our names.' Shaki [Chagnon] answered, 'It doesn't matter. If you tell me, I'll pay you.' So, although they didn't want to, people sold their names. Everyone cried, but they spoke them. It was very sad. I remember well because I was about ten or twelve years old. That's how things were with Shaki. He said, 'I want to be a shaman who works only for your village. Go ahead and teach me.' He would say this to the old ones, the shamans. But they were afraid. Later he went to Mishimishi, where they taught him. Shaki had his own shaman circuit. He would say, 'I am the *cacique* of all the Yanomami.' He played everything, risked everything. I'm not the only one who heard--everyone heard him. He can't deny it. When he would come to our village, all the children would run into the forest screaming with fear. I've never seen anything like it." [Pablo Mejía, interviews at Toki, Aug. 28, 1996, and at Shakita, Sept. 1, 1996; quoted in Tierney, p. 46-47]

He remembers well because he was about ten or twelve years old? The alleged events happened some 30 years ago. People--*nabahs*--place almost blind faith in the accuracy of the memory of native peoples, a mysterious and supernatural skill they apparently all have. I once, very recently, had a Ute Indian tell me, after reading my monograph, that he consulted with "the elders" of the tribe (people who were approximately my age), and they remembered vividly the Yanomamö, who passed through Utah--some 8,000 years ago--and volunteered, with input from "the elders", to help me fill in the missing pieces of Yanomamö settlement patterns. He was dead serious.

Contrary to what Pablo asserts, I can and I do deny what he said.

Let's take a closer look at Pablo's testimony. I don't know for sure the identity of Pablo. "Pablo" is a common Spanish name and, no doubt, there is more than one "Pablo". I only know the Yanomamö by their true names. The reason is simple: outside the Mission--and often within most missions--nobody knows what the "fake" names of the Yanomamö are. But I do remember a "Pablo"--he was the first Yanomamö I met, but he was then living (or visiting) Tamatama, the field headquarters of the New Tribes Mission. This was 1964. Tamatama was then the home base of the Dawson family before they "went independent", i.e., were effectively disassociated from the New Tribes Mission in a dispute about mission tactics and procedures in their efforts at evangelizing.

Joe Dawson and his family were "fire and brimstone" types, and engaged in what I consider to be unethical tactics to win the hearts and minds of their converts. They would show the Yanomamö "paintings" of Yanomamö-like people being cast over a precipice into a roaring fire, and explain to them that this would surely happen to them if they continued "chanting to their demons", taking drugs, and having multiple wives. The Yanomamö, of course, could not easily determine the difference between a "painting" and a photograph in those days. More on this below, because it is relevant to the intent of Tierney's "translator" or mission advisor of Pablo's account, presumably a member of the Dawson Mission Tierney says he originally went to the area as the guest of the Dawsons, and he attributes to them other, similar, translations of what the Yanomamö allegedly told him….through a translator. Tierney does not speak Yanomamö, so all the accounts of what the Yanomamö allegedly told him were translations…30 years after the events.

The "Pablo" I know was indeed from Mömariböwei-teri, but his name is Wakarabewä, but he might be the "wrong" Pablo. If Tierney would provide a little more information about who the true "Pablo" is, then I could be more certain if we are talking about the same man. I know him only by "Pablo" because that's the fake name that the New Tribes Missions gave to him. If he is the "Pablo" who married into Bisaasi-teri--to the daughter of Bäkäböwä and Yotoma, --then I know who Tierney is talking about. Maybe a photograph is all that is needed. The same "Pablo" accompanied Tierney on his "trek" to retrace my steps. [Did Pablo go through the "quarantine" that Tierney religiously says he made everyone with him go through to demonstrate that he was "clean" when he went to these remote villages? How did he treat Pablo's clothing. Did these get quarantined also? How about Pablo's machete? Tierney assures us that even machetes can spread contagion, but maybe he meant that only Chagnon's machetes do that.]

Regardless, Pablo's testimony is hilarious, whoever he might be. But only I know that because only I know what I did when I visited Mömariböwei-teri. Everybody else is expected to believe Pablo's testimony because, as he assures us, he was ten or twelve years old at the time and therefore "remembers it" well. How many of you would be willing to take the testimony of a ten or twelve year old boy, collected 30 years later, as valid evidence in, for example, a court case involving homicide? Tierney's accusations are about homicide.

Pablo claims that I appeared in feathers and took *yopo*, lots of *yopo* [the Spanish word for an hallucinogenic drug]. I was therefore in a state of hallucination when I visited his village, perhaps in this state more than once. Well, I hate to admit this, but I've only taken this hallucinogenic drug ONCE among the Yanomamö, which I will explain below because it is related to the motives of Tierney's translator. The time I took it was in Bisaasi-teri, not Mömariböwei-teri.

I have appeared in feathers (arm-bands and parrot feathers and also include monkey tails as headbands) that all visitors, if they want to appear to be friendly and courteous, use when they enter a new village, but this is nothing odd or reprehensible. I never carry such decorations, but they are easily borrowed from travelling companions. I may have even cleaned the mud off my body, acquired by a long trek through swampy areas--every visitor wants to look clean when he arrives. Again, this is nothing odd, but I'm sure Tierney could find a sinister motive for this.

The visits I made to Pablo's village had to have occurred between about 1966 and 1971, relatively early in my long-term field studies. In 1971 the village split into two approximately equal halves. One group made a long, arduous migration out to the Casiquiari in order to reach the New Tribes Mission (documented in Chagnon, 1974) at Tamatama. The other group eventually moved close to the mouth of the Mavaca River and have subsequently been known as the Nasikiböwei-teri. The reasons given by the group who made the long, arduous trek out to the Casiquiare were based on their claim that they felt very insecure because of the growing numbers of Yanomamö at the Salesian Mission at Mavaca who now had access to shotguns. The Salesian priest, Padre Berno, had embarked on a policy of luring the Yanomamö away from the Evangelical Missions by promising them boats, motors, and shotguns…as a conscious and deliberate strategy. Not long after this the Evangelical Mission (New Tribes) had to abandon their post at Mavaca. The same had happened at Platanal and at Ocamo, both of which were originally New Tribes Missions.

In 1966 I made my first of several trips to Mömariböwei-teri. I don't think that after the first trip I even bothered to "decorate" with my famous borrowed feathers. If Pablo was then a resident, he has the story all wrong…but has unwittingly provided Tierney with a juicy quote that he can fall back on to avoid claims of libel: Tierney was merely quoting an allegedly knowledgeable source….a Yanomamö "eye witness".

I never decorated myself with the *onoto* pigment, except when it was in the form of a concentrated mixture that could easily be washed off, and only then, I used it very sparingly--to make a few decorative dots and wiggle patterns on my face and occasionally on my chest. *Onoto* (known as *nara* to the Yanomamö , is bixa orenella) and once it is on your clothes or hammock, it is almost impossible to get out. I always avoided getting *nara* on my equipment and clothing. In the concentrated form, like lipstick, it is not called *nara*. So, to say that I painted myself "all over my body" in *nara* is a gross exaggeration.

I never would have done something so gross or discourteous to publicly "demand" that a Yanomamö tell me the name of his mother or father--or any other relative, nor do anything as grotesque as saying that "if you tell me I will pay you." Everything that "Pablo" reports about my allegedly violation of name taboos is simply and utterly false. I have commented on the use of names elsewhere (web site...).

The claim that "I wanted to be a shaman who works only for your village" is nothing short of preposterous, if not hilarious, or that I had my "own shaman circuit", or the claim that I wanted to be the "*cacique*" of all the Yanomamö. "*Cacique*" is a Spanish word that translates into something like "chief", a distinction that exaggerates the role of a typical Yanomamö "headman" and a word that only a misinformed Yanomamö would use to characterize a "headman" because that is a word commonly used by Spanish speakers….or highly acculturated Yanomamö. My command of the Yanomamö language, at the time I allegedly made these pronouncements was not sufficiently good for me to even contemplate learning about the "mysteries" of their spiritual world, let alone an alleged shamanisic "skill" in this arena.

Finally, Pablo's account cannot be "true" on another point of fact. I never owned a handgun (pistol) on any trip I made to this village. The charge that I "always fired off my pistol" when I entered the village is a complete fabrication. Pablo may have, because of faulty recollection, confused me with Kenneth Good, about whom I have heard many rumors that he frequently fired his pistol in Yanomamö villages. These, too, might also be just rumors. It may be that all "nabas" are thought to fire off pistols.

Now for a more humorous, but somewhat bizarre, twist to this story--the role of the Dawson family members in hosting Patrick Tierney and their assistance to him as a translator. Tierney, as is his wont, only cites a part of this story--the one that he believes will caste me in the most unfavorable light and do injury to me.

The "incident" occurred on one of my return field trips to the Yanomamö, in 197(0?). By then, I was deeply involved in my work among the "Shamatari", the villages to the south of Bisaasi-teri in the headwaters of the Mavaca River and regions adjacent to these headwaters. Kaobawä and his people were always disappointed when I would visit them en route to the Shamatari. They wanted me to remain with them, not merely to "prevent" me from giving my trade goods away to someone else, but also because they had become accustomed to and somewhat fond of me--and I of them. I always stayed a few days with them, out of courtesy, and to "catch up" on who was now married to who, the new babies, their plans for a new garden, etc.

On this particular trip they complained bitterly to me that "Bebiwä" (Joe Dawson) was threatening them and intimidating them because they were chanting to their *hekura*, their spiritual counterparts. Joe regarded this as paganism, born of ignorance, and their spirits as mere "demons." They told me he was going to make sure that "Dios" (God) would punish them and throw them over a cliff into a chasm of fire…because they chanted to their "demons" and took hallucinogenic snuff, both allegedly disapproved of by Dios. Besides, they used tobacco and had more than one wife, neither of which pleased Dios very much. Joe had, as the Yanomamö viewed it, a direct line to Dios--he spoke with Him daily. He was Joe's *hekura*, the chief of ALL *hekura*. Joe and his wife, Millie, would show the Yanomamö artistic renditions of Dios' power and authority: of Yanomamö being thrown over a cliff into a chasm of fire, punishment for their sins, for chanting to their "demons". If Joe Dawson accuses me of doing something that the Yanomamö don't understand and can't separate into 'spiritual' versus 'real' truth, then he is a hypocrite: the Yanomamö BELIEVED that he could, through his direct line to "God", have them thrown into a chasm of fire.

Joe's complaints were, at first, treated very diplomatically by the Yanomamö. They would take their drugs and chant to the hekura a short ways into the nearby forest, out of earshot. This worked when it wasn't raining, but eventually the inconvenience became noisome and they gradually moved their daily chanting back into the village. Their noisy chanting and singing usually brought Joe out of the house and into the nearby shabono, where he angrily berated and castigated them. It also frightened them because they weren't sure if Joe really could cast them into a chasm of fire.

It was a hot afternoon, threatening to rain. I was working on Shamatari genealogies in my hut when my two closest friends and best informants, Kaobawä and Rerebawä, came into my hut and urgently said they wanted to speak with me about “God-teri”, as the missionaries were called: “Diosi urihi teri…” People from God’s Village, the “land” of God. They were visibly upset when they explained to me that Joe had, once again, interrupted their chanting. They said he “threatened” them by assuring them that unless they gave up their chanting, God would cast them into the chasm of fire. Kaobawä held up two fingers and continued: “He said God would do this in this many *rasha* seasons.” I was annoyed and resented this kind of tactic. They asked me, sincerely, if I thought Dios might cast them into the chasm of fire. I normally do not take a stand on what the missionaries tell the Yanomamö and try to avoid answering these kinds of questions. But, this was just too much. I replied: “No. He can’t do that to you.” In Dawson’s account, I regularly dissuaded the Yanomamö from the teachings of the missionaries. Not true.

But I did not stop there. I decided to bolster their resolve and stripped down to my bathing trunks, saying: “Come on. Let’s ALL go and chant to the hekura!”

We walked over to the shabono, some 30 or 40 yards or so away. Word soon spread through the village that Shaki (my name in Yanomamö) was going to get decorated, take *hisiomö* (hallucinogenic snuff), and chant to the spirits. A buzz of excitement spread through the village at this news.

I had learned quite about shamanism from both informants and watching it almost daily. I was a tolerably good mimic and could go through some pretty convincing motions, pretending I was a shaman. Rerebawä decorated me with parrot tail feathers and made some designs on my face and chest with purple pigment. Someone loaned me a monkey-tail headband. We then walked over to Kaobawä’s house, for that was where we would take the drugs. The other men also began to decorate, with increased confidence that if I weren’t afraid of Dios, they shouldn’t be either.

We cleaned off an area in front of Kaobawä’s house where we planned to take the drugs. Rerebawä cleaned out his 3’ long tube with a long stick. I knelt and he put about a tablespoon full of the green powder into the end of it and said “Bei!” (Take it!). He stuck the other end of the tube into my nostril and filled his chest with air. I knew it would be painful. He blew a long, steady blast of air through the tube and the pain shot through my head instantly, He waited a few seconds, and refilled the tube. I began gagging and retching. “Bei!” Another blast, this time in the other nostril. The green mucus gurgled out of the first nostril and oozed down my chin and beard. I squatted, waiting for the drug to take effect. Other men lined up and, in turn, took *hisiomö*. My knees and arms felt rubbery, but pleasantly so. Blips of light began appearing. I remembered some of the songs I had heard shamans sing, and began singing to the spirits: *Ferefereriwä* and *Periboriwä*, two “hot” and “meat hungry” *hekura*. I began prancing and dancing rhythmically back and forth in front of Kaobawä’s house, singing to the hekura.

The German visitor clicked off photos of the mad anthropologist going native. A new surge of ecstasy filled me and I recall Kaobawä and the others groaning as I placed the arrows over my head and broke them, clutching the splinters tightly in my fists. A soft rain began to fall.

The village suddenly became silent for a brief moment. Through the haze I could see a stubby figure running into the village, screaming and shouting as he came. “The hekura are filthy! Dios will punish you!” And, through the same haze the stubby figure suddenly recognized the noisiest and most active sinner: it was me. Joe gawked in astonishment. I grinned at him. My arm tropismatically described a smooth, effortless arc upward, toward where he stood. I noticed that it had the bird finger rigidly and conspicuously stuck straight into the air.

My grin melted into a scowl and I felt the fire in my own eyes as I lined him up on it. Pious men don't swear, at least in their own language. After a moment, he made a vulgar Yanomamö gesture: he bared his eyeball, exposed it by pulling the eyelid down, and left in disgust. The others resumed their chanting, now confident that if didn't believe that Dios could cast me into the chasm of fire, they probably shouldn't be too concerned either.

In 1977 and 1983 I included a lengthier description of this incident in the 2nd and 3rd editions of my monograph. I decided to drop it in the subsequent edition (1992) because I felt that it was unfair to compare Joe Dawson with others in the New Tribes Mission. He was the exception, not the rule. I have a few cherished friends among the Evangelical missionaries and keep in irregular contact with some of them. They know I am a non-believer and have come to accept that. Some of them actually pray for my redemption. I used to also have a few close friends among the Salesian Missions, but they have all been put out to pasture or have died--or have been replaced with more disagreeable, politically motivated types.

But Joe Dawson and some of his family members did not forget this incident and got "revenge" on me for not only embarrassing him for how he intimidated the Yanomamö, but for also having written about it in a popular college text. His account of what happened that day is, as you might expect, puts him in glowing light and me in decidedly unfavorable light. For example, I allegedly ate some babies, a claim that is also insinuated in Tierney's book. More below. Several members of the Dawson family provided many of the translations that appear in the Tierney text.

The revenge came in a book published my Mark Ritchie in 1996. Ritchie was a successful businessman in the Chicago area and decided to be born again. He developed a friendship with the Dawson family and apparently visited their mission at Koshirowä-teri regularly (which he called "Honey" in his book). He became interested in Yanomamö culture, but in particular, the valiant mission work of people like the Dawsons and their success at converting the Yanomamö. The main part of the book is a candid, gripping account of the life of one man, a former shaman, who was converted by the Dawsons--"Jungleman". But, there are several additional chapters, one on me and the "baring the eyeball" incident. The "Pablo Mejia" whose alleged testimony is given above, also plays a significant role in Ritchie's book where he is known as "Shortman". They spell his name Pablo Mejias, not Mejia.

In general terms, the Dawson rendition is more or less as I have reported it, but they embellish it with alleged conversations and invented verbal exchanges between me and Joe Dawson. They also make it sound as though Joe Dawson got the best of me by "baring his eyeball" which they falsely turn into an incident that 'proves' that Joe Dawson's "Great Spirit" was more powerful than the hekura I invoked, otherwise I would have done something to prevent Joe from baring his eye at me. God triumphs in the Dawson account. But, they also do a very strange thing. Here is what they say happened:

> "As he danced, Irritating-Bee [Chagnon] said to Kaobawä, 'Buzzard Spirit wants to go to another village and kill a child there.'
>
> "If you do that," Kaobawä told him, "they will have to take revenge against our village."
>
> "He really wants me to do it," Irritating-Bee said, "so I'm going to." So Buzzard Spirit went with Irritating-Bee to that village and ate the spirit of that child."

Joe Dawson never saw or never heard what went on that day--except when he came into the village and I gave him the finger. He left immediately. If he said anything, I never heard it. So, the alleged conversations, attitudes attributed to the Yanomamö, "philosophical" musings by the Yanomamö about the incident, are all invented. Twenty five years later Ritchie and Gary Dawson traveled to Mavaca and "interviewed" Kaobawä and Rerebawä. Their version is largely based on this interview. Twenty five years later?

In the endnotes to this chapter there is a very long discussion--the longest single discussion among the endnotes. It makes a remarkable claim… that I actually killed a child:

> Kaobawä actually demonstrated for me the dance and chant that Chagnon used to call Buzzard Spirit with the end result, according to Kaobawä, that Buzzard Spirit killed the spirit of a child in another village, Kaobawa claimed that the child died as a result of Chagnon's and Buzzard Spirit's work.

The long end note terminates with the following triumphal happy ending: "Under the influence of Shoefoot and the people of Honey, Kaobawa has converted to Christianity, but he still expresses great regret that he was so badly misled [by Chagnon]." I "misled" him by assuring him that God could not cast him into a chasm of fire. So far as I know, God has not yet done that to him.

Tierney takes up this incident and converts it further into a component in a fantastic conspiracy. "But as Dawson explained, the distinction between physical and spiritual did not make much sense to Kaobawa. ""In Kaobawa's mind,"" Dawson said, ""and in anybody else's in that village, Shaki [Chagnon] killed that kid with the spirit, probably without the foggiest idea of what he was doing."

In the next paragraph, he expands on this: "He was alone with all this metallic wealth. Just as firing off his guns frightened the Yanomami into leaving his trade goods untouched, so his shamanistic pretensions also strengthened his hand. The Yanomami believed that white men were supernatural beings who had the power to send terrible epidemics. Chagnon's guns and claims of magical power were necessary correlates of the AEC's high-pressure research agenda."

As I recall, I only fired my shotgun once to show the Yanomamö that it was a powerful weapon…at a branch of a tree…when a group of Yanomamö threatened to kill us (mentioned in Neel, 1994). It was done calmly and without comment. To read what Tierney repeatedly and falsely claims, I was constantly shooting some gun off, an imaginary handgun in Pablo Mejia's testimony. Finally, the time I took *hisiomö* with Kaobawä and Rerebawä was the single time that I did it, hardly persuasive evidence that my 'claims of magical power' reinforced or lay behind my 'high pressure research agenda.'

But, I didn't inhale.

Then there is the tragic story of "Roberto Balthasar". Tierney had access to the records of the Salesian Missions, something I find incredible…unless they are "in" on Tierney's incessant efforts to discredit me and my research. Tierney could not possibly have gotten permission to visit the Venezuelan villages he did, unless the Salesian Missions approved of and "permitted him" there. Otherwise, he is guilty of entering the area illegally, something he constantly harps on--Chagnon did not have "proper" official permission to work in the area.

We read a touching, sad story about Roberto Balthasar early in Tierney's account. The unfortunate child was buried on the airstrip at Ocamo, a fate worse than hell--according to Tierney's tear-jerking, impassioned account. He was the first death due to the measles epidemic that I/we allegedly introduced. Tierney dishonestly conceals his identity, implying strongly that he was a Yanomamö, and allegedly died long after we vaccinated at Ocamo. You have to look carefully in his footnotes to discover that he was not a Yanomamö--he was the son of a Brazilian employee who, he implies, had been a long-time employee of the Salesian Mission at Ocamo as some kind of “administrator”. His father is misrepresented as a long-term resident of the Ocamo Mission. I’ve been to and passed through the Ocamo mission many times. I have never heard of “Vitalino Bathasar”, the father, and, I believe, I know all the employees of this mission. Vitalino was a Brazilian man, recently arrived from the Rio Negro area as far as I can determine. He was allegedly married to an “Indian woman” from somewhere on or near the Orinoco, leaving it up to and encouraging the reader to assume that she must have been a Yanomamö woman.

The points I want to make about his use of this incident are two, again, characteristic of Tierney’s dishonest reporting. First, after creating a bizarre argument that the “measles epidemic followed our footsteps” and therefore we caused the spread of it, he concludes, as his evidence, a dramatic and incriminating statement by Vitalino. Tierney put to him the question, to bolster his suggestion that the vaccine caused the epidemic, “Who gave you the vaccine?” Vitalino answered: “Napoleon.” Later, he put a similar question to Vitalino: “Who vaccinated your one year old child, who died of measles?” Again, he reports that the answer was “Napoleon”. This is strong evidence, according to Tierney, that the vaccinations we gave “caused” the epidemic and “followed us” everywhere we passed. But note that HE did not accuse me of causing the epidemic: He had Vitalino accuse me, something he can, of course, later (and so can his publisher) claim that “Tierney never accused Neel and Chagnon of starting this epidemic! Show me where he makes this accusation?” But his publisher is “puzzled” by the reaction to his book in the press--which invariably claim that “Tierney accuses scientists of deliberately causing a measles epidemic…” How did they get that idea? Well, if you read Tierney’s book, it is impossible to avoid that conclusion.

The fact of the matter, unfortunately, is that I never administered any of the vaccinations personally. There were always competent medical doctors present and I let them administer the vaccinations and I relied on their superior knowledge and understanding of diseases like measles.

But, even more curious, is the fact that the Salesians opened their records to Tierney and gave him free access to them. Yet, the best he could come up with to “prove” that our vaccinations caused hundreds, if not thousands, of deaths was the case of “Vitalino’s son, Roberto”. Why could he not demonstrate, from these allegedly detailed records, scores of deaths at the Missions? If there were the alleged widespread deaths, then surely the Mission records, if they were anywhere near accurate, would have documented them. Yet they appear to be absolutely silent on this question, probably something that came as a disappointment to Tierney. It didn’t support his fantastic conspiracy theory.

Finally, nobody on our expedition nor anyone else we advised on vaccination procedures, would have vaccinated a one-year old child. This whole story is invented.

Then there is the issue of the confusion of “Patanowä-teri” with “Mahekodo-teri” and our deliberate, if not sinister, refusal to vaccinate the people of Mahekodo-teri. Tierney has extensively used my data, often dishonestly implying that he himself produced the findings by the manner in which he cites it. Elaborate, almost an intimate familiarity, is dishonestly implied. He wants to convey the message that this is common knowledge, or perhaps esoteric knowledge that only he has. In fact, it is hard-gained information that can only be found in my writings, especially some of the comments and aspects of the geography and terrain that I documented. You cannot find this information in the National Geographic archives.

But, despite this deception, he has a very poor understanding of what he is, with some sense of personal authority, describing. He probably got to most of the villages in the Shanishani basin using a detailed map that came from me, a copy of which apparently fell into missionary hands, and thence into his hands: he gives the precise GPS coordinates, which could only have come from my map. He also uses the same spellings for the names of the villages that I used, despite adopting a totally different orthography for all other Yanomamö place names that is used by the missionaries. So, he located these villages by using my map, but fails to inform the reader of this minor detail.

He has such a poor, inaccurate understanding of geography that he does not know where the "Siapa" drainage is as distinct from the Orinoco drainage: which way the rivers flow and into what major watershed they ultimately drain. Almost none of the villages he claims are in the remote, mysterious, uncharted, uncontacted, unknown, etc. remote Siapa basin are even in the Siapa drainage. He shows a picture of one of them, taken from an airplane, that IS in the Siapa drainage, but he never got to this village.

He thinks the village of Patanowä-teri is in the Siapa basin or Siapa 'highlands', but it is only a few miles inland from the Orinoco, on the lower reaches of the Shanishani river. The Ashidowä-teri are claimed to be "in the Siapa", but they live on the same river, just a few miles upstream from Patanowä-teri. It is as if he has assumed that ALL villages south of the Orinoco are in the "Siapa Highlands".

He often confuses Patanowä-teri village with the village of Mahekodo-teri, whose residents live at the Salesian Mission of Platanal. This is often convenient and useful for his conspiracy theory, but is an error, perhaps a deliberate one. For example, he solemnly claims that the measles epidemic dogged our tracks and followed us up the Orinoco. Every Mission village we got to subsequently was victimized by the epidemic. One of the villages he lists--one out of the only three Salesian Missions on the Orinoco--he says is "Patanowä-teri". Later on, he tells us a very sad story of the reaction of the "elders" at Patananowä-teri and Mahekodo-teri (as if they were the same village) weeping when he showed them one of my films, allegedly because they saw many of their dead kinsmen in the film who he implies "died of measles." I cannot think of anything more offensive and reprehensible than to show the Yanomamö film footage of their deceased relatives, which he also did at the village of Toki on the Padamo and who knows how many other places. For someone who faults me in such self-righteous, outraged, and moralistic terms for learning the true names of people, I have never heard of such a blatant example of hypocrisy and callousness. If using names of the deceased in public is reprehensible, then deliberately showing them motion picture footage of their dead relatives is beyond despicable.

Presumably he is talking about the film *The Feast*, filmed in Patanowä-teri when the Mahekodo-teri were visiting and were the guests at the feast. He went there some 30 years after the film was shot. No doubt there were images of dead people in this film--people die. How many people did you know 30 years ago who are now dead?

Given the mortality patterns of the Yanomamö, a lot of people were probably dead after 30 years. For example, one of the young men shown in this film died of a snake bite shortly after we made the film. The bearded headman, Kumaiewä, died shortly after we filmed it: he had his head blown off with a shotgun that was provided by Padre Berno at the Mavaca Mission. His son was also killed in the same attack, also by a shotgun provided by Padre Berno. However, it would be possible to determine the cause of death of each dead person, but informants from

Platanal would have to be disqualified because they have been "coached" by the Salesian Missionaries and know what kind of story they should tell. Residents of Patanowä-teri would also have to be disqualified because it would be offensive to show them motion picture film of dead relatives. It would, however, be possible to find suitably neutral, disinterested, but knowledgeable informants. So, this is a testable hypothesis, one that can be yet decided by empirical evidence.

Many of the people in this film were the Mahekodo-teri "guests" at this feast and it is highly likely that many more of them are now dead--possibly from measles. When Asch and I left Patanowä-teri after filming *The Feast*, we passed through Platanal. None of the Yanomamö were home--they had been at the feast, but were soon to return. While there, Padre Sanchez asked me to look at one of his recently hired employees, who was sick. I recall the alarm that I immediately felt: this man had measles. I urged him to send him downstream, and even volunteered to take him with me. Padre Sanchez declined the offer, assuring me he would send him downstream the next day. I warned him about the potential disaster if the Yanomamö returned to the mission before the man was evacuated: he would infect everyone with the measles. Tierney makes an elaborate point about us not "vaccinating" the residents when we passed through this mission two weeks or so earlier, and as usual, suggests that it was part of some hidden agenda and conspiracy. Well, when we passed through the mission--and spent the night at it--the residents had already departed for Patanowä-teri to attend the feast. That's the explanation for the "mysterious" reason we did not discuss vaccinations in the part of the film that shows us departing from Mahekodo-teri (Platanal).

When I got downstream to Mavaca, I informed Padre Berno and asked him to check to see if Padre Sanchez had evacuated the sick Brazilian, and I continued on to Ocamo, also warning Padre Cocco that there was a Brazilian with measles at Platanal and the Mahekodo-teri were soon to arrive there…and to contact by radio both Berno and Sanchez.. They had not been vaccinated--we used the remainder of our vaccines on the Patanowä-teri. Authorities in Caracas were supposed to have sent by this time medical doctors and vaccines.

Padre Cocco had just recently spent a hectic week tending to the residents at his mission, some of who had come down with wild measles, and knew the dimensions of the impending disaster. Again, Tierney turns this incident into a fantastic conspiracy--alleging that we only vaccinated some half of the Ocamo population because it was part of a "sinister" plot to use them as "control subjects". As I recall, we didn't vaccinate the whole population because they were not at home.

Padre Sanchez never evacuated the sick Brazilian. The Mahekodo-teri came home and soon, the entire village had measles, the real thing. Padre Sanchez, and a medical doctor from the Indian Commission with a game leg who had in the meantime gone to Platanal, abandoned them at the height of the epidemic, saying what a tragedy it was but they could not bear the sight of so many sick and dying people. So they went downstream to Ocamo to get away from it. Padre Cocco was livid, and berated Padre Sanchez for his irresponsible actions.

The entire village of Mahekodo-teri was exposed to wild measles. I don't know how many died, perhaps as many as 20% of the residents. If they see a film that shows their deceased relatives--some of whom died of measles--the culprit in this case is not James V. Neel or me: it was an irresponsible Salesian Priest by the name of Padre Sanchez.

I published a detailed account of how and why this particular outbreak occurred in one of the recent editions of my textbook. Strangely, Mr. Tierney does not mention my account, but he nevertheless cites this edition of my work for other things, invariably putting the usual negative twist on them.

Tierney also gives a solemn, dramatic, and touching story about being an "eye witness" to the alleged tragedies "my helicopter" caused when I callously and recklessly touched down inside of several Yanomamö shabonos. He has mentioned, several times on radio talk shows, of having actually seen one or more of these "destroyed" shabonos, and people weeping over the havoc this caused. One of them was at Ashidowä-teri where the downdraft caused considerable damage in our 1991 field season. He claims, several people were "injured".

There were at least a dozen researchers, about half of whom were medical personnel working to develop a health-delivery program for the Yanomamö on that particular trip, most of them medical doctors associated with Parima Culebra (a Venezuelan government program). Tierney quotes several of them in footnotes.

We arrived and touched down in a clearing that was about to be planted (or was partially planted) with new cuttings to extend the garden. It was about 30 or 40 yards from the shabono. Since there were so many people on board, we set up a temporary 'kitchen' just outside the shabono where we prepared our meals for the next day or so, but most of us slept inside the shabono. As I recall, the shabono was about half-way through it's expected lifespan--shabonos only last some two or three years before they have to be roofed over again with fresh leaves, a process that is usually accompanied by simply burning off the old leaves--which are usually infested with all kinds of cockroaches, ants, spiders, and other vermin after about six months.

The helicopter crew left after dropping us and our supplies in the garden, and arrangements were made for it to come back on a designated time/day to transport us to a different shabono. When it returned, the pilot recklessly hovered directly over the top of the shabono--two high ranking Venezuelan military officers were unexpectedly on board and wanted to get a "close-up" photograph of the inside of a shabono. A section of the shabono slumped and dropped almost to the ground from the downdraft of the rotor blades, and the helicopter immediately moved away. Several people, mostly older women, were "trapped" when the section of the shabono collapsed. They sustained a few scratches, but were not seriously injured, Several of the medical doctors examined them, but it was clear that they were more startled than they were injured. Some of the accounts by various members of this trip astonish me. One claimed that a gourd of human ashes was destroyed, which was not true. Other accounts exaggerated the injuries to the people, or at least implied they were more serious than they really were.

What I find most peculiar about this incident was that the entire incident was blamed on me. I was also on the ground and as outraged by the careless, irresponsible actions of this pilot as anyone else. Why was it not described as an incident involving a large number of medical doctors, who had gone to this village in a humanitarian effort to tend to the health needs of the residents? Instead, it is presented as an "outrageous" and reprehensible consequence of my "research agenda". Surely the helicopter flight was as much for their humanitarian, health related goals as it was for my own research.

Another thing that puzzles me is Tierney's assurance that he "saw" the evidence of this "tragedy"--the allegedly destroyed shabono. That is very doubtful, but adds a dishonest "flair" to his gripping, heart-wrenching story of "injured Yanomamö, victims of Napoleon Chagnon's swashbuckling and careless research style". It is doubtful because shabonos have a finite existence and, if he saw the place where the alleged tragedy occurred, it was either an already repaired but abandoned structure reclaimed by the jungle or it was not recognizable as a former structure, a patch of crcropea trees. The incident occurred in 1991. Tierney went to this site five years later. The shabono was already "mature" when we visited it in 1991, so what he saw five years later was either an abandoned mess--a few poles where the old structure was, covered with overgrowth and tangled vines, or a new shabono constructed on that same spot. In either case, it would be dishonest to claim he saw the "evidence" of a "tragic event".

I landed by helicopter in two other shabonos, blowing some of the roofing leaves off on both occasions. Both of these incidents were very similar, and both shabonos were large and temporary: they were hastily constructed and not intended to be permanent, regular, residential villages. In both cases, I visited these villages by originally landing at some distance from the temporary shabonos--in abandoned gardens. On both occasions large numbers of people came out of the villages to greet me--and to assure me that they welcomed my visit. The Yanomamö carried my equipment through very thorny, dense underbrush or very difficult terrain that painful to walk/struggle in--if you don't have protective clothing. In these two hastily constructed villages they insisted that when the helicopter returned to pick me up, it should land right inside the *shabono*--to spare them the discomfort of having to get through the overgrown abandoned gardens, which were thickets of brush and thorny growth, or at a distance so inconvenient to them that they would rather replace the blown-away leaves than make the trip. It was, apparently in their calculations, easier and less time consuming to pick up whatever leaves got blown away than to walk a half-mile or so through dense brush. The shabonos were both rather large, and the damage to the leaves was restricted to just those portions of the temporary shabono closest to the helicopter.

Tierney presents a deliberately distorted account of my "helicopter landings", putting a grotesque and cynical twist on them, implying that I destroyed lots of villages this way--and he was an <u>eye-witness</u> to the allegedly distraught Yanomamö six years later. He evens gives the standard, incorrect, but sympathy-evoking claim that the "*garimpeiros*" (illegal gold miners from Brazil) drove all the game animals away when they began landing at their clandestine air strips. More likely, the *garimpeiros* shot and ate the game animals. In a now frequently quoted description of the alleged effects of my helicopter landings, Tierney, characteristically, cites only part of what I said to make me look bad. The relevant passage in my 1991 essay read:

> I had been worried about my reception in this village [Narimöböwei-teri] for several weeks, ever since we had discovered its current location and had attempted to land the helicopter there. A few feet from landing, we aborted when we saw the leaves of their roofs being blown away by the chopper's downdraft.
>
> We saw people fleeing in terror and men throwing sticks and stones at us as we retreated up and away. I later learned this was because they were disappointed we were <u>not</u> landing--they were terrified by the helicopter, but delighted to have us come. [1991, To Save the Fierce People, pp 37-38].

Tierney cites only the part about the people fleeing in terror and throwing sticks and stones at us. John Horgan, in a review of Tierney's book in the *New York Times*, begins his distorted review by ignorantly quoting this passage from Tierney--as if I always caused widespread "terror" when I visited a village and those were my "regular" visiting techniques.

When I returned to this village a few weeks later, we landed the helicopter about a mile or so away from the village, and had to climb an extremely steep hill to reach the village. The Yanomamö came down to greet us, happy and excited about our visit. They had to carry my equipment up that steep hill and knew that when I left they would have to carry it back down.

When the helicopter returned to pick me up, they insisted that it should land in the center of the village clearing. Despite my warnings that some of the leaves would be blown off by the downdraft, they said they had to fix the leaky roofs anyway and insisted again that the helicopter land in the village clearing.

Appendix XVI: Helena Valero's first-hand account of a Yanomamö raid

Helena Valero, a women "of Spanish blood", was abducted as a young girl by a group of Yanomamö in 1937, and lived with them for many years. She eventually told her story to Ettore Biocca, who tape-recorded and published it in 1965. The following are twelve pages from this book which recount a raid by one Yanomamö village on another (the first of several described by Valero). The events are recounted early in the narrative, and appear to have occurred shortly after Valero's capture. Valero believes the raid to have been motivated by a dispute over possession of Valero herself. As Valero's first-hand account makes clear, warfare was an important aspect of Yanomamö life long before Chagnon began his research with them. It also makes clear that wife capture was an important facet of Yanomamö raiding, consistent with Chagnon's own findings, and contrary to Tierney's thesis that machetes and Chagnon himself were the primary cause of Yanomamö warfare.

NOTE: The Yanomamö, like every other ethnic group, have had wars. The events depicted here occurred long ago. Use of this material to inform any debate about current policy regarding the Yanomamö themselves is entirely unwarranted.

YANOAMO: THE NARRATIVE OF A WHITE GIRL KIDNAPPED BY AMAZONIAN INDIANS (AS TOLD TO ETTORE BIOCCA)

(pp. 31-43; translated from the Italian by Dennis Rhodes. The text was scanned; there are probably typos)

CHAPTER 3: The Karawetari Attack

[Valero is living with the Kohoroshiwetari, a group of Yanomamö. The Kohoroshiwetari are soon to be raided by the Karawetari, another group of Yanomamö.]

An old man arrived at the Kohoroshiwetari shapuno to warn us that the Karawetari would come and kill us all. Then the women began to cry and cry. One of them gave me a push and said: 'Must we all die because of this woman?' I was already beginning to understand their speech: I fell down and wept. The men began at once to plant in the ground tree-trunks which I think were about six feet high, all round the shapuno, tying them tightly one to another, so that arrows could not come through; the women put short tree-trunks in the low corner below the roof, to protect themselves better. Thus the arrows could reach us only if shot from the other side of the shapuno. The tushaua, or chief, had a watch kept all night. He sent five men along the path: two men a long way in front, two further back and one near the shapuno. They say that if they send more than five men, they begin to talk and laugh; the enemy approaches and hears them. Then he made some of them hide around the shapuno, one here and one there, and said: 'If you hear any people making a noise, run and call us, so that everybody can come out.' Several days went by and no enemy came near; then the men began to say: 'No, they're not coming; they're afraid of us, because we are so many and we have arrows.'

One morning two Kohoroshiwetari men had gone to get roots and bark to prepare the curare. From a distance they saw some Karawetari men who were crossing a large bridge suspended over an igarape. The Karawetari saw them too, and shouted: 'You think we are only passing by. No, we shall return to our shapuno only when we have carried off all your women.'

That morning the daughter of the old woman who was looking after me wanted to go with some other women into the wood to get fruit from the buriti palm. Her mother told her: 'Take this girl along with you.' Then the woman gave me her little boy and I accompanied her. So we went under the buriti trees. I remember that I had sat down with the child while the mother gathered fruit.

Suddenly I heard shouts: 'Waiucape, waiucape (the enemy, the enemy) and I saw the other women and the young girls who were running in front of us. The mother took the child from my knees, gave me a push and said: 'Let's run! Let's run!' So we all ran towards the shapuno; we found it almost empty. The men had gone running to meet the enemy; the other women, with their children, had run away. Only six men and the old woman had stayed behind to wait for us.

It must have been nine o'clock in the morning, and the sun was still low. It is easy to guess the time when the sun is shining. The old woman's daughter untied my hammock from the trunks; I took a basket of buriti fruit, hung it over my shoulders, holding with my forehead the long strip of bark with which it was tied, and we ran away. We ran and ran to catch up with the others, but we could not do so. At last we caught up with them. There were lots of women, youths, old women and children, but no men. We sat down to rest. Soon after a young man came running and said, 'What are you doing? Run away! The Karawetari have already reached that place where we were catching monkeys that day near the shapuno. Every one of them has a bundle of arrows in his hand. They have shouted that they won't go away without taking our women!' Then we fled again. We ran all day. We stopped to rest in the evening and the women said, 'We're a long way off now; they won't come this far. Then they began to prepare the tents for the night.

It was well into the night when two men came running and said 'Run away again; the Karawetari have shouted that they will capture you all.' Those Kohoroshiwetari men did not shoot any arrows at the Karawetari; they ran away without shooting and stayed between us women and the enemy. The Indians with whom I lived later did not do that. Whenever the enemy approached, they shot arrows straight at him. We picked up the firebrands which we had brought with us from the shapuno and we began to flee again. I, behind all the other women, fell down and wept; I was thin and weak. For some time I had eaten those bitter roots of theirs and I always had a stomach ache. I had spent the nights doing to the igarape.

At last we stopped: the night was dark. I lay down on the ground and fell asleep. I had perhaps just gone to sleep when I felt myself being shaken: 'Get up, get up,' said the old woman. Three youths were saying, 'The Karawetari have already arrived where you wanted to sleep and where you had begun to put up your tents.

They have already shot one of us in the shoulder and one in the leg.' The women were crying, because they thought the men were dead; they said: 'Where shall we go now? The big rocks are nearby.' The tushaua had sent those three men to guide us among the rocks; they must have known the way, because they had been hunting on that hill, but one of them said: 'Let's go that way,' while another said: 'No, that other way.' So we walked and walked all night. At last the old woman's daughter put the firebrand on the ground and said to me: 'Sit down'; she gave me the little boy and went to look for wood. I immediately went to sleep. It was just getting light again when they woke me up once more. Above us were some very high rocks; it was cold.

Two men came running; one had a child in his arms. ‘Run away again,’ they said, ‘we can already hear the enemy shouting, "Miserable women, miserable women! Why do they run and fall among these boulders? Do they think we shall leave them alone?" ‘ Then the old woman said to me: ‘I am going back down the hill to where my sons are; the enemy want to kill them. You go with my daughter. Climb up towards the top of the hill; you’ll be safe, because they won’t come up there; they must already be hungry. I’m going back to my children.’ I never saw that old woman again.

We were now very high up. I looked down and saw the enemy, all painted black, running towards us: I pointed them out to the old woman’s daughter. The three men, who had accompanied us during the night, met those who had arrived and they disappeared together. Thus we women were left alone with the children. We tried to climb up the rocks, but we could not flee any more; the Karawetari were by now quite close. They had split up into two groups. One group had climbed up the rocks and were already above us, while the other group was following us from below.

Then a woman began to shout loudly to the enemy, who were below: ‘Karawetari! Have I by chance killed your fathers, so that you should persecute me like this? You have pursued us all night and still you pursue us!’ While she was shouting, those who had climbed up the hill and were above us began to shoot arrows: tak, tak. Six or seven arrows fell, but did not hit us. A child, trembling with fear, climbed up a tree. Then the woman shouted louder: ‘Karawetari, dirty bunch!’ Behind us a voice replied: ‘I’ll carry you off to my shapuno; you have a big mouth for talking, but you can’t frighten us. So much the worse for you, that you have no arrows and you have a husband who’s afraid! He threw his arrows down on the path and ran away. I’ll catch you and take you away with me.’ Several times before the Karawetari had seized Kohoroshiwetari women, but many of them had succeeded in escaping. Meanwhile the enemy continued to shout: ‘Kohoroshiwetarl women, your husbands are miserable! They make you eat roots from the forest; we eat bananas and pupugnas. Your husbands make you eat only the roots of wild plants.’ The women replied: ‘Yes, we eat fruit out of the forest, and roots of plants, but we haven’t come to ask you for fruit and bananas.’

In the meantime I had seen a little grotto among the rocks and I had gone inside; behind me other women had come to hide. Outside there were many women, a lot of them with children in their arms. The enemy were coming down from above and climbing up from below. The boy who had climbed a tree shouted at a man who was coming nearer: ‘Father, don’t shoot me!’ ‘I’m not your father,’ shouted the man; ‘if I had been your father, you would have been happy to run towards me’; and he shot him. The arrow hit the little boy from behind in the leg and came through in front. The child fell, picked himself up and ran with the arrow still in him.

The men began to seize the women; they caught them by the arms, they caught them by the wrists. ‘Ahi!’ shouted the women. Some said: ‘You won’t take only us away; inside that grotto there are other women hiding.’ The men came closer: ‘Come out; if you don’t come out, we’ll shoot arrows inside.’ One woman came out at once. Another wouldn’t come out and hid in there, but the Karawetari pointed their arrows at her: ‘Don’t do that,’ she said, ‘I’ll come out.’ I was still hiding at the back, but I heard the voice of a woman who was saying: ‘In there at the back is that woman whom they caught not long ago.’ Then a man came and shouted: ‘Come out!’ He threatened with his bow as though it were a spear. I came out and stood in line with the other women.

One woman had a baby girl in her arms. The men seized the little child and asked: ‘Is it a boy or a girl?’ and they wanted to kill it. The mother wept: ‘It’s a little girl, you mustn’t kill her.’

Then one of them said: ‘Leave her; it’s a girl; we won’t kill the females. Let’s take the women away with us and make them give us sons. Let’s kill the males instead.’ Another woman had a baby boy only a few months old in her arms. They snatched him away from her. ‘Don’t kill him,’ shouted another woman, ‘he’s your son. The mother was with you and she ran away when she was already pregnant with this child. He’s one of your sons!’ ‘No,’ the men replied, ‘he is a Kohoroshiwetari child. It’s too long since she ran away from us.’ They took the baby by his feet and bashed him against the rock. His head split open and the little white brains spurted out on the stone. They picked up the tiny body, which had turned purple, and threw it away. I wept with fear.

So we began to go down the hill. The men held their women prisoners by the arms. When the forest had thinned out and they were afraid that the women might run away, they put them in the middle; one man stayed on one side and another on the other. While we were going downhill, we saw a woman hiding among the rocks; she couldn’t run away any more. She had three children, one strapped to her back, one on her knees and the biggest by her side. They were near a precipice of rocks. One man came up to her, saying: ‘What are you doing here?’ He kicked the woman and the children and hurled them all down the abyss. They rolled right down among the rocks to the bottom. When we arrived, we found them all injured, but still alive: the blood was flowing from so many wounds and the children couldn’t even cry. One woman prisoner recognized them: ‘It’s the wife: they are the children of the brother of that Kohoroshiwetari who lives with you.’ A Kohoroshiwetari did in fact live with the Karawetari; his name was Matahewe (Mata is a big snake). He had married a Karawetari woman and had grown-up children.

Meanwhile from all sides the women continued to arrive with their children, whom the other Karawetari had captured. They all joined us. Then the men began to kill the children; little ones, bigger ones, they killed many of them. They tried to run away, but they caught them, and threw them on the ground, and stuck them with bows, which went through their bodies and rooted them to the ground. Taking the smallest by the feet, they beat them against the trees and the rocks. The children’s eyes trembled. Then the men took the dead bodies and threw them among the rocks, saying: ‘Stay there, so that your fathers can find you and eat you.’ They killed so many. I was weeping for fear and for pity, but there was nothing I could do. They snatched the children from their mothers to kill them, while the others held the mothers tightly by the arms and wrists as they stood up in a line. All the women wept.

Meanwhile the Karawetari began to call and insult the Kohoroshiwetari, shouting in a loud voice: ‘Kohoroshiwetari, Kohoroshiwetari, cowards, cowards; you ran away! Come and avenge yourselves and kill us! We have killed all your children, take vengeance on us!’ No one answered. They went on: ‘Kohoroshiwetari, from afar you are brave, at close quarters you are the biggest cowards! You run away and leave your women with their children! It’s a good thing that we have killed your children!’ So they continued to shout for a long time. Then a voice was heard from afar: ‘You Karawetari, you are cowards too: you have the courage to take women and children among rocks where there’s no way out. We’ll see your courage when the Shamatari come and make war. Then you’ll be the biggest cowards!’ While he was shouting from a distance, the Karawetari were saying: ‘We’ll make him talk; we’ll find out where he is and we’ll go and shoot him.’ They recognized him by his voice: once Kohoroshiwetari and Karawetari had been friends. The men shouted: ‘Eh! Amiana, Amiana!’ This was his name. ‘Come here, come here.’ The man did not answer. A group of Karawetari went among the rocks to kill him. They came back not long after saying they had shot arrows among the rocks where he was hiding, but they had not hit him: they brought back a bow.

All we women stood still together. It was perhaps eleven o'clock, and the sun was very high, when the Karawetari tushaua, whose name was Maniwe, arrived. He had been in the quarrel with the others; he said: 'Now let's go away; you have already done plenty of killing. I don't want any of these women for myself, because they are too clever at running away. The last time we caught them, they nearly all escaped.'

Shortly afterwards Matahewe, that Kohoroshiwetari who lived with the Karawetari, arrived, and saw his sister-in-law with her children all bleeding: 'Who has done this to my brother's wife and to my nephews? Children must not be killed.' He shouted and shouted: 'If I had been up there, when you hurled them down the precipice, I would have shot you!'

Then in a very loud voice he called his brother, the father of the three children who had been thrown down the abyss. We women were all in a line, waiting to leave. A voice was heard from a distance and soon the man appeared. The Karawetari looked at him; the brother, who was with the Karawetari, said to him: 'Why did you too run away from the shapuno? You should have waited for me with your wife and children. Have you seen what they have done to your family?' the other replied: 'You came with the Karawetari; you too came with the intention of killing my children.' 'No, it's not true; if I had been there, I would not have let them do what they did.' The other looked at that heap of dead children. There were children of one year old, of two years, three years, and bigger ones. Matahewe said: 'Why have you killed these children? Children don't know what grown-ups are doing. We are here to make war on adults.' He shouted, and he wept also. The Karawetari looked at him and wanted to shoot him.

The Karawetari tushaua said to his own men: 'Let him speak! Let no one point his arrow at him; let everyone keep their arrows in their hands.' The man continued: 'Children don't know how to take an arrow or stretch a bow; why have you killed them? No; you shouldn't have killed them, you should have sought and killed the fathers. When I go to make war and give the command, I say to my men: 'Don't shoot your arrows at little children, at old men or at women."' All listened to him without a word; even the Karawetari tushaua listened. At last the brother, who also had two grown-up sons with the Karawetari, said: 'Now we are leaving. If you want to come to us, to the Karawetari, you can; nothing will happen to you. The fault for all this is with the Kohoroshiwetari; their chief, Ohiriwe, speaks very badly of us. We wanted to kill him and his son-in-law, not your sons, nor yourself.' The brother replied: 'Now I will gather wood and I will burn the bodies of the dead.' The Karawetari left that man his wife and three injured children.

Before leaving, the men began to shout loudly, 'Au, au, au . . .' in a ranting voice. This is the cry of the enemy when attacking and it is the cry which they make when they go away after the attack.

CHAPTER 4: The Women Prisoners and the Jealous Wives

They all stood in line, a man, a woman, a man, a woman. On the track, one behind another, they let go of our wrists, because it was difficult to escape. There were many Kohoroshiwetari women, perhaps about fifty. With the Karawetari there were only three women who came behind the fathers and brothers.

We heard a whimper among the leaves. One woman prisoner went up and saw that boy whom the Karawetari had shot at in the tree. With us was that Kohoroshiwetari who did not want them to kill the children. The woman called him and said: 'Shoriwe (brother-in-law), why don't you start a fire and warm this child?' The other Karawetari looked at him and wanted to kill him.

The man said: 'Don't shoot him. He's got such a big wound that he will surely die; leave him alone to die by himself.' He took his hammock, hung it up on the branches, put the little boy in it and lit a fire nearby to warm him.

We walked and walked, and at last we reached a wood of buriti palms. The tushaua said: 'We are hungry and we have nothing to eat. Take these women to collect buriti.' Some women went with the men into the forest to collect the fruit, while others stayed behind; there were lots of them. In the buriti palm forest a young woman tried to run away: she began to run and run. As soon as the man who had captured her saw that she was now capable of escaping, he drew a hooked arrow, one of those pointed with monkey-bone, and hit her in the middle of the back. The woman fell face forward. She must have died; she could never have pulled out one of those hooked arrows. The men came back and said: 'A woman has run away, but we have shot her. She fell down; let's leave her there to die, let's go.'

We walked on and on. By the evening I was very tired and hungry. We stopped in a place where there were some old tapiri. Some men came up to me and looked at me: 'Who took her?' they asked. 'With whom will she stay?' The young Karawetari girl, who had accompanied her brother and who I afterwards learned was called Xoxotami, said: 'It was my brother who caught her, she'll stay with us'; then she turned to her younger brother: 'Give this girl your hammock.' The youth gave me his hammock and lay down on the ground. So we settled down under that roof of leaves, I, the girl, the young lad who had given up his hammock and the other brother. Then the girl began to talk to me; I now understood everything. She said: 'We are going a long way, that way.' Meantime the men were cooking monkeys and had collected those roots out of the wood. They gave me a piece of monkey meat. Thereafter that girl stayed with me all the time.

Early in the morning we left and we walked all day. We reached that bridge where the two Kohoroshiwetari had seen the Karawetari crossing. It was dark now: the river was broad, but we all crossed and camped on the other side. Then the men cut the bridge right down, so that the women could not escape and the enemy could not come and make a surprise attack. The girl called me and made me hang my hammock near her. The younger brother lay on the ground. The older brother and the father put their hammocks up on the branches near us, and they made no tapiri.

No sooner had I begun to fall asleep than I heard: tuk, tuk, tuk . . . they were arrows falling nearby. The Kohoroshiwetari were shooting. The Karawetari then began to shout: 'Kohoroshiwetari cowards, you can shoot from a distance! In the daytime you didn't have the courage to show your faces, now you can shoot from afar, by night!' The Kohoroshiwetari on the other side of the river saw the fires and shot their arrows from a long distance: that is why they didn't hit us. The Karawetari covered up all the fires. The girl, who was with me, was afraid and said: 'Come and hide among the great roots of that tree.' So we ran and hid among those roots and there we stayed until dawn. During the night a woman came silently up to me. She was the one whose tiny baby they had killed. She said to me: 'I'm going to run away; you can go with them. Perhaps it's all right for you; you have neither father nor mother with us. You can go. They already seized me once before. You won't be badly off with them; they don't only eat fruit from the forest, but bananas and uhina roots.'

We continued our journey; the way was long. The Karawetari did not cross the forest and did not climb the trees to see a long way off, but returned by the same path which they had taken while coming. Some Karawetari had been wounded, but none had died. One had a wound in the shoulder, caused by that bamboo arrowhead like a spear: it was a big cut. Another had had an arrow hit him in the heel, another in the knee, and yet another in the chest. From the wound of this last man blood and bubbles were oozing out; he couldn't walk and his companions were carrying him. The other wounded, after two or three days, were already beginning to walk by themselves again.

Some men said: 'The Kohoroshiwetari are cowards, but they still follow us. They wait till they think we are a long way off and are unprotected and then attack us. We have taken so many of their women; they'll come to take vengeance on us.' A few other men replied: 'No, they are not following us: now they must burn their dead and so they are looking for them among the rocks.' The tushaua said: 'Don't say they won't come; they will come. Do you think perhaps that you have killed the sons of wild boar in the wood? No, you have killed the sons of Yanoama; we Yanoama love our sons very much. Certainly they will come; they are waiting for us to be off our guard to attack us.'

So after five days we arrived at a great roca. Three men went off to cut down an inaja palm tree and get its palmito: suddenly an arrow came and stuck in the tree. They ran back to where the group was, shouting: 'The Kohoroshiwetari are attacking!' Then they all grasped their arrows in their hands. A fresh arrow fell near the running men. The tushaua said: 'I said that nobody was to go away by himself. If they had killed you, no one would have seen you.' Then he ordered seven men to stay behind on the path to keep watch. In front of all the rest went a group of men, then a man and a woman prisoner, a man and a woman prisoner. The line was very long; there were so many of us. I, with that girl and her younger brother who had given me the hammock, went ahead, near those who were leading the file.

Sometimes those who bad taken the women did not want theirs and said: 'I took this woman, but I don't want her any longer; who wants her?' Some others replied: 'Give her to me.' Then the one who didn't want her any more gave her to the other and so he passed in front with those who were leading the way and had no women. The young man who had taken me, brother of that girl who was always with me, said to her: 'I want to give this woman to another man.' 'No,' answered his sister, 'she'll stay with us, I'll take care of her.' We passed near a roca which belonged to the Kohoroshiwetari. The men went and found cara roots and tobacco leaves: they filled the baskets with them. Then they carried the cara to those who were leading the way. So we came close to a group of tapiri, where their women were waiting for them and which was still a long way from the big shapuno. When we got near we stopped; the men said to the women prisoners. 'Now you must all paint yourselves.' The women painted themselves red with uructu which the men had brought. The brothers of the girl who was with me had no uructu and painted themselves black with ashes and coal. I was still a child and so they did not paint me. The men who had killed did not paint themselves: they pushed two smooth little sticks through the holes in the lobes of their ears and tied two little sticks to their wrists.

While they were going into the middle of the tapirl, the Karawetari women came angrily to meet them, shouting: 'You, Kohoroshiwetari women, after they have killed your sons by beating them against the rocks, you, dirty bitches, come here all painted as if it were a feast!' They said all sorts of things, accusing them of everything. The Kohoroshiwetari women didn't reply; they went where the men took them. Then the tushaua said: 'We haven't brought Kohoroshiwetari women for you to quarrel with. They have been so hungry during the journey; just see you give them something to eat.' But the Karawetari women continued to shout and to insult them.

Xoxotami took me to her mother in her tapirl. 'Who's this?' asked the mother. 'Where does she come from?' Xoxotami replied: 'She is a Napagnuma.1 She's that white woman whom our men captured together with the Kohoroshiwetari, the one whom the Kohoroshiwetari did not want to give up. This is why our men have become their enemies and have gone to fight them. Xoxotami continued: 'She's pretty thin; no one wanted her. I have helped her and brought her here; now she'll stay with us.' The mother looked at me and answered: 'There are lots of young men here who have no women. This white woman will stay with me; then nobody shall come to take her away.' Then she said in a load voice: 'Let everybody listen. There are so many men without women. Soon this one will become a woman and then nobody must take her away from me.' She took me by the arm and led me to her hammock. 'Now have a rest; you'll be tired after such a long walk.'

Meanwhile the wives continued to insult the Kohoroshiwetari women who were now crying. They said: 'You come here all happy, painted all over. You are happy, you have found your husbands! If they had killed our sons, we should have come in tears. Not you, no, you come with painted faces. Does it give you pleasure that they have killed your sons?' None of them replied; all was quiet. During the journey some of the women had wept, but the men did not like it; when they saw them weep, they threatened them with arrows, saying: 'You weep because you want your husbands to hear you from a distance and to know where you are. If you cry loudly, we will shoot you.' Then the women had been afraid and had cried no more.

The next morning we resumed our journey towards the great shapuno. Nearly all the men went hunting and collecting pupugnas; the Karawetari stayed in the line to guard the women prisoners. The old woman said to Xoxotami and to another daughter who was big and tall: 'Let's go ahead; the women will shout all the time, I don't want to listen to them. The other time, when they took the women prisoners, our women shouted and shouted, and they beat them too; that's why many of them escaped later.' We reached an igarape; Xoxotami's mother said: 'Let us catch crabs.' We stopped and we caught many large crabs. When the sun was already high, we began to march again. The other women, with the prisoners, came quite near to us.

While I was walking, I heard a noise behind me and I saw two young Kohoroshiwetari hidden amongst the trees; they were looking for their women among the prisoners. The wife of one of them, in fact, was a little way behind me. Suddenly one young man jumped out, gave a push to the basket which the woman was carrying on her back and caused it to roll to the ground. The old woman saw him and said: 'Let's go, come on, come with me; no one will come and take you away. If they come, I will hit them with this bow.' She was fairly old and old women, when they travel, lean on a long piece of a bow.

Meanwhile the two men, with the woman, ran off into the bush. The Karawetari women left their baskets on the ground and went after them. The husband allowed himself to be caught to give his wife a better chance to escape. They wanted to take his bow away; the man said: 'Leave my bow, grandmother,' but the old women had surrounded him and wanted to kill him. One shouted: 'Let's catch him down below by his organs, so he won't have any strength left!' He tried to defend himself with the bow, but a woman seized him by one leg, shouting: 'Let's kill him!' The man fell down; his companion kept poking at the women with his bow as though it were a spear, to liberate his friend. The Karawetari women-they were only the old ones, because the young ones were afraid and had gone away-said: 'You can kill us with your bow if you like, but today will also be the end of this man who came to take his woman back. He came to get himself killed by us!' Meanwhile the man had freed himself, while his companion kept the old women off with his bow. So the two managed to escape, taking with them the woman who was running ahead. Then the Karawetari women shouted after the fugitives: 'Go on, go on! Go back to eating wild fruit and bad fruit. Stupid woman for running away! If you had stayed with us, you would have eaten pupugnas and bananas from our rocas. Now you'll have to work hard enough to find wild fruit in the woods!'

So we continued our march, until we reached a group of old tapiri, where we awaited the men who soon began to arrive with baskets laden with pupugna fruit which they had found in a nearby roca. Then-some Karawetari women began to say: 'You left us alone; the Kohoroshiwetari have come and got their women back!' The men, annoyed, said: 'It was all your fault. When they arrived, you maltreated them so much; now they have run away, you are so happy!' Then the women said: 'It's not true, they have taken only one.' The man who had captured her shouted: 'I'm going to get her back.' He wanted to kill the woman and her husband. He left with about ten men. We stayed in those tapiri; the old woman began to roast bananas and Xoxotami to cook pupugnas. Those men came back later: they said they had run, they had found the footprints; then the footprints became almost invisible. Afterwards, in the mud, they could see the footprints clearly again, then they disappeared altogether.

Appendix XVII: Excerpt from Hamilton Rice

Excerpt from: “The Rio Negro, the Casiquiare Canal, and the Upper Orinoco, September 1919-April 1920.” Geographical Journal v. 58, 1921.

(Scanned; there may be typos)

That day we reached the raudal Guaharibos. What looked to be the remains of two bridges, much smaller than the one below, were opposite an island which lies at the foot of the raudal. The opposite left bank is very high, steep, and concave, scoured by the eddy of the raudal, much of it covered with overhanging vines, and the base timber-strewn with great boles and booms of fallen trees. The island is narrow, tree covered, fringed with rock and sand, and its upper end separated from the right bank by a narrow waterway not over a feet deep.

During the day Pedro pointed out to me where Chaffanjon and his party passed up along the left bank; the old fellow also brought me a piece of twig that looked recently cut, significant of Guaharibos.

On the morning of January 22, Pedro and Filomeno embarked in the curiara to go down river a short distance to cut some macanilla palms in order to make new estivas for the canoes, the falca being in very poor condition. They had been gone very few minutes when Filomeno thought he detected an Indian peering at them from behind some bushes on the high left bank, and called Pedro's attention to it; the old man failed to discern anything at first, but a second scrutiny confirmed Filomeno's suspicions, and the fact that there were many of them. Immediately the canoe was headed for camp, and both came running to give the alarm. The Indians, seeing themselves discovered, stood out in groups along the banks, uttering a babel of shouts and yells that completely drowned the noise of the raudal.

Directly opposite the camp a large, stout, dark, hideous individual gesticulated violently and kept shouting in an angry manner when his paroxysms of rage subsided sufficiently for him to articulate. A thick, short growth of hair adorned his upper lip, and a great tooth was suspended from the lower. He was the leader of a band of which some sixty were visible at first, but more seemed to spring up each minute, until the bank was lined with them as far up and down as we could see.

Fuentes, Ober, and I boarded the falca, whence the scene could be better surveyed, and to get ready if necessary the only firearms we had, a rifle, shot-gun, and revolver in addition to the superannuated muzzle-loader belonging to Filomeno. Andre, machete in hand, stood on some rocks just above the falca; the other men were grouped together in the camp, with faces stoical as they gazed steadfastly at the Guaharibos. Attempts to communicate with them in Spanish, Tupi-Guarani, Maquiritare, and Barb were equally futile, as were signs and the offer to them of some knives, fishhooks, and mirrors. Andre was the only one among the men who bad the slightest pretensions of understanding any Guaharibo. He knew a few words, and from what he could make out the Guaharibos were implacable, going to kill and to eat, notified to us also by many of them carrying their hands to their mouths, at the same time making hideous grimaces.

They were a big, muscular, well-nourished-looking lot, with broad, round faces and shocks of thick black hair. A man and woman who stood far out on the left were of lighter hue with tawny or cendre hair, and had less sinister expressions than the majority. My men afterward declared the woman good looking. They were completely nude, as were the others, and all but the woman were armed with long bows, arrows, clubs, and staves. No shields were to be seen.

During the attempts at parley before actual hostilities commenced, Chandless's account of his encounter with the Nauas on the upper Jurua came vividly to mind, and every effort was made to conciliate or at least establish neutral relations, before the last expedient of shooting was resorted to. Our attitude however seemed to be interpreted as weakness; for suddenly four Guaharibos on the down-river side descended the bank, ran out on a fallen tree-trunk lying in the river in shallow water, and started across, fitting arrows to their bows as they came. They meant to fight, and as the first shot was fired over their heads an arrow sent from up river landed beside me. Those crossing the river scampered back for cover. The discharge of firearms seemed to surprise and terrify them, for the bank was cleared in an instant; but it was evident from the grouping of parties up and down the river that flank movements to completely encircle us were being tried, and that our tactics had best be to break through as soon as possible and pass down river.

Camp was broken quietly and orderly, the instruments taken down, all paraphernalia stored in their usual places in the canoes, some specimens of rock and an orchid obtained, during which time it was necessary to fire to kill; and the feud which apparently has endured from 1763 still continues. To old Pedro the losses of the Guaharibos seemed fitting retribution, for they had killed his brother near the sources of the Umauaca.

The arrow which fell alongside me was ingeniously made, 2 metres long the shaft of verada, the tip of macanilla palm, notched and smeared with curare; the feathers at the butt were those of the paujil, the threads or binding from the leaf of the curaua and bark of the yarumo.

What seems most likely is that the day before when we passed the bridge we were seen by some of them who gave the alarm, and a party was collected to make an attack the following night, so as to fall upon us unawares, with a result that is not pleasant to contemplate. To Filomeno's Teen eyes and quick action we owe our lives.

The Guaharibos are plainly forest, not river Indians, their activities on he river being confined to throwing bridges across the Orinoco when they visit its banks in times of low water; they use neither rafts nor canoes. They are cannibal, and eat their food raw: principally wild animals and fruits of the woods, and fish which they shoot. They live in cylindrical-shaped houses with roofs tapering to a point, on the north side of the Guanaya mountains, but they neither cultivate plantations nor have dogs. They leave their houses, cross the mountains, and roam the forests to the south, west, and east during the period of low water from November to April. They understand the use of and manufacture curare, which is employed in hunting and war; and they bury their dead in baskets, which are afterwards exhumed, burned, and the ashes preserved.

During a lull in the hostilities, we passed down the stream, both canoes together, with heavy canvas tent-flies and tarpaulins ready to lift to protect the marineros, Fuentes and myself on the bridge in front of the carrosa of the falca, covering either bank with the gun and rifle. We reached the bridge in half an hour, passed down the raudal below the cano Bacon at 6.10 p.m. in as many seconds as it required minutes to ascend, and camped on the playa where we had breakfasted two days before, opposite the great battlement-like hill at whose base the Orinoco winds like a fosse.

Appendix XVIII: Letter from the ABA and response by Chagnon

The following letter from the Brazilian Anthropological Association (ABA) appeared in the January 1989 Anthropology Newsletter. According to Chagnon, this letter appears to be the source of the myth that Chagnon's work has been used to justify Brazilian government and military policy regarding the Yanomamö. This accusation has been investigated and refuted by Euripedes Alcantara, reporter for VEJA (Brazil's equivalent of *Time Magazine*).

Scanned. There are probably 'typos'.

[*The following letter from Maria Manuela Carneiro do Cunha was addressed originally to the AAA Committee on Ethics. Subsequently, the president of the Brazilian Anthropological Association (ABA), Antonio Augusta Arantes, stating that Carneiro da Cunha's letter "expresses the (Brazilian Anthropological) Association's point of view about Prof Chagnon's (Science) article" (cited below), asked that the letter be published in AN. We herein publish the exchange between Carneiro do Cunha and Napoleon Chagnon (California-Santa Barbara), which will appear concurrently in Portuguese in the ABA's bulletin. Ordinarily, AN Correspondence submissions are not to exceed 500 words. This exchange, between one of our own distinguished members and another national anthropological association, is extraordinary and an exception to the rule. - Ed.*]

To the Editor:

The recent appearance in the Brazilian press of two articles on the Yanomami Indians based on Napoleon Chagnon's latest paper on Yanomami "violence" ("Life Histories, Blood Revenge, and Warfare in a Tribal Population," Science 239:985-992, 1988) 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. The articles in question appeared in two major newspapers, *O Esrado de Sao Paula* ("Violencia, marea dos Yanomami," March 1, 1988, p 14), and O Globo ("Antropologo aponta violencia entre indios," March 1, 1988, p 6), both translations into Portuguese of pieces that originally came out in *The Los Angeles Times* ("Anthropologists Study Homicidal Yanomamos: Remote Tribe Shows Streak of Violence," February 26, 1988, p 34), and *The Washington Post* ("Sexual Competition and Violence: Researcher Advances New Theory for Amazon Tribe's Homicides," February 29, 1988, p A3).

Without getting into the academic details of Chagnon's arguments, we would like to stress some points that bear directly on the appropriation that the press has been making of his writings.

First, he affirms that violence is the principal cause of death among the Yanomami, with 44% of the males in his samples having killed someone. Correlating these figures with male reproductive performance, he concludes that killing is biologically advantageous for reproduction-killers are more efficient at securing mates. This argument, appealing as it may be for sensationalist purposes, is actually built on shaky grounds. The statistical tables with "hard" figures are supplemented with a text riddled with expressions such as the following:

"I can only *speculate* about the mechanisms that link a high reproductive success with *unokai* ['killer'] status" [p 989];

"*seem* to be more attractive as mates than non-*unakais*" [p 989];

"achieving cultural success *appears* to lead to biological (genetic) success" [p 985];

"intensity of grief *appears* to follow patterns predicted from kin selection theory" [p 991, emphasis ours]

We all know that figures do not speak for themselves, but are collected and analyzed by the human beings doing the research. Indeed, figures are as vulnerable to misrepresentation as any other kind of research tool, as Leach, for example, once realized: "The numerical apparatus in which these conclusions are embedded seems to me to be very largely a complicated piece of self-deception" (*Anthropologists in the Field,* Jongmans and Gutkind, eds, 1967, p 76).

Chagnon's 1988 figures are all the more puzzling when we turn to the PhD dissertation of one of his students and read that of the broad categories of causes of death, violence falls rather low. From 1970 through 1974, infectious diseases due to contact with whites killed 69%, violence 12%, accidental trauma 7% and degenerative diseases 6% (Thomas Melancon, *Marriage and Reproduction among the Yanomamo Indians of Venezuela*. PhD dissertation, UMI, 1982, p 42). Melancon wrote his study entirely based on Chagnon's data. Combined with Melancon's study are the figures Chagnon himself gives in his 1974 book, *Studying the Yanomamo*. In a rather disturbing table on p 160, we find that of a total of 555 deaths that occurred in two villages, 53% were due to contagious diseases and 20% to warfare. The figures, however, are so arranged as to make the numbers for deaths by warfare stand out as very large because the deaths due to infectious diseases are spread out throughout six categories, two of which, given in Yanomamo language, are mutual synonyms (shawara and wayuwayu), as explained in a footnote. Now, in 1988, Chagnon claims that Yanomami killings are the main cause of deaths. One wonders why the figures changed so much from 1982 (the date of Melancon's approved dissertation) to 1987 (the date of Chagnon's latest field trip) to reverse the trend, when the most notable change in Yanomami lives has been the intensification of contact with whites and their diseases.

Second, the alleged correlation between successful killing and successful reproduction, the thrust of Chagnon's article, has been contested by various specialists of Yanomami culture, including Melancon himself. Furthermore, the category of "killer" (unokai), translated as "assassin" by the media, has been demonstrated for another subgroup of the Yanomami to be a symbolic rather than a practical category (Bruce Albert, *Temps du Sang, Temps des Cendres, doctoral* dissertation, University of Paris X, 1985). A man in the condition of *unokai* enters a period of purification when he "kills" someone's spirits, when he kills someone or when he thrusts his arrow into a body killed by someone else. Actual killings are, thus, much less frequent than symbolic killings, and both are the appropriate occasion for men to become *unokai*, ie, who have undergone ritual purification. It is interesting to point out that Chagnon admits that he never witnessed any violent deaths: "I did not accompany raiding parties and did not witness the killings that occurred while I lived there" (p 991); also, "Many raiding parties turn back before reaching their destination" (p 987). What this shows is the lack of anthropological sensitivity on his part to distinguish between real practices and ideological elaborations.

Third, Chagnon's article ends with the following passage:

A particularly acute insight into the power of law to thwart killing from revenge was provided to me by a young Yanomamo man in 1987. He had been taught Spanish by missionaries and sent to the territorial capital for training in practical nursing. There he discovered police and laws. He excitedly told me that he had visited the town's largest *pata* (the territorial governor) and urged him to make law and police available to his people so that they would not have to engage any longer in their wars of revenge and have to live in constant fear. [p 990]

The damaging effect of this passage, which is extraneous to the overt purpose of the article, is immediately visible in the echoes it produced in *The Washington Post* and *O Globo* articles. It provides the State with arguments for a complete control over the Indians.

The concern for the consequences of our professional activities was pointed out as early as 1967 by J A Barnes, the English anthropologist who expressed himself as follows:

[the modem ethnographer] is aware that what he writes may well become the basis for action designed to alter what he describes and *will* therefore either take special steps to prevent this happening or, alternatively, he *will* seek consciously to influence and even to take responsibility for such action. [*Anthropologists in the field.* Jongmans and Gutkind, eds, 1967. p 195]

Barnes's warning gives the ethnographer the benefit of the doubt. referring to possible alterations of his writings by others. But this is not Chagnon's case. If we examine the three articles--Chagnon's and the two pieces in the American and Brazilian newspapers--we notice a remarkable fidelity of the journalistic material to its academic source.

We would like, therefore, to emphasize that the academic reification of "violence" and "sexual competition" as the dominant features of Yanomami society, as well as the tendency to encourage their propagation in the mass media with all the sensationalism it generates are not devoid of serious implications for the people who become the object of these public images. This is a very grave matter and leads us to ponder on the social responsibility of anthropological work.

On May 10, 1976, *Time Magazine* (p 17) published a highly biased article, "Beastly or Manly?" about the Yanomami, based on Chagnon's writings. Since his first book, published in 1968, he has labeled them "The Fierce People"; this epithet has turned into a stereotype that is difficult to avoid even among university audiences. Chagnon's publications not only contribute to reinforcing the negative prejudices which usually weigh on indigenous populations (something of a paradox for anthropological research), but also their appearance in the media has consequences that are even more directly damaging for the Yanomami. Thus, less than a year after the *Time Magazine* piece came out, top-level officials of the Brazilian Indian Service (Fundacao Nacional do Indio-Funai referred to the Yanomami "violence" as sufficient justification for a plan to cut up their lands into 21 micro-reserves that were to be surrounded by corridors for the installation of regional economic projects, a plan that was intended to put an end to the aggressive practices of the Indians.

The recent publicizing of Chagnon's writings in Brazil through the mass media is a repetition of the same situation. Precisely at this moment the over-9000 Yanomami in Brazil are suffering the effects of an invasion by nearly 20,000 miners in the largest gold rush of Amazonia since Serra Pelada in the State of Para. At the same time, an Interministerial committee is once again carrying out a preliminary survey prior to the definition and demarcation of Yanomami territory. Wide publicity about Yanomami "violence" in racist terms at precisely this time and in this context is being used by the powerful lobby of mining interests as an excuse for the invasion of these Indians' lands. Four Yanomami were already killed by miners in August 1987, not to mention the untold numbers of Yanomami who have died since 1974 as a result of constant epidemics due to invasion by miners, highway workers and colonization projects. It is very difficult to know what is happening in the area now, as researchers, journalists, missionaries and members of support organizations are prohibited from going into Yanomami territory by Funai and the military.

To conclude, 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 people 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. Since 1979, the American Anthropological Association has taken an active role in the international concern for the rights of the Yanomami people, through resolutions and as a cosigner of a complaint to the Organization of American States in 1980-81. 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.

Maria Manuela Carneiro da Cunha Past President, Brazilian Anthropological Association

Napoleon Chagnon's response:

To the Editor,

Most of the ABA criticisms of my work in general and my recent Science article in particular fall into four broad categories. (1) sensational press coverage of science reports, (2) the accuracy of my ethnographic portrayal of the Yanomamo, (3) the use of biological theory in explanations of human behavior, and (4) my alleged complicity in Yanomamo genocide. 1 will address only these and end with (5) a comment on the AAA policy of "reciprocity."

1. I agree that some members of the press wrote stories based on my *Science* article that

deserve condemnation for their senseless, inaccurate and irresponsible portrayal of the Yanomamo. I cannot control what journalists say about my published scientific works. Perhaps the most useful outcome of this exchange will be to sensitize the press to important issues. Freedom of the press has costs and benefits. One draconian way to handle the press would be to advocate censorship, which none of us would be willing to consider seriously. The other side of the coin is that journalists can be very helpful to publicizing native problems, and we should encourage them to do so. Boyce Rensberger of the *Washington Post*, criticized in the ABA document, carried a very sympathetic front page story on the plight of the Brazilian Yanomamo (April 4, 1988) and is to be congratulated for this. Those of us interested in native rights must rely on the press. We should try to make journalists more aware of the kinds of issues raised by this element of the ABA document. My agreement with the ABA document ends here.

2. I have spent a considerable amount of time trying to put Yanomamo warfare and aggressiveness into a global and historical perspective. I have never claimed that they are the most warlike or violent people on earth, despite what some journalists assert. What little quantitative ethnographic data we have on comparable peoples is that the Yanomamo have moderate levels of mortality due to violence. I have also consistently argued that their military patterns should be viewed as we view our own: that defending one's kin group and culture with forceful means is a common attribute of all sovereign people, isolated tribesmen as well as citizens of large nation-states.

The charge that I have manipulated my data on causes of death to exaggerate the importance of violence is *ad hominem*. My published data clearly show that diseases are the primary cause of death in the Yanomamo population. The authors of the ABA document are manipulating my published data on Yanomamo mortality for their own purposes. In fact, the most reliable large source of data on Yanomamo mortality due to diseases comes from my published works, and it is primarily because of my fieldwork that we know how significant disease is in their demographic profile. My statistics on mortality due to violence take into consideration diseases, some of them recently introduced. If these introduced diseases had not occurred, the rates of mortality due to violence under "aboriginal" conditions would be very much higher. I have the impression that the ABA authors would like to see me report more mortality due to introduced diseases or, perhaps, only that kind of mortality, concealing other kinds. The assumption seems to be that only then will "good" people want to save them. Deliberately manipulating anthropological evidence, even for good purposes, would probably increase the peril of the Yanomamo. When the falsehoods are discovered, some will argue that anything anthropologists say about them should be dismissed because anthropologists lie. My position is that we should try to "save" them, whatever the reasons for their mortality patterns. I am devoting a significant fraction of my time attempting to help the Venezuelan Yanomamo meet the new challenges that face them, with the support of others. Those of us working together in Venezuela do not feel that the fact that they have the same "defects" we ourselves have disqualifies them from being worthy of our efforts on their behalf.

The ABA claim that the doctoral thesis of one of my former students, T Melancon, contests my findings is ridiculous. The ABA authors have manipulated the facts and are demonstrating their ignorance of statistical procedures. They have deliberately confused my clearly stated distinction between mortality due to violence among adult males with rates for all age grades and both sexes in the whole population. While Melancon's thesis focused only on a fraction of the villages in which I collected the data, and on a very short time span in the histories of those villages, his calculated rates of violent death among adult males not only are consistent with what I have reported; they are, if anything, slightly higher. Had the authors of the ABA document read Melancon's thesis more carefully, they would have noted (p 45) that he also broke down mortality into sex and age components, showing that 29% of the males 15 years or older and 42% between 15 and 49 years at time of death died violently in the villages on which his thesis is based. My several publications indicate that mortality due to violence among adult males ranges from about 24% to slightly over 30%, depending on the date of the sample and the population under consideration at the time of publication. In my Science article, I stated (p 985) "approximately 30% of adult male deaths are due to violence. . ." and repeated this again on p 986. Perhaps the ABA authors should themselves take heed of the message they quote from E R Leach.

Bruce Albert claims that unokai status has a different meaning in the part of the Yanomamo area in which he worked, implying that it has only "symbolic" meaning among all Yanomamo. I eliminated "symbolic" *unokai* in my *Science* report. The Yanomamo where I work clearly distinguish between "true" *unokais (unokai n tai)* and "false" *unokais (unokai horemou),* as I suspect they do where Albert worked. We will probably have to ask Giovanni Saffirio, a Consolata priest and PhD in anthropology (U Pittsburgh, 1985) how many "true" *unokais* there are in the Catrimani area, the mission source of much of Albert's data. Where I have worked. men who deflower prepubescent virgins must *unokai.* I did not count these. Men who *unokaied* and later learned that the victim recovered were also eliminated from my analysis. All *unokais* in my report were men who delivered intended fatal blows or shot arrows (and, in a few cases, shotgun pellets) into a living, *real* person who died as a consequence. There are, of course, more *"unokai"* events than there are victims, which I clearly distinguished in my *Science* article. The ABA suggestion that I cannot tell a "symbolic" from a "real" death is nothing short of silly, which provokes me to treat with considerable suspicion all ethnographic reports of Mr Albert and Ms Ramos, for whom such a distinction appears to be hardly more than an arbitrary desideratum. [A somewhat shorter version of the ABA document was submitted in May 1988, to *Science,* signed by B Albert and A Ramos. The *Science* editors accepted it for publication and sent it to me for comments. The authors then withdrew it.] In my view of anthropology as a science, the ethnographer should attempt to make observations and report facts that can be replicated by another observer. I am confident that facts 1 report on the basis of some 50-odd months of living in many Yanomamo villages can be verified by any competent scientific field researcher who is willing to spend the time and effort required to check on the accuracy of my reporting. Those interested in a totally independent, nonscientific but highly informative view of the people I studied would do well to read the account of Helena Valero's life among many of the same people. I doubt that she confused symbolic deaths with real ones: she had the advantage of not being anthropologically trained to confuse symbols with the things they stand for. Husiwa (Fusiwe or Husiwe in her accounts), her former Patanowa-teri husband, by the way, is counted in my *Science* article as one of the "real" victims whose body I did not see.

The ABA document argues that since, by my own admission, I did not witness the killings I reported or make "body counts," the violent deaths therefore may not have actually occurred. I also did not witness the vast majority of deaths that were attributed to "introduced diseases." Nor did I witness the brutal killing and desecration of the bodies of four Yanomamo men killed in August 1987 by Brazilian *garimpeiros.* But the authors of the ABA document are willing to believe, even enthusiastically, my reports on deaths due to introduced diseases and sensational press reports of the killing of four Yanomamo by *garimpeiros.* This suggests that the authors of the ABA document choose arbitrarily to believe what they want to believe because it is useful for their own purposes, requiring body counts for intratribal violent deaths, but willing to accept just about any form of evidence on violent deaths caused by outsiders or diseases introduced by outsiders.

3. I did not conclude in my *Science* article, or anywhere else, that killing or homicide is biologically advantageous as a general principle in Yanomamo culture, or in all cultures everywhere and at all times. An untimely violent death certainly doesn't do much good for the biological future of the victim. Humans have both a natural history and cultural history, and a scientific understanding of human behavior requires an understanding of both biology and cultural anthropology, regardless of what some schools of thought in anthropology might argue. In my *Science* article I did demonstrate that there was a positive and statistically significant correlation between the male status *unokai* (a ritually purified killer of another real person) and two other variables: (1) marital success and (2) reproductive success. This might be the first time this has been demonstrated for any human population, and it is an important scientific finding. I left open the ultimate explanation for this correlation because, as a scientist, I do not have additional facts that would enable me to conclude that aggressiveness alone leads to reproductive success in this historical-cultural-ecological context. I did suggest a number of possible avenues of further inquiry that might explain this correlation and very cautiously chose words like "speculate," "seem," "appears," etc to let the reader know that I do not have the definitive explanation for this correlation, but did have some informed suggestions. This is a standard procedure in scientific reporting, and *scientific* readers are normally aware that when a colleague says he is speculating about something suggested by his empirical findings he is not suggesting that his findings are simply free inventions of his mind. However, it is entirely possible that the single most important variable explaining higher reproductive success among Yanomamo *unokais* is their greater willingness to take mortal risks and demonstrate their aggressiveness. however repugnant this might be to some anthropologists or others who believe, *as a matter of faith,* in extreme forms of cultural or environmental determinism.

4. The suggestion in the ABA document that I am encouraging or promoting genocide by my ethnographic descriptions of the Yanomamo and my alleged *racist* manipulations of data on their violence is gratuitous and insulting. If is also libelous. Those of us in the Americas who come from European backgrounds have been systematically causing the extermination or disappearance of native Americans for nearly 500 years. Among large nations in the Americas. Brazil in particular has a sterling track record in this regard, followed closely by my own country. We have accomplished most of it in complete ignorance of anthropology, and we continue to inflict enormous harm on native peoples not only regardless of what anthropologists do or say, but usually in spite of those things anthropologists do or say that are deliberately intended to have the opposite effect. Anthropologists are an easy target, a convenient scapegoat: they "know" about native peoples and are often highly visible. But the power, control and influence attributed to us is mythical: we are generally incapable of having much of an effect when what we are opposing is the spread of a vast, powerful and economically gigantic process. I am struck with the similarity of the Brazilian government's treatment of US anthropologist Darrell Posey and his Kayapo informants and the ABA's accusations against me: *both* Brazilian

groups blame a US anthropologist for their own frustrated attempts to achieve what it is that each is striving for.

5. 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 US 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. This policy, I believe, should be discussed in the *AN* and at a future AAA business meeting. Whence and when did this policy emerge'? Are there *any* guidelines regarding what the AAA guarantees to publish in the *AN* if submitted by a "sister" AA?

Napoleon Chagnon
University of California-Santa Barbara

Appendix XIX: The UCSB team's response to Terence Turner's letter to The New York Review of Books

From: ucsbteam@hotmail.com
Date: Wed Apr 25, 2001 2:37 am
Subject: UCSB Team's Response to Turner's NYR letter

Terence Turner's recent letter to the New York Review of Books [NYR April 26] makes several extremely misleading claims. In that letter, Turner says:

> ----
> The medical care [Neel and his team] did give prevented the completion of some of their planned research, but their persistence, under Neel's leadership, in attempting to carry out the higher-priority research tasks forced them into corner-cutting and improvisation that had deleterious medical effects. Unable or unwilling to stay the requisite ten days with those they vaccinated to see them through the severe reactions to the Edmonston vaccine, which were sometimes indistinguishable from the disease itself, for example, they adopted the expedient of vaccinating only half the population of villages so that the unvaccinated could care for those suffering from the reactions. This, however, left the unvaccinated to face the epidemic without the protection of immunization.
> ----

Readers not familiar with the details of Neel's expedition in early 1968 might understandably conclude from the above that Neel either deliberately or carelessly left half of each Yanomamö village unvaccinated and vulnerable to infection in order to better pursue his unrelated research objectives. This is not true. Doses of vaccine were limited, and the number of Yanomamö in Venezuela and Brazil outnumbered the available doses by at least a factor of 5 (assuming a total population of 10-15,000, and 2-3000 doses of vaccine).* Thus, the vaccination of any one individual necessarily left another "without the protection of immunization." Until we see the materials Turner is referring to, we are withholding judgement on whether Neel routinely, or even once, vaccinated only half a village (see below). Regardless, the following points can be made: (1) ALL available doses of vaccine were administered in early 1968 (2) more vaccine was urgently requested from the Venezuelan government, and (3) those doses were also administered. In addition, Neel continued to solicit vaccine and gamma globulin donations after returning to the US, and was able to provide at least 500-1000 additional doses by mid-April. Given the limits on vaccine availability, every Yanomamö that could be vaccinated WAS vaccinated. As we discuss below, Neel and others also provided medical supervision for individuals suffering vaccine reactions and measles. Under any reasonable interpretation, the statement that Neel "left the unvaccinated to face the epidemic without the protection of immunization" is false.

A fair assessment of Neel's strategy to distribute very limited supplies of vaccine, gamma globulin, antibiotics, and medical personnel would require careful analysis by individuals with expertise in measles, measles vaccine, epidemiology, and vaccination and healthcare programs in remote, rural areas. So far as we know, Turner has expertise in none of these areas (and his home page gives no indication of any such expertise: http://falcon.arts.cornell.edu/Anthro/faculty.html#turner).

Also, Neel DID prioritize vaccinations, although he was frustrated that his research had to take a back seat to the vaccination program. The following is from Neel's field notes, Wednesday February 21, 1968. Neel and his team were distributing the last doses of vaccine allocated to villages on the high Orinoco (above Mavaca):

> ----
> Sleep as best all could, and up with the dawn, getting here [a Yanomamö village] between 8:00 and 11:00, in three very hard hours [of travel]. And have been vaccinating all afternoon [i.e., first priority]. Hate to do it first but there is an urgency especially since two of the Pats [the Patanowa-teri, a Yanomamö group] who _may_ have been exposed to the measles case at Mavaca [another village lower on the Orinoco] are now here. There is a real bit of drama here -- we finally reached the Pats, and its [sic] neck and neck to do what we hope to do (resigned to no physical exams or anthropometrics to speak of, and absolutely no dermatographics) [i.e., Neel is abandoning significant aspects of his research]. Tomorrow urine and stools and Friday blood and salivas and blood films.
> ----

Elsewhere in Neel's notes, it is clear that he and his team spent considerable time at sick call. So, Neel put vaccinations and medical care first, and reduced or eliminated data collection in order to do so.

As noted above, until we see the material Turner is referring to, we will withhold judgement on whether or not Neel and his team "adopted the expedient of vaccinating only half the population...." The original plans for the vaccination program (prior to departure for the field) called for one half of each village to be vaccinated first so that the number of vaccine reactions would not overwhelm missionaries and village members caring for those who reacted strongly. It is possible that Turner is referring to this original plan (it is also possible that he is referring to evidence that there were two rounds of vaccinations at one village--Ocamo. See note below**). Once the epidemic broke out (after Neel et al. were in the field) plans changed dramatically, however. We do not know the details of Neel et al.'s plans to distribute their limited doses of vaccine after the epidemic broke out, but we do know that all doses were in fact distributed.

Turner also claims:

> ----
> [Neel] did give medical help during their stops for research, and sent supplies of vaccine and medicine to missionaries to help the sick at their posts; but as Tierney reports, they did not follow

established vaccination practice and stay with groups suffering from both measles and severe vaccination reactions until other medical help could arrive. Tierney's description of such a hasty evacuation, during which [James] Neel was recorded scolding the cinematographer, [Timothy] Asch, for filming a colleague trying to give medicine to a sick Yanomami, because medical assistance was not the proper business of the expedition, catches the prevailing attitude, which translated itself in various ways into short-changing medical care in favor of research.

The International Genetic Epidemiology Society (IGES) has reviewed a transcript of the same recordings that Tierney describes (and Turner cites above), and has come to a very different conclusion:

We have reviewed a transcript of these sound reels prepared by members of the American Society of Human Genetics who have listened to them, and do not arrive at the same interpretation as Tierney. In its proper context, the first of these quotes appears to be an instruction to Asch as to the type of material he wanted on the film, not an instruction to his medical colleagues to refuse care as Tierney interprets it.
(IGES, http://hydra.usc.edu/iges/Darkness/Darkness.html)

In other words, Neel was scolding Asch for wasting film on scenes of routine medical care (and also possibly for attempting to glamorize routine medical care). Neel did not want a "picture of the physician ministering to his flock" (Neel's words on tape); he wanted a film of the unique aspects of the expedition. Neel's comments were an instruction to his filmmaker, and can in no way be construed as an instruction to his medical team (note that Neel is scolding Asch, the cinematographer, and is *not* scolding the colleague providing medicine).

Turner also endorses the claim that Need did not follow "established vaccination practice and stay with groups suffering from both measles and severe vaccination reactions until other medical help could arrive." However, contra Tierney and Turner, Neel DID allocate medical personnel to monitor those suffering either vaccine reactions or wild measles. In the very next section of tape (not quoted by Tierney), there is the following statement by Neel (transcription from IGES):

Neel: We have 1000 [doses of vaccine] by our estimate; we will allocate about 750, so we have about 250 left. These we want to use to get Platanal [another mission and Yanomamö village] and catch it on the upper Orinoco and then the Patanowa-teri who are the principal inland village we might get to. Actually, Ocamo [a village that Neel supposedly abandoned], we can't be sure what's going to happen next, but it would be excellent insurance to have two physicians here. Ocamo will be pretty well over in the next 3 or 4 days [i.e., reactions will have subsided]. We'd like to have one standing by here and one allocated to help the missionaries if the inland village comes down.

As the IGES notes, "This hardly sounds like a plan to abandon the Ocamo mission! Neel's field logs show that extensive medical care was indeed provided." (Chagnon and Asch stayed with the Patanowa-teri during their reaction phase).

Finally, Turner claims the following, based on material that we haven't seen:

It is clear, for example, from Neel's and some of his colleagues' correspondence that vaccinations against measles and several other diseases were originally planned as research tools for studying the ability of the Yanomami to develop resistance to the stress of alien diseases, long before they learned of an actual outbreak of measles in their research area.

Although we are not familiar with the material that Turner is referring to, we certainly hope that Neel planned to extract the maximum amount of biomedical information possible out of any vaccination programs with the Yanomamö and other indigenous groups. The development of the measles vaccine, for example, involved numerous studies of its effects on remote, rural populations, including those previously unexposed to the disease. In general, both basic and applied medicine rely fundamentally on research and experiments, and the Yanomamö themselves have benefited tremendously from previous research conducted with other remote, rural communities (development of the use of gamma globulin to attenuate reactions to the Edmonston vaccine, as well as development of the vaccine itself are but two of numerous examples). Turner also says:

This supports Tierney's claim that the vaccinations were, at least originally, conceived as an "experiment." It is also clear, however, that the vaccinations were never the main purpose or priority of the expedition....

Tierney strongly implies that Neel deliberately used a dangerous vaccine to conduct an unethical experiment. This absurd claim, widely advertised by Turner in his original email, has been thoroughly debunked.

(There are several claims by Turner in this letter that we do not address; this should not be construed as an implicit endorsement of those claims)

The UCSB Team, April 24, 2001.

(This response is not an official statement by the University of California)

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*Neel originally obtained 2000 doses of Edmonston B measles vaccine. Learning of a measles epidemic in Brazil prior to his departure for Venezuela, he diverted 1000 doses to missionaries in Brazil, leaving him 1000 doses. A "Measles Vaccine Check-Off" in his log indicates that 1033 individuals were vaccinated from late January to mid February (many of the entries are undated, however). 65 of those individuals were vaccinated with Swartz vaccine, which we assume was obtained from the Venezuelan government (government personnel were working with Neel on the vaccination program). Thus, there is documentation that 97% of all available doses known to us were administered. Given that vaccine was administered by more than one team in the field, given that vaccine continued to be administered after Neel left the field, and given that we have made no effort to exhaustively account for each and every dose of vaccine, we feel confident that 100% of all available doses were administered in a relatively short period of time.

**The Neel et al. article on the epidemic indicates that there were two rounds of vaccinations at Ocamo, the first village on the Upper Orinoco to receive the vaccine (implying that half the village was vaccinated during the first round, and half during the second). There are several alternative explanations for this however, including the possibility that half the village was not in residence during the first round (quite common at this time of year); that there were visitors at Ocamo during the second round (thus making it seem that only half the village was vaccinated during the first round); or that there wasn't enough vaccine to vaccinate everyone on the first round. Most importantly, the first round of vaccines at Ocamo were NOT administered by Neel or any member of his team, nor did he or any member of his team make the decision to vaccinate! Another team of French doctors, headed by the Venezuelan doctor Marcel Roche, coincidentally arrived in the field at the same time as the Neel team. The French team had no connection with the Neel team, and Neel notes in his log his frustration at having to coordinate river and air transportation with them. When the French team decided to administer the first round of vaccinations at Ocamo, Neel was over 100 kilometers to the north, vaccinating and collecting samples from Ye'kwana on the Ventuari River. The French team used vaccine provided by Neel, however (Chagnon arrived at Ocamo early with some supplies, including some vaccine. Chagnon left the vaccination decisions to the French and Venezuelan doctors). In any case, the French team's decision to vaccinate was correct, and if they decided to vaccinate only half the village, they probably had an excellent reason for doing so.

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