



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.

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>

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Executive summary

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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 galley's 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 untrammelled 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.

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 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. However, 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 the 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 field trial of Edmonston B among Native Americans and ‘mestizos’ in Panama.
3. A field trial of Edmonston B among Native Americans in Alaska.
4. 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.

Here is Tierney on G. S. Wilson:

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)

We looked up G. S. Wilson’s article. Wilson was not warning about possible fatalities from Edmonston B in particular, he was noting that “In practice no vaccine has yet been devised that has not occasionally given rise to a severe and sometimes fatal reaction.” It is quite clear that he is talking about vaccines “against any disease,” and he nowhere singles out any measles vaccine as particularly dangerous. Wilson *was* concerned, however. Why? Because measles “has now in many parts of Europe and America become so mild that death is quite exceptional (Wilson, 1962).” In other words, the disease is so mild in some populations that even mild vaccine reactions might indicate against using it. As Wilson reasonably asks, “Under these conditions, is the disease worth preventing...?” But what about vaccinations in non-US and non-European populations? What about tropical populations like the Yanomamö? In the same paragraph that Tierney cites, Wilson has this to say: “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 make a point of endorsing the use of Edmonston B in tropical populations like the Yanomamö.

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 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 B *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 B 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 B (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.

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 vaccinees 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, then 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 discuss 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). [Note: all citations of Tierney are of the galleys]

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 than 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 headmen 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 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

*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.

counterectors 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>

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 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 Asistencia Social”, an official government agency, within which the Venezuelan Malariología 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, 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 (although we confess that we are still not sure whether the Brazilian teenager is likely to have been such a case). Wilson spends 1/2 page of his five page article laying out the evidence for "latent" cases of measles (i.e., those without characteristic symptoms).

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 that 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. 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.

This latter study in particular 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 that 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. 30). 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.

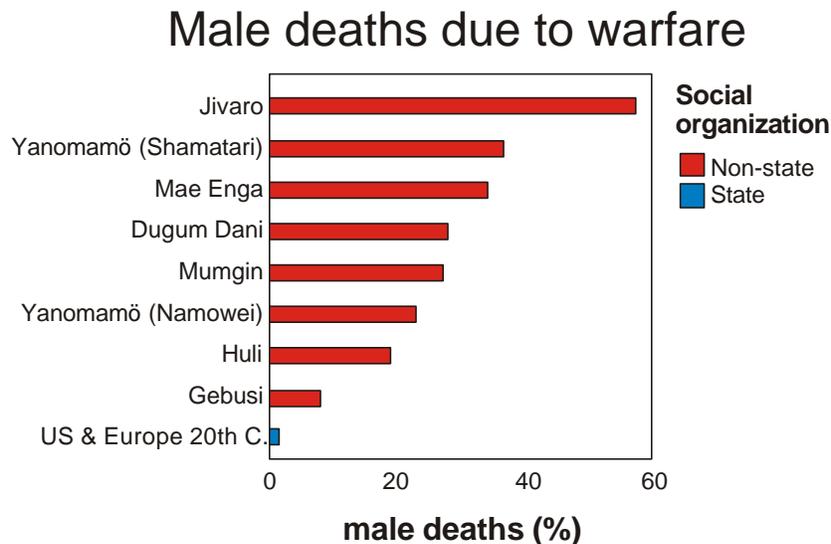
9. Based on information in the Neel et al. account of the epidemic, Tierney insinuates that Neel et al. failed to provide proper medical care when, in fact, the information Tierney cites shows precisely the opposite.

Detailed Evaluation of Chapter 10: To Murder and to Multiply

Chapter 10 of *Darkness in El Dorado* (galley copy) 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 presents data which suggest that Yanomamö unokai (men who have killed in war) have more wives and offspring than non-unokai. We detected several instances of misrepresentation and error in Tierney’s chapter.

Brief Introduction:

Many people 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 Keely 1996):



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. 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 suggests 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.

Appendices

The appendices contain commentary by experts on Darkness in El Dorado. 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: 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 Awerohé 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 losing 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 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 Ocoma Mission, a note which said that

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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

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 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).

++++
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 anthropologist-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.

November 12, 2000

Preliminary Report on the Neel/Chagnon allegations

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 VI: 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 verité 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 Yanomami 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 studied (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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My Web page is <http://www.temple.edu/anthro/ruby/jayruby.html>

Link to my new book, Picturing Culture -
<http://www.press.uchicago.edu/cgi-bin/hfs.cgi/00/13964.ctl>

Link to a description of my ethnographic study of Oak Park, IL -
<http://astro.ocis.temple.edu/~ruby/opp>

Appendix VII: 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 responsibly 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 (39-40° 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 Ocamo 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 VIII: '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):

September 28, 2000

Dear Dr. Katz,

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

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 allegations 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. 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.

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 IX: Original email from Dr. Samuel Katz to Bill Oliver

The following 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 demonstrate 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 descendants 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

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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

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Executive summary

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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 galley's 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 untrammelled 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.

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 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. However, 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 one of the most primitive measles vaccines, 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 the 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 field trial of Edmonston B among Native Americans and ‘mestizos’ in Panama.
3. A field trial of Edmonston B among Native Americans in Alaska.
4. 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.

Here is Tierney on G. S. Wilson:

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 virus was “not so clear.” (Tierney, p. 56)

We looked up G. S. Wilson’s article. Wilson was not warning about possible fatalities from Edmonston B in particular, he was noting that “In practice no vaccine has yet been devised that has not occasionally given rise to a severe and sometimes fatal reaction.” It is quite clear that he is talking about vaccines “against any disease,” and he nowhere singles out any measles vaccine as particularly dangerous. Wilson *was* concerned, however. Why? Because measles “has now in many parts of Europe and America become so mild that death is quite exceptional (Wilson, 1962).” In other words, the disease is so mild in some populations that even mild vaccine reactions might indicate against using it. As Wilson reasonably asks, “Under these conditions, is the disease worth preventing...?” But what about vaccinations in non-US and non-European populations? What about tropical populations like the Yanomamö? In the same paragraph that Tierney cites, Wilson has this to say: “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 make a point of endorsing the use of Edmonston B in tropical populations like the Yanomamö.

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 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 B *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 B 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 B (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.

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 vaccinees 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 discuss 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 fearing the opprobrium of an 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 obtained the Edmonston vaccine from Parke Davis Laboratories, Philips Roxane, and Lederle, rather than seeking 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 than 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 headmen 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 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

*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.

counterectors 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>

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:

Scientists had been competing worldwide to observe measles in a “virgin soil” population....Because measles attacked everywhere with such predictable ferocity, 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 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 Asistencia Social”, an official government agency, within which the Venezuelan Malariología 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, 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 (although we confess that we are still not sure whether the Brazilian teenager is likely to have been such a case). Wilson spends 1/2 page of his five page article laying out the evidence for "latent" cases of measles (i.e., those without characteristic symptoms).

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 that 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. 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.

This latter study in particular 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 that 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. 30). 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 experiments and also a normal safety precaution in the absence of an outbreak (p. 60-61)

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).” 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.

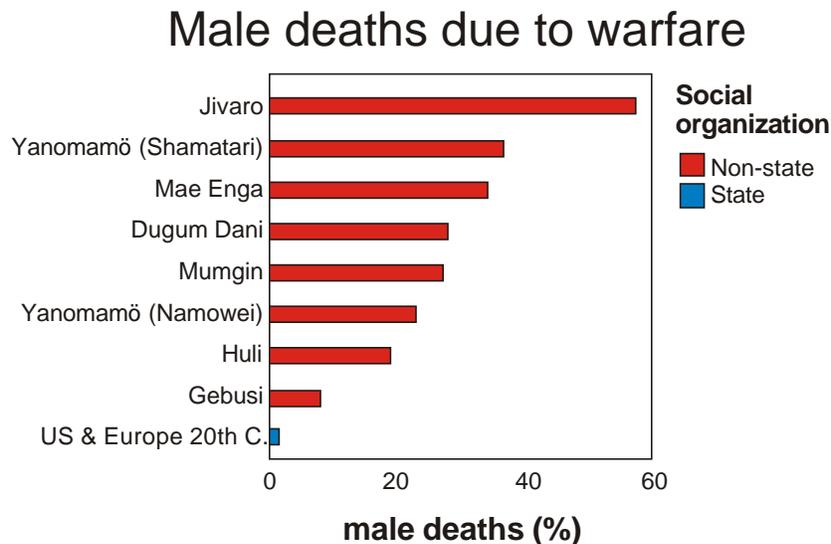
9. Based on information in the Neel et al. account of the epidemic, Tierney insinuates that Neel et al. failed to provide proper medical care when, in fact, the information Tierney cites shows precisely the opposite.

Detailed Evaluation of Chapter 10: To Murder and to Multiply

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 presents data which suggest that Yanomamö unokai (men who have killed in war) have more wives and offspring than non-unokai. We detected several instances of misrepresentation and error in Tierney’s chapter.

Brief Introduction:

Many people 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 Keely 1996):



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. 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 suggests 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.

Appendices

The appendices contain commentary by experts on Darkness in El Dorado. 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: 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 Awerohé 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 losing 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 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 Ocoma Mission, a note which said that

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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

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 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).

++++
 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 anthropologist-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.

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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 VI: 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 verité 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 Yanomami 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 studied (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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My Web page is <http://www.temple.edu/anthro/ruby/jayruby.html>

Link to my new book, Picturing Culture -
<http://www.press.uchicago.edu/cgi-bin/hfs.cgi/00/13964.ctl>

Link to a description of my ethnographic study of Oak Park, IL -
<http://astro.ocis.temple.edu/~ruby/opp>

Appendix VII: 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 responsibly 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 (39-40° 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 Ocamo 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 VIII: '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):

September 28, 2000

Dear Dr. Katz,

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

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 allegations 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. 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.

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 IX: Original email from Dr. Samuel Katz to Bill Oliver

The following 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 demonstrate 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 descendants 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

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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

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