PRELIMINARY REPORT OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION EL DORADO TASK FORCE
Submitted November 19, 2001

PART II. ALLEGATIONS AND CASE STUDIES

A. The Measles Epidemic of 1968.

Among the allegations made in *Darkness in El Dorado*, the one that received the most attention was Tierney's claim that research by the human geneticist James V. Neel in 1968 might have exacerbated the effects of a measles epidemic among the Yanomami. The Peacock Report and the charge from the AAA Executive Board to the El Dorado Task Force suggested that the Task Force consider these allegations.

This allegation was publicly made only by Terence Turner and Leslie Sponsel (Turner[i] and Sponsel, Letter to Lamphere and Brenneis, August ? 2000) repeating a claim made in the galley proofs of *Darkness in El Dorado*, that Neel started the 1968 measles epidemic in order to test his hypothesis that headmen would show lower mortality than others in the Yanomami population. This claim does not appear in the published book. This allegation has also been reviewed by the International Genetic Epidemiological Society (www.genepi.org; Morton, 2001), the University of California, Santa Barbara (www.anth.ucsb.edu/chagnon.html), the University of Michigan (www.umich.edu/~urel/darkness.html,local copy), The National Academy of Sciences (http://www4.nationalacademies.org), and the medical team of the Federal University of Rio de Janeiro (Lobo, et.al, 2000) among others. In addition, scientists involved with the development of measles vaccine, including Samuel Katz (http://www.psych.ucsb.edu/research/cep/eldorado/katz) have reviewed the allegation. All of these sources found that the allegation was completely without merit. The AAA El Dorado Task Force joins these other groups in finding this allegation to be without foundation.

Although this most serious allegation against Neel has been rejected, discussion of his role in the epidemic has continued since the publication of *Darkness in El Dorado*. Tierney's published book (2000) did maintain that the Edmonston B vaccine used by Neel was "one of the most primitive measles vaccine [sic]" (Tierney 2001:55), and was "dangerous" (Tierney 2001:56). Tierney suggested that Neel had chosen it because it "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 2000:59). Turner (2001) and Stevens (2001) continued to develop this allegation that Neel's program of vaccination had an experimental as well as a humanitarian purpose: to permit Neel to document differential effects of the vaccination in an unvaccinated population. However, they believe that Neel abandoned this research goal once the vaccination program had to be speeded up in the face of a spreading epidemic. Turner (2001) has also alleged that Neel was inappropriately committed to meeting the goals of his research program, over and above his concern for Yanomami well-being, and even considered dropping the vaccination program, which had taken up an unexpectedly large amount of time. Further, Turner believes that he gave insufficient attention to the health needs of vaccinated Yanomami, many of whom suffered flu-like reactions to the Edmondson B vaccine. Finally, Turner believes that Neel may not have been sufficiently careful with quarantine, given that his journals and autobiographical statements note that he had an upper respiratory infection while he was in the field. Turner concludes that Neel's priorities, with which he disagrees, were in a sense inherent in the kind of large-scale grant-funded scientific program represented by the 1968 expedition. The Task Force felt it necessary to address these recent allegations during our inquiry.[ii] Through interviews and reading of the available documents, we determined that Neel used the following procedures.

The major allegation made by Tierney (2000), that Edmonston B was chosen because of its experimental value, was downgraded by Turner to a claim that Neel chose this vaccine without regard for potential side effects. Terry Turner (2001b)) states "Neel simply did not care enough about the more severe reactions to the Edmonston B…to forego the free donations of the vaccine from the manufacturer".

We will examine this question in addition to the following questions that remain about the conduct of the participants of the 1968 expedition:

1. The vaccination program was based on a scientific experiment and not on a humanitarian effort.
2. There was no consultation as to which vaccine to use or how to administer the vaccine
3. There was no permission from the Venezuelan government to vaccinate
4. Neel received 2000 doses of vaccine but only had 1000 in Venezuela. What happened to the other 1000 doses?

5. There was an experimental protocol to vaccinate half the inhabitants of the village
6. Neel waited in Caracas before entering the field when he knew an epidemic was in progress
7. Neel was in conflict because he was more interested in the science than in the humanitarian efforts to help the Yanomami
8. Neel had an upper respiratory infection when he entered the field, thus evidencing little concern for the health of the Yanomami.
9. Neel did not have informed consent for his collections..

1. The measles vaccination program was part of a scientific experiment and not a humanitarian effort.

Neel discusses a vaccination program beginning in 1967. Blood samples taken during the 1966/67 field season were tested for antibodies to measles and other infectious diseases (Neel, et al., 1970). There is no mention in the correspondence, field diaries or grant proposals to the Atomic Energy Commission (Atomic Energy Commission AT(11-1)-405,1960 (Continuations 1961, 1962); AT(11-1)-942,1965; AT(11-1)-1552,1966; AT(11-1)-1552,1967; AT(11-1)-1552,1968; AT(11-1)-1552,1969; AT(11-1)-1552,1971; AT(11-1)-1552,1972; AT(11-1)-1552,1973) that there is any experimental protocol regarding the measles inoculations. Indeed, it seems to be completely a humanitarian effort.

Some of the relevant documents in the Neel papers include the following. In March 10, 1967 Neel writes to Hawkins asking about inoculating for smallpox, tuberculosis and measles: Measles vaccination the most difficult because it must be kept frozen and the most expensive.

Neel's 15 September 1967 correspondence to Hingson is indicative of his understanding the importance of inoculating "virgin soil" populations: "We would welcome the opportunity to inoculate against [measles, smallpox, pertussis, tuberculosis] (assuming the Indians…would accept this)." He specifically addresses the notion of humanitarian concerns that are not in conflict with his scientific mission: "In addition to our scientific interests…we are impressed by the humanitarian opportunity here. As you must know, when a group such as this comes in contact with our culture, the decimation is fearful to behold."

A later letter from Neel (19 September 1967) to missionary Daniel Shaylor expresses the same concerns for the health of the Yanomami: "measles and whooping cough, not to mention smallpox and tuberculosis have not reached these Indians to any significant extent, and we are considering whether we could do some type of inoculation which would minimize the effects of these diseases when they finally do reach the Indian."

On November 21, 1967 Neel writes to Shaylor "Although our orientation is primarily research, we also are quite concerned with the humanitarian implications of extending proper medical services to the Indian, and would try very hard to lay a vaccination program onto our medical studies."

2. There was no consultation on which vaccine to administer.

As soon as Neel realized that measles was an imminent threat to the population he actively sought donations of measles vaccine. After his return from the field, he continued to try to get additional donations of vaccine. In April, 1968 Neel states that the CDC told him which companies to approach.

On April 22, 1968 Neel writes to Roche "Following receipt of your phone call, I contacted our Communicable Disease Center in Atlanta, Georgia, who suggested that I turn to Merck, Sharpe and Dohme, and to Philips Roxanne."

Neel attended a meeting at the CDC before he left for the field in November, 1967. Lindee (2001) indicates that he discussed vaccines and the vaccination process with them. He received gamma globulin from Parke Davis to administer with the measles vaccine to reduce side effects.

There have been numerous responses from epidemiologists and measles experts that Edmonston B was in use during the time period and was a reasonable choice (see IGES, NAS, Katz references cited previously). In addition, in April of 1968, Merck-- the manufacturer of the Schwarz vaccine -- appeared to be in a contractual agreement with the Venezuelan government and did not want to jeopardize this with a donation to Neel.

It should be noted that Neel had very little time to arrange for a large donation of vaccine. Shaylor wrote to Neel late in November about a measles outbreak. Neel was leaving for the field early in January.

Also, there was no money for the purchase of the expensive measles vaccines and Neel was dependent on donations.

On November 28, 1967 Shaylor wrote to Neel: Measles has reached Guaira Indians in Brazil and are preparing for the worst.

It probably took at least a week for Neel to receive this.

On December 11, 1967 Shaylor wrote to Neel: Reports of measles coming down the Orinoco from Brazil

After receiving the information from Shaylor Neel contacted several pharmaceutical manufacturers and got 2000 donated doses (received on December 19, 1967) of Edmonston B vaccine.

3. There was no permission from the Venezuelan government to vaccinate.

On December 11, 1967 Neel wrote to Miguel Layrisse, a human geneticist at the Venezuelan Institute for Scientific Investigations (IVIC):

> "I believe I can obtain about 2000 immunizing doses of vaccine free.CAN YOU OBTAIN PERMISSION FROM THE VENEZUELAN GOVERNMENT FOR US TO VACCINATE ALL THE INDIANS WE COME IN CONTACT WITH?"

There is an undated hand-written note that corresponds to this letter from Layrisse. It states in language that mimics the Neel letter:

"Agree bring 2000 immunizing doses measles vaccine"

This is not to be confused with the dated permission from Marcel Roche (another IVIC scientist) in 1968 concerning an additional donation of vaccine.

4. Neel received 2000 doses of vaccine. He brought 1000 into Venezuela. What happened to the other 1000 doses

On January 11, 1968 Neel was in Washington and dropped off 1000 doses in Georgetown. These were transferred to a representative of the Unevangelized Fields Missions (UFM) to be carried to Boa Vista and the Yanamomi there. This is confirmed in two places; by a letter from Napoleon Chagnon to Dan Shaylor (December 20, 1967) saying they would be dropping 1000 doses off in Georgetown and by an entry in Neel's field diary. 1000 doses were carried by Neel into Venezuela

This is not to be confused with the April 26, 1968 correspondence (Neel to Philips Roxanne Corporation) which discusses sending another 2000 doses to Venezuela. These doses are the ones that are about to expire and the company suggests doubling the dose, thereby effectively leaving Neel with only 1000 vaccinations (April 22, 1968 Neel to Roche).

5. There was an experimental protocol to vaccinate half the villages.

This idea originates with the January 9 letter from Willard Centerwall (one of the physicians who accompanied the 1968 expedition) purportedly accompanying the 1000 doses of vaccine given to the UFM missionaries. The major points of the protocol are:

1. Avoid vaccinating infants, especially under one year of age, tuberculosis patients, acutely ill people, and persons who are old and/.or infirm.
2. Vaccinate only half of the able-bodied village population at one time so the unvaccinated individuals will be able to care for the needs of the vaccinated ones.
3. Vaccinate populations which can be observed during the resting period (8-12 days post vaccination) so that any high fevers can be treated with aspirin and fluids and any bacterial complications can be treated with antibiotics or sulfa drugs.
4. Alert the people being vaccinated that they may feel a bit ill from the vaccination, but not as badly as the disease from which they are being protected.

This is not a research or experimental protocol, but a specific protocol for vaccinating in the field. There is a written addendum at the bottom of the page- if possible compare the reactions of the two makers of the vaccine-canine kidney or egg culture. This is a procedural reporting of information.

Both manufacturers of measles vaccine recommended the administration of 0.01cc/lb of body weight of measles immune globulin (MIG) to reduce the effects of the measles vaccine. The maximum dose stated is 0.5cc per individual. This dosage is based on trials with children up to a maximum weight of 50 lbs. There had been no studies of the mediating effects of MIG in adults, since adults had either been vaccinated as children or had had measles and were immune. Neel was sent 1000 doses of MIG which translated into 500cc of material. Centerwall noted in the January 10 letter to Francis Black, Associate Professor, Department of Epidemiology and Public Health, New Haven, CT, that this dosage would not be adequate to attend to the needs of adult Yanomami who weighed more than 50 lbs. His letter states:

> We have been able to look up most of the references relative to this and find as you suspected no support for 0.5cc of gamma globulin being adequate for measles vaccine modification for average adults. It would appear that the 0.01 cc per pound of body weight or 0.5 cc per individual statement refers mainly to children although it is not so stated and is thus ambiguous. We plan to avoid vaccinating the very young, the old and the acutely ill and will graduate our dosages as best we can on the remainder covering half villages at a time and following with aspirin where possible and when needed.

This implies that in the days before they left for the field they realized they did not have adequate supplies of gamma globulin and decided to do the best they could.

There is no evidence in the field notes that the team followed the half village procedure. In fact, once the epidemic arrived, the field team vaccinated everyone in the villages. The field notes also indicate that the vaccine was given with gamma globulin everywhere but at Ocamo.

6. Neel waited in Caracas for two weeks before entering the field.

Neel's field notes indicate that they spent time in Caracas waiting for transport to the field. They also document his frustration with not being able to get to the field sooner.

7. Neel was in conflict because he was more interested in science than in effort to give humanitarian aid to the Yanomami.

This statement can be found in Turner's recent work. Turner's position is actually a rather nuanced one, that

> '"science" on the scale of the AEC Orinoco expedition, is not merely an ideal system of abstract truths nor an activity of isolated, autonomous individuals, but a complex social activity, shaped by the collective institutions and sociopolitical conditions that make scientific research possible" (Turner 2001a:59).

> Within this social field,
> "The relative priority Neel attached to the fulfillment of what he deemed to be the essential parts of his research program ... over the medical needs of the Yanomami ... was to a large extent a function of the institutional requirements, pressures and expectations of government-funded Big Science" (Turner 2001a:59).

Turner bases much of his argument for Neel's ambivalence about the priority of Yanomami medical needs on the following statement in Neel's field notes, especially the last three words:

> Thus, I will get stools and soils while Bill does PE for three to four days-then we get blood, saliva, urine (? +dermat.), <u>then</u> inoculate if at all (5 February 1968 entry in field notes:80).

We suggest an alternate reading of the phrase "if at all", as follows.

It is important to note that Neel addresses the vaccinations specifically as a "a gesture of altruism and conscience" (5 February 1968 entry in field notes: 79) Likewise, he notes how frustrating this vaccination process is: "more of a headache than bargained for." However, he *never* suggests that he ever "seriously considered jettisoning the 'altruism and conscience' of the vaccination campaign and [abandon] the vaccinations altogether" (Turner, 2001b: 33); he does, however, clearly state in frustration that he would like to put the vaccinating into the "hands of the missionaries." Moreover, the context of "if at all" must account for the fact that the Indians had a history of fleeing those administering the vaccinations: "they took off in fright when they heard we were giving inoculations" (1 Feb. 1968 entry in field notes: 76).

Neel's note about vaccinating "if at all," administering the vaccinations "at the very last." (5 February 1968 entry in field notes: 79), or placing the vaccinations into the hands of the missionaries may be simply addressing this problem of "flight," and have no reference at all to any ambivalence on Neel's part about the vaccination program.

It should also be noted that this was all written before Neel was aware of the magnitude of the epidemic and before the "all-Orinoco" plan was devised. Once he was aware of the magnitude of the epidemic he immediately took steps to prevent further spread of measles. At this point, he gives preventative doses of MIG to those exposed, not yet sick, but not vaccinated. He also administers penicillin to those who are most ill. Neel clearly had a concern for the health of the Yanomami. This is documented by Salzano and Callegari-Jacques (1988) and Neel (1994) who discuss the various tests and other health measures they provided.

8. Neel had an upper respiratory infection and was not concerned with the health of the Yanomami

There is a good deal of information suggesting that Neel gave the health of the Yanomami a very high priority, quite apart from the vaccination program itself. We have not yet contacted medical experts who might help us address the problem of possible contagion to the Yanomami from James Neel's upper respiratory infection, mentioned several times in his field notes as producing annoying symptoms while he was in the field in 1968. We note that the question of whether Neel's URI represented a significant source of contagion while he was in the field, raised by Turner (2001a:33-34, 2001b:16-17), remains open. However, the general picture that has emerged from our inquiry is that Neel was careful about medical and quarantine issues.

Ernesto Migliazza, a member of the 1968 expedition, recalls that Neel never entered a new village without taking an M.D. with him and was punctilious in caring for the sick. On first arrival in a new village, medical doctors treated the sick. The doctors then helped in taking samples (blood, stool, saliva, and urine). Three doctors (in addition to Neel himself) were with the expedition, Dr. Marcel Roche, Dr. Willard Centerwall, and Dr. Bill Oliver. In his written report to the task force (Migliazza 01-07-23), Migliazza states that before leaving the U.S. every member of the expedition had a complete medical workup at the University of Michigan hospital, including x-rays, stool and urine samples, and half a dozen vaccinations. Members who were not health care practitioners received training in first aid, and all members received cultural training.

James V. Neel, Jr., MD (Telephone conversation with Hill, ? May 2001) recalls that on the expedition on which he accompanied his father, Neel had sick call every morning, left the expedition group in order to attend to medical emergencies (e.g. problematic childbirth), and insisted that JVN Jr. could not join his father's research team until he could contribute medical skills legally.

One kind of evidence for Neel's ambivalence about the priority of Yanomami health Turner (2001a, 2001b) was that Neel's group did not respond adequately to vaccine reactions that left Yanomami feverish and sick. The task force believes that Turner's concerns include a contradiction. Turner believes that the team should have abandoned its research schedule to, on the one hand, vaccinate as many people as possible, and, the other hand, to remain in individual villages to treat people with severe vaccine reactions. The expedition consisted of only eight or nine members, so could not really satisfy both concerns. It is likely that pursuing either strategy would have led to problems on the other front.

The view of the Task Force is that Neel and his expedition, dealing with an extraordinarily difficult situation, did the best that they possibly could to address the emergency. However, the experience of the 1968 expedition is indeed an instructive one, and requires us to reflect on its significance for anthropological practice. Can anthropologists be trained to respond more effectively to such emergencies? Should they be so trained? The AAA Committee on Ethics Draft "Guidelines for determining what constitutes a health emergency and how to respond in the course of anthropological research with human subjects" provides a very useful framework within which these questions can be discussed.

Beyond addressing health emergencies, the task force believes that anthropological procedures for work among populations with high levels of health risk require extensive discussion. Anthropologists working among such people might wish to work in teams with health practitioners or seek for themselves appropriate nursing and/or medical education and take adequate medical supplies with them to the field. However, the most practical solution is probably for anthropologists to (a) become thoroughly familiar, through sound research, with the health needs of indigenous populations so that they can give competent

advice (cf. Hurtado, Hill, Kaplan and Lancaster 2001), and (b) join in vigorous advocacy for indigenous peoples before their governments. The national professional associations may wish to set up formal commissions to pursue such advocacy; K. Hill (2001) has pointed out that individual anthropologists risk the suspension of research permits or other reprisals if they "go public" as individuals with advocacy for health care in the local context. Public health and care of the sick is certainly the responsibility of governments, regardless of what type of system they develop to provide it. Both the Brazilian and Venezuelan constitutions recognize health as a basic human right. The Venezuelan Constitution also recognizes the rights of its indigenous citizens to culturally appropriate health care: Title III, Chapter VIII, Article 122 states, "The indigenous peoples have a right to an integrated health care that considers their practices and cultures. The State will recognize their traditional medicine and complementary therapies, within a framework of bioethical principles". This is an excellent framework within which advocacy can be conducted, permitting the development of advanced health care systems while recognizing the potential of local practitioners to contribute to these.

The AAA El Dorado Task Force recommends that the AAA go formally on record, perhaps in a codicil to the AAA Statement on Human Rights, with a statement to the effect that people everywhere have the right to a healthy environment, to the best possible public health regime, and to a full range of medical care from basic clinical attention to specialized and advanced treatments. The AAA might wish to endorse the Venezuelan position that it is the responsibility of governments to guarantee the availability of such care. The responsibility of anthropologists would, then, be twofold. First, they should develop procedures to minimize the risk to indigenous populations caused by the presence of researchers. Anthropologists should follow rigorous quarantine regulations, ascertaining that they are free of communicable disease before entering the field, and discouraging the presence of others (tourists, film crews, etc.) who may not be subject to such rules. Second, they should strongly encourage research on health and illness in indigenous groups. Third, they should participate in advocacy for fully adequate health care for the groups with whom they work. Indigenous peoples surely feel sickness and death no less than any other human beings, and Yanomami representatives specifically have repeatedly asserted that the need for adequate health care is one of the most pressing concerns in their communities.

B. Informed consent and the 1968 Neel expedition

The Peacock Report raised the question of informed consent in connection with Tierney's allegations regarding Marcel Roche's experiments with radioactive iodine among the Yanomami beginning in 1958. The Task Force awaits material from Venezuela regarding these experiments. However, the question also arises in connection with Neel's expedition, since Neel collected biological materials among the Yanomami that remain under study. Thus it is important to evaluate whether or not these materials were collected with appropriate attention to informed consent. In discussions of the informed consent procedures that were used during Neel's 1968 expedition, it is important to recognize both the codes that were in force governing consent during that time and also to understand the way in which consent was actually obtained by researchers working with similar populations during that time period.

Important codes regarding informed consent in 1968

There are several excellent reviews of the history of informed consent by ethicists, philosophers, attorneys and historians of science (Beecher, 1970; Tranoy, 1983; Engelhardt, 1986; Faden and Beauchamp, 1986; Beauchamp and Childress, 1989; Gert, Culver and Clouser, 1997; Doyle and Tobias, 2001). Discussions on the history of informed consent often distinguish between the consent practices of practitioners of clinical medicine and the consent practices of researchers using human subjects. The earliest authors of treatises on clinical medical ethics were guided by the principle of beneficence and dealt very little with the principle of autonomy. Standards for research using human subjects began as a reaction to the medical experimentation of Nazi Germany. The ethical principle of respect for persons or autonomy was of primary importance in the resulting Nuremberg Code. This principle of autonomy was then - and continues to be -- articulated as voluntary or informed consent.

The Nuremberg Code became the model for many of the governmental and professional codes formulated in the 1950s and the 1960s, even though it presents an ideal without detailing the particulars of application. Among the most important codes and laws during this time period include the 1953 National Institutes of Health (NIH) Clinical Center code, the 1962 Drug Amendment Act and the 1964 Helsinki Code. All of these codes deal with the issue of informed consent. The Helsinki Code was formulated by

the World Medical Association and was used by many other agencies to develop their own guidelines. Unlike the Nuremberg Code, the Helsinki code distinguishes between therapeutic and non-therapeutic research. In 1966 the U. S. Public Health Service instituted a requirement of peer review of research, however, this was entrusted to the local institution and there was little oversight.

These codes were often difficult to apply. It was not until the 1970s that additional clarifications and standards were set. In 1971 the Department of Health, Education and Welfare issued guidelines for human subjects research. In 1974 Congress created the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. The commission was charged with developing a new set of guidelines for human subjects research. These guidelines became known as the Belmont Report. The report and the principles it represents, autonomy, beneficence and justice, have been codified into federal regulations and are routinely used by Institutional Review Boards (IRBs) in their analysis of research protocols. The National Research Council continues an on-going examination of ethics issues and prepares updated guidelines. More recently, the National Bioethics Advisory Commission established by Executive Order in 1995 was charged with making recommendations to the National Science and Technology council regarding both clinical and human biology and behavior research.

Neel's 1968 expedition to the Yanomami took place several years before the articulation of the bioethics principles in the Belmont Report. Although there were guidelines, the ways in which researchers obtained consent and explained risks and benefits were not firmly established.

One of the first documents to discuss the relationship of an investigator to a "non-westernized" study population was a 1964 World Health Organization (WHO) report. In 1962, the WHO convened a study group of scientists to discuss the organization of studies of "long-standing, but now rapidly changing, human indigenous populations". The resulting report, "Research in Population Genetics of Primitive Groups" (*WHO Technical Report Series*, 1964), was authored by James Neel. In the report Neel discusses the relations of the research team with the population studied. The study group met again in 1968 and produced a second report, "Research on Human Population Genetics" (*WHO Technical Report Series*, 1968), again authored by Neel, reiterating, with slight modification, the principles of the first report. The report states:

> Any research team has certain ethical obligation to the population under study. The investigator should always be bound by the legal and ethical considerations governing the conduct of medical and biological research workers. It is essential that harmonious relations be maintained both during and after each research visit. From previous field experience, the following factors have been found to be especially important.
>
> (a) The privacy and dignity of the individual must be respected at all times and the anonymity of subjects must be maintained in publications. The comfort and individuality of subjects must be safeguarded, e.g., some people are unwilling to queue, or to have others present during examination or questioning. Care should be taken that individuals do not undergo an excessive number of examinations at any one time.
>
> (b) Satisfactory reward should be provided for the subject's participation in the research and for any services provided. The nature of the recompense should receive careful consideration. The advice of local authorities may be invaluable, both on this question and in general, so as to avoid giving offence through ignorance of local customs.
>
> (c) The local population should benefit from such studies by the provision of medical, dental and related services.
>
> (d) The maintenance of congenial social relationships will be enhanced by methods suitable to particular areas, e.g. eating with families on occasion, exchange of information.
>
> (e) All groups have learned individuals, e.g., experts on oral traditions and those with systematized knowledge and interpretations of natural phenomena. Consultation and exchange of information with such persons will often be of immediate value to ensure good relations and lead to the appreciation of the achievements of such peoples. Such information is pertinent to their cultural and therefore biological history.
>
> (f) There should be the utmost regard for the cultural integrity of every group. All possible measures should be taken to prevent the activities and presence of the research team from adversely influencing the cultural continuity of the population being studied.

Issues of research involving indigenous populations were not examined in depth again until the United Nations Working Group on Indigenous Populations began meeting in the 1980s. Discussions in the United States in the 1990s on research among indigenous peoples were triggered by the 1990 Native American Graves Protection and Repatriation Act (NAGPRA) and the planning of the Human Genome Diversity Project. The National Research Council and the National Bioethics Advisory Commission both issued reports on research initiatives in the late 1990s.

Practices Relating to Informed Consent

In order to determine the practices of researchers in the late 1960s regarding informed consent, El Dorado Task Force member Trudy Turner surveyed a number of individuals who were active in the field at that time. The determination of individuals to consult was made by consulting various journals (*American Journal of Physical Anthropology, American Journal of Human Genetics,* etc.) to see who had published on genetics of indigenous populations in the late 1960s and early 1970s. Among the individuals responding were:

Alan Fix	William Pollitzer
Jonathan Friedlaender	Francisco Salzano
Eugene Giles	Jack Schull
Henry Harpending	Emoke Szathmary
Geoffrey Harrison	Kenneth Weiss
Newton Morton	

Individuals contacted did research in the following areas of the world and with the following listed populations and nations.

Ayamara	Solomon Islands
!Kung Bushmen	Canada
Japan	United States
Brazil	Haiti
Micronesia	Malaysia
Venezuela	Ethiopia
Paraguay	New Guinea

Each individual was asked the following three questions:

1. How did you attempt to get informed consent from individuals?
2. Did you have discussions about informed consent while you were in the planning stages of your research?
3. Did you exchange/reciprocate anything for samples?

1. How did you attempt to get informed consent?

Although there were some differences in responses about how information was conveyed to individuals, all of those surveyed stressed that voluntary consent was assumed since some individuals in the population elected not to participate. Some of the respondents indicated initially that they had approval from national or regional governments in the appropriate regions to conduct the research, while others dealt with the population or individuals. The leaders of the group under study were often consulted first and their approval was sought. If the researchers worked with medical personnel, the medical personnel were often responsible for obtaining consent. If they were not accompanied by medical personnel, researchers told the individuals/groups that they could not provide medical assistance. In every case some explanation of what the individuals were looking at in the blood samples was provided.

2. Was there any discussion of consent in planning stages of project?

Everyone said there was no discussion in the planning stages of the project.

3. What was given in exchange/reciprocity for samples?

If medical personnel were present, medical and dental exams were given. If a doctor was present, medical help or immunizations were provided. The following items were given: tobacco, candy, small

sums of money, photographs, toothbrushes, bubble gum, powdered milk, rice, machetes or a community purchase such as a film projector.

It should be noted that Neel did consult with local authorities concerning remuneration before his field work among the Yanomami. In a September 20, 1966 letter to Reverend Macon C. Hare he states:

> "With respect to the matter of trade goods, I would say that it has been our custom after we have completed the work-up of each family to make its members a suitable present. Here I would repeat, as mentioned above, that we would rely on the advice of those in the field concerning what is appropriate to the present situation. We know by experience that we must do something to enlist the cooperation of the Indian, but, on the other hand, do not wish to upset whatever "economy" you have been attempting to establish"

The impact of Neel's work with the WHO

The participants in the WHO meetings on research with indigenous populations went on to conduct research around the world. Many were involved in the Human Adaptability section of the International Biological Program (IBP) (Collins and Weiner, 1977). The participants and their students worked in the Kalahari, the Andes, New Guinea, the Solomon Islands and other locations. Many of the respondents to the survey were under the IBP umbrella. The WHO document Neel authored reflects the standard of conduct for work with indigenous populations as well as the protocols for obtaining samples from populations.

Resources on informed consent procedures

Publications on Informed consent are available from the National Research Council of the National Academy of Science (http://www.nationalacademies.org/nrc) and the National Bioethics Advisory Committee (http://www.bioethics.org). The charter of the NBAC has expired but the papers are still available on line. In addition, the AAA Committee on Ethics Draft Briefing Paper on Informed Consent cites other sources.

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C. Yanomamö Names

We consider here Tierney's allegation that Napoleon Chagnon caused distress and heightened the likelihood of interpersonal violence among the Yanomamö by violation of their prohibitions against the public utterance of personal names. This is an allegation singled out by the Peacock Report as deserving of inquiry.

Tierney makes a number of claim in regards to the nature of Yanomamö personal names and then claims that Chagnon repeatedly violated a name taboo. It is important to examine this issue because it goes to the heart of one of the complaints of unethical field work among the Yanomamö. That is, Chagnon broke a name taboo by collecting personal names and caused the Yanomamö a great deal of anguish and personal affront in the process and even caused conflicts between villages. In regards to the name taboo Tierney says "This was frustrating for him [Chagnon] because the Yanomami do not speak personal names out loud. And the names of the dead are the most taboo subject in their culture." (Tierney, 2000: 32). Both of these statements have elements of truth but require considerable qualification which we deal with below. Tierney's main accusations regarding breaking of the name taboo are found on page 32-33; 46-48, and 170 of *Darkness in El* Dorado. Although there are some elements of Chagnon's practice regarding the collection of names that we do question below, we believe Tierney's accusations to be, in the main, unfounded. There are a number of reasons for this. The first is that although names of the dead are subject to taboos they may be mentioned under appropriate circumstances. Second, names are used in every day conversation subject to the rules briefly described below (and in much greater detail in Chagnon's writings

and those of other ethnographers who have worked with the Yanomamö). Finally, Tierney's claim is self-contradictory. If names are not to be uttered, then how do the Yanomamö come by this knowledge?

We found it useful to consider what other ethnographers have written about Yanomamö names and the methods they used to collect names. Bruce Albert, a French ethnographer with considerable field experience among the Yanomamö of Brazil, states:

> "Traditional Yanomami names, which are nicknames and frequently pejorative to one degree or another, cannot be pronounced in front of a person or his/her close relatives—"to insult" is a synonym of "to name" in Yanomami (Albert 1985:394-404). But these nicknames circulate freely at a distance among unrelated people." (http://www.publicanthropology.org/Journals/Engaging-Ideas/RT(YANO)/Albert2.htm).

Chagnon makes a similar observation in *Studying the Yanomamö* (Chagnon, 1974: 95). He admonishes field workers about the collection of names in the following way "Do not accept an informant's statements about his own close kinsmen, and do not solicit them."

Ramos, discussing personal names among the Sanumá, observes that some people's names do become "public" because they are used as one element of teknonyms. That is, when a person is called, for instance, "Older brother of Sopai," Sopai's name must be uttered. To use such names makes those referred to "public figures," and denies to them one of the few rights of privacy that is closely guarded by Sanuma adults (Ramos 1995:184).

The fact that personal names can be used by individuals unrelated or distantly related to those being named is also recognized by Chagnon (1992: 29). Albert, in a passage just below the material quoted above describes how he used to two techniques for name collection that parallel those used by Chagnon (1992: 29; 1974: 91, 95). These methods are the use of informants from different villages and interviews with children:

> If the person does not have a Portuguese nickname, one should find out his or her Yanomami name from another person who is not a relative, preferably coming from another village. The question should be made discretely, out of earshot of the person named and close relatives. Children or leaders can be of great help in identifying Yanomami names: the former, because it is a fun game, the latter because no one is going to complain about being named by them (since publicly naming people is a demonstration of courage). (Albert and Gomez 1997:182-183). (http://www.publicanthropology.org/Journals/Engaging-Ideas/RT(YANO)/Albert2.htm).

Whether one can utter the name of an individual and the social circumstances under which it is permissible depends largely on the social status of the individual named, whether the person named is alive or dead, the degree of kinship between the speaker and the person being named, and the presence of others who might overhear the name. Chagnon has described these circumstances in a number of publications (e.g., Chagnon, 1968 [1st edition]: 12; Chagnon, 1974: 91-94; Chagnon, 1992: 23-30; Chagnon, 1997 [5th edition]: 19-21). In general, it is permissible to use the names of children in address or in reference. Likewise, it is permissible to address or refer to women by their personal names. However, it is more polite to use a kinship term to address a woman to whom one is not closely related.

Ramos reports for the Sanumá slightly different rules from those prevailing among the Yanomamö. She states that the names of the dead are not the object of special taboos (1995:200), and that Sanumá name secrecy has a "playful" quality in contrast to the dead seriousness about the issue encountered by Chagnon and Albert. She attributes this to the presence among the Sanumá of unilineality; if Sanumá could not say the names of the dead, they would have to give up lineages (Ramos 1995:205).

Among the Yanomamö, however, use of personal names for maturing males, mature men, or the dead regardless of sex is subject to a number of stringent regulations (Lizot, 1984: 125-136.). In a public context, it is inappropriate and insulting to address a man by his name or mention the name of a dead relative to a close kinsperson. In a private setting these rules change depending on the social relations and context that exist between speaker and listener. At one end, someone who is not related to a living or dead individual may freely utter that person's name so long as a close relative of the named is not within earshot. At the other end, it is not permissible to utter the name of a dead person who is closely related to the listener or even to ask the listener to name a close relative. To do so is a grave insult. In fact, if the

deceased's name was a common noun (e.g., tapir) the village in which the deceased resided before death may find a substitute for that term so as to not come close to breaking the name taboo (Lizot, 1984: 132). This practice coincides with Yanomamö belief that all remains of the dead (e.g., body and possessions) along with the name must be obliterated. Nevertheless, this last rule, under very special circumstances, can be legitimately circumvented if one establishes a prior agreement with the person being queried. Specifically, the person giving the name may not be a relative and the name should be whispered into the ear of the listener.

Finally , it is also clear from Albert that one is able to legitimately collect personal names although Albert believes that Chagnon may have done this unethically:

> Here, once again, the atypical "hit-and-run" fieldwork methods used by Chagnon in his frenetic schedule of collecting genealogies and blood for the AEC must have induced him to invent ad hoc measures for getting around Yanomami name secrecy in ways that were more aggressive and less ethical. Had he used the more typical slow pace and low-profile attitude that most anthropologists use during fieldwork, he would never have found himself in situations of having to resort to bribery, trickery, or offensive behaviors to collect names. The chaotic and peripatetic nature of his AEC agenda probably did force him into such situations. (http://www.publicanthropology.org/Journals/Engaging-Ideas/RT(YANO)/Albert2.htm).

It should be noted that Albert's statement above about "hit and run fieldwork" seems to be based on a reading of Tierney's accusations and not a reading of Chagnon's writings about name collection while working with geneticists (Chagnon, 1974: 93). On this point, Chagnon has written as follows:

> *2. <italOn once-only visits to a new village do not try to collect genealogies by using namesital>*...Much of my collaborative work with medical-genetics colleagues takes place in circumstances such as this. In these cases I write identification numbers on everybody and use kinship terms to discover the *<italprobableital>* biological relationship among those who are alive (Chagnon 1974:93)

We would also add that aside from Bruce Albert, other Yanomamö ethnographers such as Jacques Lizot, Alcida Ramos, Eguillor Garcia, and Marco Ales have collected Yanomamö names (of individuals both living and dead) and published them. Tierney (2000) contains many Yanomamö personal names, some accompanied by photographs.

The more interesting claim about the collection of names is that Chagnon used unethical methods in his genealogical research by relying on local pariahs, enemies, and children. To some extent, the use of these techniques was a consequence of the Yanomamö providing Chagnon with false information during the initial six month period of field research. This of course, does not excuse Chagnon for the tactics he may have employed to gain correct genealogies, but it does provide a relevant context. Of these three specific accusations it seems to us that the use of children and "bribing" of children is the most questionable. Current U.S. human-subjects regulations require special precautions on the use of children as informants . It seems reasonable to predict that if Chagnon were to submit research protocols that stated Yanomamö children were to be interviewed to collect genealogical information because adults were unwilling or hostile to such queries because of cultural taboos, his protocol would be denied. Even more problematic is Chagnon's use of the word "bribe" to induce children to reveal true names. Our standard definition of the term suggests that one uses a bribe to induce someone in a position of trust to do something he or she would not otherwise do. Nevertheless, we think it clear from Chagnon's research that children were only used during his earliest field work. They are an extremely poor source of genealogical information. But it should also be recalled that Albert (see citation above) defends the use of children as genealogical informants because no one take offense at a child uttering the name of an adult. We also note that, apart from payments to children, that appropriate payment for information is accepted under contemporary human subjects regulations, and can be distinguished from "bribing".

The use of enemies and the exploitation of personal animosities between Yanomamö (e.g., reliance on Rerebawä as an informant discussed in Chagnon, 1992: 27) are the other two issues in regards to the collection of names. Tierney states:

> Finally, he invented a system, as ingenious as it was divisive, to get around the name taboo. Within groups, he sought out "informants who might be considered 'aberrant' or 'abnormal,' outcasts in their own society," people he could bribe and isolate more easily.

> These pariahs resented other members of society, so they more willingly betrayed sacred secrets at others' expense and for their own profit. He resorted to "tactics such as 'bribing' children when their elders were not around, or capitalizing on animosities between individuals."32-33.

In the passage above, Tierney cites *Studying the Yanomamö* (Chagnon 1974:91). Here is the entire statement:

> I knew that it would be difficult to work around the name taboo, and I knew that important gains would be made only after I identified the good informants. As happened in Bisaasi-teri, the early good breaks came from informants who might be considered "aberrant" or "abnormal," outcasts in their own society, individuals like the twelve-year-old Karina, who guided me to Mishimishimaböwei-teri the first time.

This statement is about the difficulties Chagnon had early on in his dissertation research, during the first six months. It is unclear what Chagnon means by aberrant or abnormal. In the case of Karina, however, it is clear. Chagnon, 1974: 18) describes him as someone who spent much of his life in Sibarariwä's village (Mishimishimaböwei-teri) and returned to live in Mömariböwei-teri and was treated badly by his relatives in that village who seemed to consider him a stranger. Nevertheless, it is clear that Chagnon took advantage of aberrant or abnormal individuals to gain information about names. Use of such individuals to divulge difficult to acquire information is not uncommon in ethnographic field work and we need to carefully reconsider this issue.

Chagnon, both in his standard ethnography *Yanomamö* (1997, 5th edition) and in *Studying the Yanomamö* (1974) provide numerous examples of mistakes he made in the collection of names. He consistently admits he made mistakes and that he learned not to repeat them. Sometimes mistakes recurred after he thought he had made proper precautions (e.g., Chagnon, 1992:30). Yet Tierney seizes on these mistakes as Chagnon's standard practice when in fact they were not. It is our sense that many of the mistakes Chagnon made around names were honest and unintended and that he learned from these errors. We are, however, concerned about the use of children as informants as well as the use of aberrant and abnormal individuals. While these are "classical" anthropological field techniques, we believe that in today's environment, of increasing concern for the dignity and autonomy of human subjects, we should open a new dialogue on such methods.

D. Involvement in Yanomami Political Affairs

We discuss here the allegation in *Darkness in El Dorado* that Napoleon Chagnon put Yanomamö lives at risk in a peace-making negotiation in one instance, and by aiding a raiding party in another. The Peacock Report distinguished this as an allegation that required inquiry.

On page 112 of *Darkness in El Dorado* Tierney's account of Chagnon's role in fostering an alliance between Mishimishimaböwei-teri and Bisaasi-teri begins "He had some initial misgivings". Quoting Chagnon, Tierney continues: "This was taking risk in spades …I was also worried that I might be a contributor to an enormous disaster.' This citation from Tierney is taken from Chagnon, 1997 [5th edition]: 217. What Tierney does not tell the reader is that the "this" refers to an antecedent subject, Kaobawä, in the previous paragraph and actually indicates that Kaobawä was taking the risk (and not Chagnon) in attempting to establish peaceful relations with the enemy village of Mishimishimaböwei-teri. In Tierney's text this extract is followed by an ellipsis, followed by a quote from Chagnon: "I was also worried that I might be contributor to an enormous disaster." (*Darkness,*112). This quote is the start of a paragraph in Chagnon (1997:217) that is found two paragraphs below the "…risk taking in spades" paragraph. Here Chagnon debates whether he should assist Kaobawä in peace-making. He decides to assist because Kaobawä assures him that he will go ahead with or without Chagnon's help and convinces Chagnon that his presence will help him succeed, because Kaobawä believes that "…the Shamatari had accepted me and my role would be useful as a neutral intermediary and probably would contribute to the possibility of his success at making peace" (Chagnon, 1997 [5th edition] 217). It is clear from Chagnon's writing that the Yanomamö want to use Chagnon as an instrument of peace and that he obliged them at great personal risk to himself.

The second allegation made by Tierney about inappropriate political involvement on Chagnon's part that might have endangered Yanomamö lives concerns Chagnon's role in helping transport a raiding party. In the second edition of *The Fierce People* (1977), Chagnon describes how he assisted a raiding

party from Monou-teri, a village where he was residing and doing his research. The account begins on page 135 where he describes how "emotionally close…" he had become to the Monou-teri after watching a mortuary ceremony of a slain warrior and listening to his male relatives weep during the night. He states:

> I allowed them to talk me into taking the entire raiding party up the Mavaca River in my canoe. There, they could find high ground and reach the Patanowä-teri without having to cross the numerous swamps that lay between the two villages (Chagnon 1977: 135).

He later notes "Hukoshikuwä and his raiders did not locate the Patanowä-teri on this raid, although they searched for over a week." (p. 137)

Tierney remarks (2000: 87) that this assistance had given the raiders a significant advantage (citing Ferguson, 1995: 300). This is true. However, Ferguson (1995: 300) accurately notes, unlike Tierney, that the raiders did not locate the Patanowä-teri. Therefore, the raid was a failure and did not result in fighting.

In a direct reading of Chagnon's text we find the following (1977: 134-137): The raid was going to occur with or without Chagnon's assistance; he made it easier for the raiders by providing transportation; the raid failed.

Tierney, and to some extent Ferguson, seem to suggest that the failed raid would not have occurred without Chagnon's assistance. Chagnon's text clearly states that the Yanomamö had decided to make the raid and then asked him to help. There is no indication that the raid was contingent on Chagnon's assistance. In fact, the Monou-teri and the Bisaasi-teri had jointly or singly raided Patanowä-teri six times and Monou-teri had raided the Patanowä-teri alone on at least one of those occasions (Chagnon, 1977: 134). His description of his participation was on one of those occasions in which Monou-teri had raided alone. Nevertheless, it is clearly the case that Chagnon enhanced the probability of a successful raid by transporting the Monou-teri in his canoe.

Should ethnographers assist in the pursuit of feuds and alliances? In the case of the failed raid Chagnon felt emotionally and perhaps morally obligated to help. This obligation was probably a consequence of living with the Monou-teri where he internalized their animosity toward the Patanowä-teri as he listened to Yanomamö mourn their slain relatives. The Task Force believes that Chagnon should not have permitted the Monou-teri to "talk him into" taking them on a raid; indeed, Chagnon's language suggests that he himself regretted making that decision. We believe that ethnographers should not, with premeditation, directly or indirectly involve themselves in hostile acts. But one could imagine other circumstances where involvement in hostilities is unavoidable. What if the Monou-teri were attacked while Chagnon resided with them and his own life or the lives of his co-villagers were under immediate mortal threat? And what if the attackers were Brazilian gold miners?

That Chagnon assisted the Bisaasi-teri in brokering a successful peace treaty with the Mishimishimaböwei-teri is clearly praiseworthy However, we believe that the proper stance for anthropologists is to encourage those we study to make peace and not war, and to avoid direct or indirect facilitation of hostilities except in an emergency.

E. Engagement of anthropologists in public dialogue with members of study communities

Here we inquire into the lessons that anthropologists can learn from Chagnon's engagement in public forums with Yanomami individuals. The Task Force notes that this issue was not one singled out by the Peacock Report. Nor does Tierney raise this issue as a specific allegation in *Darkness in El Dorado*. Instead, it appears in the book as a sort of rhetorical background. For instance, Tierney repeats twice (pp. xxiv, p. 11) that Chagnon is said to have called Davi Kopenawa, a Brazilian Yanomami who has made frequent public statements over more than a decade, a "parrot." In spite of the fact that these statements by Chagnon were not singled out either by Tierney or by the Peacock Report, the Task Force wished to reflect upon them, since they exemplify an issue that is increasingly faced by anthropologists and that is, as far as we know, not generally raised in their training in methodological and ethical foundations. Anthropologists today all work in highly dynamic political contexts where both anthropologists and members of subject populations have access to many arenas, including international mass media, to advance their agendas.

Like most anthropologists working today, Chagnon has had to confront the emergence of new leadership styles and new forms of political discourse, some in direct opposition to his own ideas. Unfortunately, unlike, for instance, in North America, there is no established legal (such as NAGPRA) or political (such as a well-established system through which communities evaluate and approve research) framework within which he and Yanomami interlocutors can engage. Instead, direct access to Yanomami

is mediated within a very complex and often highly personalistic field of political players, within which the Yanomami themselves are, to date, profoundly subordinate. Nonetheless, some Yanomami do speak out.

Of all Yanomami who have emerged as public figures, probably the most important is Davi Kopenawa Yanomami. Davi Kopenawa Yanomami has worked closely with organizations such as CCPY and Cultural Survival in the fight for the integrity of Yanomami lands in Brazil, but has also spoken out on many other issues, often completely on his own. Interviews with him examined by the Task Force include Kopenawa Yanomami and Turner (Boa Vista, March 1991; Turner and Kopenawa 1991), Albert and Kopenawa Yanomami, April 8, 2001 (Albert 2001, Appendix 1), and Kopenawa Yanomami and Chernela (Demini Village, Parima Highlands, Brazil, June 7 2001), the last conducted in the name of the AAA El Dorado Task Force.

Regarding public statements by Davi Kopenawa Yanomami, Chagnon has written as follows:

> Davi Kobenawä Yanomamö was educated by the New Tribes missionaries, a mostly American Protestant group, in a village on the Demini River in Brazil. There he learned Portuguese. His non-Yanomamö supporters in Brazil, intelligent and well-intentioned advocates of the Yanomamö cause, are promoting him as a spokesman for his people. Such a role exists largely because *our* culture must deal with other cultures through their leaders – it is the only way we know how to deal with them. Everything I know about Davi Kobenawä is positive, and I am confident that he is a sincere and honest man. When I read his proclamations, I am moved – but I am also sure that someone from our culture wrote them. They have too much the voice of Rousseau's idealism and sound very non-Yanomamö. My concern is that he is being put into a difficult position, fraught with consequences for the future of the Yanomamö. For one thing, there is currently no such thing as a pan-Yanomamö awareness, and so he cannot possibly be speaking for the Venezuelan Yanomamö ...
>
> There is also the danger that if outside parties can so easily create Yanomamö leaders, everyone who has a special interest will promote his own leader. For example, in 1990 the Brazilian mining interests introduced their own Yanomamö leader, a young man they called Marcelo Yanomami, who advocated their rights just as strongly as Davi Kobenawä advocates the policies of his mentors (fortunately, in my view, the latter are more consistent with the Yanomamö's future well-being). In an article in the Brazilian journal *Veja* in January 1990, Marcelo Yanomami, obviously reflecting his mentors' interests, argued that the Indian has the right to exploit the material riches in his territory in any manner he wishes – presumably meaning the right to turn them over to powerful Brazilian mining interests for a modest fee (Chagnon 1992:275-76; repeated in similar language in Chagnon 1997:252).

We are unable to confirm that Chagnon ever referred to Davi Kopenawa Yanomami as a "parrot"; this language is quoted by Tierney from an article by Peter Monaghan in the *Chronicle of Higher Education* (Monaghan 1994:A19) and is not there attributed to Chagnon directly. Monaghan states " Mr. Chagnon and his supporters dismiss [Davi Kopenawa Yanomami] as a parrot of human-rights groups and say he does not speak for the tribe." However, the above citations are the only published writings by Chagnon on Kopenawa Yanomami that we have seen cited, or identified ourselves. They are carefully worded and say nothing about "parrots". However, we suggest that Chagnon's remarks were problematic in their context. They were written at a time when there was the most serious threat to Yanomami lands; between the mid 1980's and 1992, when Yanomami lands in Brazil were finally demarcated with their present boundaries, Brazilian anthropologists, accompanied by other anthropologists (the AAA's own efforts are briefly reviewed in Part I, Section D), international NGO's such as Survival International, and the Yanomami themselves were engaged in an extremely difficult and dangerous fight to protect these lands. To raise questions, in very widely-distributed publications, about the authenticity of a person who had unquestionably become a very positive symbol of the Yanomami and an important political asset in this fight, could not fail to undermine Yanomami interests. We note also that the opinion ascribed by Chagnon to Marcelo Yanomami is an opinion that a rational person might well advance, and advocates a right of full control over their lands and resources that Arvelo-Jiménez and Cousins (1992) have argued Indians should be allowed to have.

Laura Graham, a linguistic anthropologist with considerable experience with shifting styles of indigenous leadership among the Xavante of Brazil, has written very thoughtfully on the challenge posed to anthropologists who must respond to indigenous spokespersons. She points out that linguistic

anthropological theory, following Bakhtin, acknowledges that no one is ever fully the author of his own words. Furthermore, she argues that "authenticity" is a "colonial folk category" (Graham 2001:6). Graham argues that to challenge the "authenticity" of a speaker "is a political statement. It is a challenge of boundaries and presupposes asymmetrical relations of power. Such challenges cannot be grounded in an evaluation of the performance as "indigenous" or not because, in the global context, indigenous performance is, by nature, decontextualized, reinvented and hybrid" (Graham 2001:27).

We would argue that Chagnon's point -- if pro-Indian NGO's can create puppet spokespersons, then anti-Indian exploiters can do the same – can be turned back on his challenge to "authenticity." Anyone can mount such a challenge, for good or for evil. Thus anthropologists should avoid this rhetorical strategy. Furthermore, in our view the challenge of inauthenticity is, fundamentally, unanthropological. It fails to recognize the contemporary context in which indigenous people must live, and it fails to grant indigenous speakers autonomy and agency. Certainly it is fair to ask an indigenous spokesperson hard questions about for whom he speaks, or to argue against his position, or even to say that he is a liar. However, such challenges should be made in specific and dialogic terms, directly engaging the content of the speech and the voice of the speaker, rather than simply bypassing both as devoid of intentional content. And they should be made in arenas that include those where the speaker has some chance of answering.

We do, of course, recognize that a thunderous blast against an indigenous leader in the international media may from time to time be precisely appropriate, if an anthropologist has very good information that such a person is a danger to a community. However, to be effective, such attention must be developed on a number of fronts (including in the local contexts), in culturally appropriate ways, and include a "full disclosure" of the anthropologist's role in the political context.

The lesson that we take from Chagnon's engagements with emerging Yanomami leaders for the development of anthropological practice is discussion of this and similar cases should be a part of anthropological training, because the future of anthropology will certainly increasingly involve the necessity to maneuver in complex and dynamic political fields such as that presented by the current developments among the Yanomami.

F. Allegations of inappropriate sexual relationships with Yanomami by anthropologists.

We take up here the allegations made in *Darkness in El Dorado* about inappropriate sexual contact with Yanomami boys and young men, in exchange for gifts, by the French anthropologist Jacques Lizot. We note that Lizot is not the only anthropologist who has had sexual relations with Yanomami people; Kenneth Good (1991) has written about his relationship with his ex-wife, Yarima, whom he married within the Yanomami system when she was about fourteen years old, establishing the marriage under Venezuelan civil law at a slightly later time. Good, of course, has written with considerable frankness about this relationship and its eventual sad end, providing ample material for reflection by all anthropologists who may be drawn to such relationships. In contrast, Lizot has had much to say about the sexual life of the Yanomami themselves, but, as Tierney quite correctly observes, has implied that his involvement in this life was entirely that of a neutral observer. The El Dorado Task Force believes that the allegations about Lizot's activities among the Yanomami made in *Darkness in El Dorado* are well-founded. These activities continued over many years. We have confirmation independent of Tierney's book that many people knew about them, beginning in the late 1960's. One of Lizot's habits that aroused suspicion was his use of teenage boys as guides. Contrary to Tierney's statement (2000:127) that many anthropologists preferred hiring young boys as guides, in fact most anthropologists when they could find them preferred mature men who knew the territory better, had many allies, and were competent hunters. One of our sources states that he saw Lizot inviting young boys to his hammock in his village. A number of Tierney's sources reported that Lizot attracted and rewarded boys with gifts of the type that would usually be made only to senior and influential persons.

We have also found support for Tierney's allegation that the kinds of sexual behavior that Lizot encouraged were not acceptable to Yanomami. Lizot's sexual exploitation of adolescent boys was greatly resented, very repugnant, and totally unacceptable to the Yanomami, and left them suspicious of anthropologists in general, whom they fear may share Lizot's sexual proclivities. Hames, a member of the Task Force, spoke in 1998 to an elected Yanomamö leader, Fermín (from the village of Cejal), who was a deputy to the *municipio* established for the Ye'kwana and Yanomami in Venezuela. This leader officially stated that he had personally spoken to three or four Yanomami boys who had had relations with Lizot and strongly objected to Lizot's prostitution of boys and young men.

We believe, however, that it is unfortunate that Tierney focussed so extensively, and exclusively, on Lizot's case. While we share Tierney's view that Lizot's behavior was unacceptable, and are baffled that Venezuelan authorities and the Salesian missionaries permitted it to continue over a very long period of time, we must point out that sexual exploitation of the Yanomami that is far more dangerous to them than anything undertaken by Lizot is reported by those who have observed the behavior of soldiers around Brazilian army posts, where young Yanomami women (and probably boys as well) are prostituted in a context that includes epidemic levels of venereal disease including AIDS (Peters 1998:247). The Yanomami have requested that the posts be withdrawn from their territory.

In reflecting on the Lizot case, we observe that anthropologists, like other human beings, are sexual creatures. Inevitably, sexual attraction and sexual relationships will develop between anthropologists and those they encounter during field work, including members of the populations under study. Every anthropologist is familiar with successful long-term partnerships that began in such relationships, and every anthropologist is equally familiar with cases where such partnerships failed, or where relationships seemed from the beginning to be ill-advised and exploitative. The task force notes that sexual exploitation is not always imposed by the anthropologist on a member of the study population; there are cases in the literature of the opposite type, including violent rape (Kulick & Wilson). There are also cases where members of study populations cynically exploit the attractions they hold for an anthropologist to gain access to perceived wealth or privilege. Nonetheless, the task force points out that in most field situations, most of the power in a relationship with a member of the study population will reside with the anthropologist. Given this problem, we believe that sexual relationships with members of study populations should be undertaken only after the most careful reflection on this point, and with full attention to the dignity and autonomy of the potential partner. The contemplation of sexual contact with children or young adolescents should not survive such reflection and attention. Indeed, we note that the United Nations Declaration of the Rights of the Child (http://www.unhchr.ch/map.htm) specifically protects children against "exploitation." Furthermore, entering into sexual relationships in a responsible way requires special attention to ethnographic considerations: What material and emotional responsibilities is the anthropologist assuming, not only to the object of his or her desire, but perhaps to a wide network of that person's kin and friends? Is the anthropologist prepared to take on these responsibilities? What is expected in the way of duration of sexual relationships? Will the anthropologist be able to manage in an ethical way the consequences of the dissolution of a relationship that is shorter than an expected duration, especially where children may be involved? Very few anthropologists will be prepared to think seriously about such questions until many months or even years of fieldwork have passed.

There is a considerable debate about the place of sexual involvements in participant observation. Indeed, in the case of Kenneth Good's documentation of his own marriage to Yarima, his attraction to and affection for her was accompanied by a realization that a marriage to her might solidify his relationships in the study community (Good 1991). Some anthropologists have taken the position, parallel to that generally held in other professions, that a sexual involvement should never be a tool of fieldwork; others have pointed out that in some situations, such as work in bathhouses that are sites of transitory sexual liasons, not to participate in sexual contact would be to adopt a perhaps even more dubious sexual role as voyeur (cf. Bolton 1992).

One solution is obviously to carefully observe not only international but local laws, against, for instance, sexual involvement with legal minors. However, anthropologists are familiar with situations where local laws are profoundly unjust, as in regimes where miscegenation, homosexuality, prostitution, or adultery are felonies or even capital crimes. In some cases, a larger responsibility strongly militates in favor of work with populations among whom such illegal sexual practices occur, for instance, a study of the behavior of prostitutes and their clients in a community at serious risk of the spread of sexually-transmitted disease. In such cases it is difficult to prescribe absolutes about behavior. We recommend again very careful reflection on the possible consequences of using sex as a fieldwork tool. For instance, if the forms of sexuality in which the anthropologist will be involved are illegal, the anthropologist will probably be putting the partner at considerably more risk than is faced by the anthropologist him- or herself. Anthropologists may be relatively conspicuous presences in a community, and thus draw attention to the illegal sexual activities of partners. The penalty faced by the anthropologist might be deportation, but the partner may face very serious consequences including ostracism or even execution. Anthropologists should not only be thoroughly familiar with local law, but should be able to answer the most probing ethnographic questions about the possible consequences for a partner of an illegal or even of a legal liason. It is unlikely that an anthropologist will be able to answer such questions after a short period of fieldwork.

We believe that anthropologists contemplating sexual liasons of any type should reflect also on their responsibility to the discipline. From the time of Boas, we have recognized that behavior by any anthropologist reflects on all anthropologists. An image and reality of probity and responsibility benefits the discipline as a whole, while an image, or, worse, a reality of sexual libertinage or irresponsibility restricts the ability of every future anthropologist to develop research at the field site that has been thus compromised. Indeed, such compromise may put a future anthropologist at genuine risk, for instance of rape if it is believed locally that all anthropologists are sexually loose. From this point of view, one of the early goals of ethnographic involvement perhaps should be to determine the sexual behavior appropriate to a person with a high reputation, unlikely to be the object of gossip, in the local community (which may ramify into regional and even national contexts), with the goal of adopting that sort of behavior. Anthropologists will inevitably be the objects of gossip and thus may have to hold themselves to a slightly higher standard than would a local person. However, as we have noted above, some have argued that exceptions can be advanced, where participation, or the appearance of participation, in sexual contexts that may be locally regarded as illicit would be evidence of adherence to a higher ethic of addressing significant human problems.

Finally, we urge that the issue of sexuality in anthropological practice be addressed in the training of anthropologists. Discussion of the sort of case-study literature presented in recent volumes such as (Kulick & Wilson 1995, Lewin & Leap 1996, Whitehead & Conley 1986) should be a standard component of methodological training, and development of this literature should be recognized as a legitimate contribution to anthropological practice and theory. The Committee on Ethics has drafted a "Briefing Paper for Consideration of the Ethical Implications of Sexual Relationships between Anthropologists and Members of a Study Population" (COE November 2001), and has presented also a draft "Plan of Action for Developing Dialogue on the Ethical Implications of Sexual Relationships between Anthropologists and Members of a Study Population." We urge that the briefing paper be widely read and that the plan be carried out.

G. Warriors of the Amazon.

The Peacock Report noted that one allegation requiring inquiry by the El Dorado Task force was a report that "a film crew allegedly watched a woman and child die during a NOVA documentary filmed with the assistance of [Jacques] Lizot." This film is currently entitled "Warriors of the Amazon," and is distributed by NOVA/WGBH. The date on the current edition of the videocassette version is 1996, and Jacques Lizot is listed as ethnographic advisor to the film. Several versions of the film seem to exist. Tierney observes that a BBC version, entitled "Survivors of the Amazon," also dated 1996, "showed more of the film crew's impact on Karohi-teri (Tierney 2000:219-20). (We note that the NOVA version does include a sequence of a headman mentioning that goods received from the film crew will be used for trade during the feast, and that these include "machetes, axes, hammocks, cotton for fixing arrow feathers, and money.")

Task Force member Hames has identified yet a third version of the film, listed as shown at a Margaret Mead Film Festival at USC in 1995, with the following information:

> Spirits of the Rainforest - (Venezuela)
> Andy Jillings, Jacques Lizot. 1993. 50 minutes.
> The Yanomami of Venezuela invite their enemies to settle old scores and feast. When sickness and sudden death threaten the preparations shamans call upon healing powers from the spirit world, but their traditional defense offers no protection against new diseases carried by gold prospectors.
> http://www.usc.edu/dept/elab/mead/mead95/desc95.html

Members of the Task Force have viewed only the NOVA/WGBH "Warriors of the Amazon" and in this preliminary report will remark only on that film. The author of the narration for the film is Melanie Wallace, who produced several films in the *Odyssey* series (Trailer, "Warriors of the Amazon"). A transcript of the narration of the film as it was aired on PBS September 2, 1997 can be found at http://www.pbs.org/wgbh/nova/transcripts/2309warr.html.

Sponsel (1998:99) states that "Among the several dozen films and videos on the Yanomami, in my opinion by far the most balanced and humanistic is *Warriors of the Amazon*, which Lizot made in

collaboration with the television science series *Nova*." This is the film that was viwed by Task Force members.

The film unquestionably has some very compelling footage, and has as well the redeeming feature of giving at least brief voice to a Yanomami woman about her decision to run away from her village. However, the Task Force concurs with Tierney that the film is profoundly problematic. It is particularly problematic given that NOVA/WGBH is obviously marketing it to schoolteachers; the NOVA web site (see above) includes a selection of lesson ideas and supplementary materials to accompany the film (it can be purchased for $19.95 from amazon.com).

First, the film, made in the 1990's, is obviously staged (Tierney enumerates a number of pieces of evidence for this (Tierney 2000:216-217). The film is incongrous in that while it shows many trade goods, the Yanomami wear almost no western clothes (one or two men in shorts are shown). Task Force member Hames states that one of the most striking incongruities of the film for him is the sight of so many shotguns and the sound of so much firing, since most Yanomami communities have few shotguns and are careful of ammunition. In spite of the fact that one of the film's themes is diseases brought by contact, the narration of the film's introductory trailer states that the film will bring "a rare and intimate glimpse of an isolated tribe. Explore the unique culture of the Yanomami, from the role of powerful hallucinogenics to the ritualistic consumption of their dead, witness the human drama of a people on the brink of extinction. Can they make peace with their enemies before it's too late? Warriors of the Amazon." The film builds to a climax with the funeral of a woman and the statement that four people died while the film was being made. This narration reiterates themes of primordiality, isolation, cannibalism, and extinction that have endured for hundreds of years in representations of the indigenous peoples of the Americas.

The narration continues stating that the Yanomami world is "marked by aggression and revenge", and that the Yanomami "live in ways similar to their ancestors of two thousand years ago, following age old traditions...". As noted above, the filmed scenes accompanying this essentializing narration support it in every detail. Interestingly, the narration, which follows a plot line where a Yanomami village reconciles with enemies, has the headman stating that the fight started over an adultery, with the narrator reiterating this point: "Instances of adultery are common, but when it takes place among members of different groups, what begins as a personal affront can quickly escalate into a conflict among villages. And here, vengeance is not easily satisfied."

One of several narrative threads in the film is the sickness and death of a very young woman, who had recently given birth, and her baby . Tierney characterizes these sequences as "memorable, beautiful, marketable, ... inevitable ... images of [an] Amazon Madonna." He argues that the representation of this death was one of the many metaphoric and literal references in the film to "Indians as losers in the Darwinian struggle" (Tierney 2000:222). We concur.

The images of the dying young mother and her baby are problematic in at least two more ways. Before proceeding, we note that Tierney leaves the impression that the filming of the death is long and voyeuristic; in fact the shots are very short in the context of the film's pacing. This may very well be due to the fact that taking pictures of a dying person, even more of a dying infant, and even more especially of a dead person, must have enormously upset the Yanomami. Task Force member Hames believes that it must have taken an enormous amount of trade goods to overcome their objections and permit the film makers to quickly grab a few shots.

The second way in which the death is immensely problematic is that it is filmed as a moment in "nature." Tierney states that the film was made only an hour by motorboat from the infirmary at the Mavaca mission (Tierney 2000:221). Hames states that this is an exaggeration; the distance might be as much as 3 1/2 hours, depending on conditions and mode of transportation. Nonetheless it would have been easy to take the woman, who is quite young, perhaps even still a teenager, to the hospital. Tierney reports that a government doctor visited the woman, but did not have proper equipment to treat her. This scene, which would have interrupted the film's vision of wilderness isolation, is not shown. Nor is the film crew shown as intervening in any way; Tierney quotes an American missionary, Mike Dawson, as saying, "Let's be real. They're giving them machetes, cooking pots, but they can't give a dying woman aspirin to bring her fever down?" (Tierney 2000:217).

One member of the Task Force reacted initially by proposing that the AAA request that the film be withdrawn from circulation. The Task Force decided not to make such a proposal, but we remain faced with the problem of reflecting on how the film might possibly be made meaningful. Perhaps its most important lesson is not about the Yanomami at all, but about exactly the power of a representation in which the Yanomami are more part of "nature" than of "culture"; we are reminded of Charles Briggs' work on

the cholera epidemic in the early 1990's among the Warao, where Briggs (1997:447) interviewed a Venezuelan health official who stated that "The Indians – they're people who accept death as a normal, natural event in their lives. And when an Indian dies, it's not anything trascendental: an Indian dies and nothing happens. Or, let's say, there isn't, there isn't this, uh, fondness for life, or anything like that". This representation is sufficiently constitutive of the affective state of the film crew that they are apparently able to maintain complete non-interventionist detachment, taking much the same position of fatalism as if they had observed the death of a mother and infant baboon on the African savannah. Had they filmed the same sort of footage in, say, Glasgow, San Diego, or Osaka, we believe that the tone of the film would been one of intense attention to finding help for the sick woman. There is a grim lesson here for us all: decent ordinary people, in the grip of a racializing representation that the film reproduces in almost every dimension, can behave in ways that deeply shocked members of the Task Force as well as Tierney and his informants and that must have been a dehumanizing experience for the Yanomami.

We are concerned, unfortunately, that it will be difficult to use the film in classes in such a way as to bring students to confront this issue. Since every shot and almost every narrative moment in it powerfully reproduces a racializing and dehumanizing representation, it seems likely that extensive preparation and deep teaching and discussion at an almost psychotherapeutic level would be required to overcome its power with many of our students. Certainly nothing in the NOVA material for teachers (which include cheery lesson plans about "the rain forest") even remotely engages what we see as the film's most fundamental lesson. Tierney's chapter might be one teaching tool that could be used with the film; his analysis is clear and revealing.

[i] Throughout this document "Turner" will refer to Terence S. Turner. The less frequent references to Trudy R. Turner, member of the AAA El Dorado Task Force, will include her first name.

[ii] We thank Terence Turner for providing us with copies of his most recent manuscript (Turner 2001a) and the manuscript of Stevens (2001).