

## Darkness in Anthropology: A Conversation with Napoleon Chagnon

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***Editor's Note:** Napoleon Chagnon's career as an anthropologist has been fraught with controversy. His extensive field research in the Amazon with the Yanomamö tribe over an expanse of thirty years documented frequent warfare among the people and supported a theory of violence as an evolved human reaction to threats by neighbors, an idea contrary to the cultural determinist inclination of many scholars in his discipline. While his early monograph, *The Yanomamö: The Fierce People* (1968) became a classic in anthropology and is still widely read and assigned, Chagnon was branded a biological determinist and accused of ethical lapses in his treatment of the natives, accusations that were eventually proven groundless. His election to the prestigious National Academy of Sciences in 2012 was a crowning endorsement of his career as a social scientist and yet also renewed the controversies surrounding him, which continue to this day. In this interview, Prof. Chagnon discusses his work and its implications, the opposition it has generated, and what it all reveals about the current state of anthropology.*

***Iannone:** How did your interest in anthropology begin? What made you want to be an anthropologist?*

***Chagnon:** My original major as an undergraduate in a local two-year college—Michigan College of Mining and Technology—was physics.*

At that time I had never heard of anthropology. I transferred to the University of Michigan after my first year and discovered that “physics” was in the College of Literature, Science and the Arts and I would have to take courses in each of these fields. The only thing I could fit into my schedule for the social science requirement was a course in a field called anthropology. It was taught by a man who was very prominent in the field, Elman R. Service. I enjoyed it enough that I took another course in anthropology the next semester, this time from a man who was even more prominent than the first: Leslie A. White. I immediately changed my major from physics to anthropology after Prof. White assured me, when I visited him during his office hours, that I could “make a living” at being an anthropologist. Anthropology dealt with questions about humans and human nature that were, to me, far more interesting than, for example, the laws of nature or the physics and chemistry of life.

***Iannone:*** *Can you give us a general sense of the state of the field when you entered, as it seemed to you then, the major figures, methods, issues, controversies? Were you especially inspired by anyone?*

***Chagnon:*** The dominating figures in the late fifties and early to mid-sixties were Margaret Mead, Claude Levi-Strauss, George Peter Murdock, Julian Steward, A.R. Radcliffe-Brown, Raymond Firth, Alfred Kroeber, Robert Lowie, Bronislaw Malinowski, Edmund Leach, Meyer Fortes, and both Service and White at my own university, but many others were emerging as prominent “theoreticians” in the field and some of the ones just mentioned were waning in influence. Also, disagreements had by the 1960s divided the field into “cultural evolutionists,” “structuralists,” “functionalists,” “ecological and environmental determinists,” “personality and culturists,” “cultural materialists/Marxists,” and a half-dozen other “ists.” Major problems revolved around questions such as: How is history different from evolution? How are cultures held together as they grow? Can the function of institutions be understood from their structure? In what way are civilizations more complex than tribes? Does the biology of humans have any relevance for understanding the nature of culture? Is there such a thing called “human nature”?

***Iannone:*** *What made you decide that you would research the Yanomamö, a tribe in the Amazon who had very little contact with the outside world?*

***Chagnon:*** I had originally chosen to study a different tribe in Brazil, the recently contacted Suyá, but a military coup happened in Brazil after I was funded by the National Institute of Mental Health but before I left for the field. I therefore chose to study the Yanomamö, who were then very poorly known. The Yanomamö resided in a border region between two countries—Brazil and Venezuela—so I assumed that if one of the countries had a revolution, I could study the same tribe in the other country.

***Iannone:*** *Reading your most recent book, Noble Savages (with the provocative subtitle My Life Among Two Dangerous Tribes—the Yanomamö and the Anthropologists), I was amazed at the physical strength and endurance you needed, the risks you took, the loneliness you must have endured, and the sacrifice of ordinary life you had to make in order to live for long periods with the Yanomamö, and to do the kind of research you did. Was that typical of classic anthropological research at the time?*

***Chagnon:*** By the mid-1960s much of the tribal world had disappeared and foreign anthropologists were having difficulty getting permission from other governments to study tribesmen in their countries. Thus, fewer people were able to study isolated and relatively un-aculturated tribes because of these political difficulties as well as the inconvenience and discomforts that this kind of study entailed. So, it would be fair to say that my study was relatively unusual for that time, especially my frequent return field trips for thirty years.

***Iannone:*** *I understand that many anthropologists who study tribal life do so for shorter periods of time than you did. As a result of the extensiveness and thoroughness of your work, you made some major discoveries about the nature of native peoples, and I guess by extension about the nature of human beings in general. Could you mention/summarize some of your major findings and accomplishments?*

***Chagnon:*** This is not possible to summarize in a few paragraphs. One very general thing I recognized and documented was that when tribesmen had not

been decimated by introduced diseases, defeated militarily by invaders, and forced to live on reservations, their social and political institutions played a much bigger role in how they organized their lives—especially if the nation states surrounding them had little or no influence on them and were not interfering with their affairs. Their political relationships with their tribal neighbors are usually much different. I, for one, gained a deep appreciation of how the members of small communities of fifty to one hundred people must be aware of what their neighbors are doing, especially if they are not closely related to one another. Under these conditions one gains new insights into the importance of politics and dispute resolution that are not possible when the tribe being studied is, for example, confined to a reservation or its freedom to negotiate its relationships with neighbors is subject to the approval of the surrounding nation states.

In addition, the fact that most of the Yanomamö villages were demographically “intact,” that is, they continue to experience the high birthrates and high death rates of preindustrial society, means that there are many fewer older people compared to the more numerous younger people and children, so much more time is devoted to caretaking and parenting by the adults. These circumstances make it possible for anthropologists to gain a more informed idea of how our ancestors probably budgeted their time—in both domestic activities within the community and political activities between groups. But the quality of Yanomamö political activities has more to do than just with the availability of “time”—it also has to do with their freedom from constraints from the political states (Venezuela and Brazil) that surround them. Few anthropological studies have been able to document this. This a very important but rarely experienced aspect of life in the tribal world. It was as if each village was itself a sovereign power, like a small state with the power to make war and appropriate its neighbors’ lands, territories, and women.

***Iannone: Your book, Yanomamö: The Fierce People (1968), became a classic in the annals of anthropology and a staple in classrooms. You were something of a hero for a time, an Indiana Jones of anthropology. But when did you start to see that you were rubbing some people the wrong way?***

***Chagnon:*** I began to understand that a small number of colleagues were feeling “rubbed the wrong way”—as you put it—when they began criticizing some of my work in less than academically appropriate ways, like making

accusations that did not follow from what I had claimed in the publications they were criticizing. Over time their criticisms increased in severity and academically unacceptable ways. Most of my critics worked in or near the Amazon Basin and among tribes similar to the Yanomamö and much of their motivation stemmed from professional jealousy, which gradually became much more political in tone, like the claim that my publications were “causing harm” to the people I studied. This eventually shifted to the claim that I was guilty of “racism” and “genocide” in my writings—they called it “academic genocide”—but it was genocide nevertheless. The “nicety” of “academic” was soon dropped. These accusations came principally from colleagues who were studying tribes in the Amazon basin who were political activists and who subscribed to Marxist anthropological views.

***Iannone: What do you think especially bothered your academic foes about your work? The idea of an unchanging, biologically-based human nature; of an evolved facultative human tendency to violence in high-threat circumstances, which you claim your research supports; of how women figure in tribal society; of supposedly negative things being documented about non-white people?***

***Chagnon:*** Probably the single most anthropologically unacceptable thing I did was to take “biology” seriously, because it seemed to be important in Yanomamö political circumstances and because the demographic (population) structure of the Yanomamö was almost the classic form that demographers used as the model for a non-contracepting human population. In these circumstances the population had not sustained debilitating epidemics and had not gone through the “demographic transition” that most human populations had experienced—the transition from high to low in birthrates and death rates. Indeed, many of my “discoveries” that shed light on contemporary populations were essentially demographic discoveries. These discoveries raised two issues in the contemporary anthropological milieu in which I was trained. One issue had to do with the “meaning” of kinship, and the other had to do with the generally negative attitude historically about “biological” explanations of cultural phenomena in cultural anthropology.

I began using commonly known ways to “measure” relatedness between organisms—Sewall Wright’s coefficient of inbreeding and its extended concept, the coefficient of relatedness. This was very unusual in cultural anthropology. In short, I was discussing Yanomamö kinship with *the biological meaning of*

kinship in mind—at a time when the vast majority of cultural anthropologists assumed that “kinship” and “the biological meaning of Yanomamö kinship” were, at best, only vaguely similar and any allusion to this was generally “suspect.” But since “kinship” was traditionally a central focus of anthropological theory, I began developing ways to show precisely, using my meticulous genealogies on the Yanomamö, that people took sides in fights according to how closely *genetically* they were related to each other and fought against people they were *less related to genetically*. Many of the most prominent anthropologists held the extreme view that “kinship” among humans had *nothing* to do with the biological meaning of kinship as is assumed in the fields of biology, genetics, or animal husbandry. A common claim was something to the effect that “whatever kinship among humans was about, it was NOT about biology.”

By 1974 I was attempting to shed additional light on Yanomamö social and political behavior by using Sewall Wright’s widely known coefficients of relatedness and inbreeding. As I read more and more work in what was emerging as “evolutionary biology,” I realized that I was trying to do what William D. Hamilton had done in a much more sophisticated way in 1962 in his two classic papers on *inclusive fitness*, now more widely known as “kin selection” theory. In 1975, E.O. Wilson published his widely acclaimed book, *Sociobiology*, and touched off a wave of public reactions from individual academics in the social sciences, including the cynical reaction of one of my former professors, Marshall Sahlins, in a book he hastily rushed to press entitled *The Use and Abuse of Biology* (1976). The distinguished English theoretical biologist, Richard Dawkins, immortalized a central argument in Sahlins’s book by naming it “the Sahlins Fallacy”: that kin selection could not possibly apply to humans because most languages do not have words for the fractions needed to calculate relatedness. That’s like saying that rocks cannot fall according to the laws of gravity because rocks cannot calculate their mass.

I—and other social scientists and anthropologists—publically defended E.O. Wilson and his academic freedom to extend the arguments of W.D. Hamilton, G.C. Williams, and other theoretical biologists in explanations of some human social behavior regardless of how antagonistic cultural anthropologists such as Sahlins were to these ideas. And of course, this made me very unpopular among those cultural anthropologists who yet subscribe to the view that all human behavior is learned and “cultural” and none of it is the consequence of our evolutionary past. In short, there is no such thing as a “human nature”—there is just a “cultural nature.”

***Iannone:*** *What can we learn from the Yanomamö about the position of women in early human society? How does that differ from how feminist-inspired anthropology depicts it today?*

***Chagnon:*** The “position” of women varies widely in human societies, depending importantly on the mode of descent, i.e., women in matrilineal societies generally enjoy more privileges, because political authority and wealth generally passes through them to their female and male descendants. But matriliney was a recent development in human societies and patrilineal and/or bilateral descent was more common before agriculture and animal domestication became established. I think most women in most of today’s democratic societies are, on average, better off than were their tribal ancestresses. I think your allusion to “feminist-inspired anthropology” tends to be “feminist-inspired political theory” and is not necessarily “anthropological theory” in the general sense of that topic.

***Iannone:*** *I suppose the view that modern women are better off might unsettle many feminists, who would like to believe that civilization “constructed” the sexual differentiation they see as oppressive. On another note, E.O. Wilson and the introduction of sociobiology was important to your work and to your discipline. You were present at the conference when Prof. Wilson received his famous dousing with ice water by radical activists from a group called the International Committee Against Racism, who couldn’t bear to hear his views. What did sociobiology bring to anthropology and what were the objections to it? Was the origin of the split between what are now called “cultural” and “scientific” anthropologists?*

***Chagnon:*** I believe that one of the most shameful events in the history of twentieth-century American science was the attack on E.O. Wilson at the 1978 meeting of the American Association for the Advancement of Science in Washington, D.C., during a debate about the very scientific *permissibility* of using theories drawn from the fields of biology and psychology to explain some aspects of human behavior. Wilson’s *Sociobiology* brought to anthropology—and other social sciences—a new and fresh way of looking at human behavior and attempted to make it more comprehensible in the context of primate evolutionary tendencies. But the “split” within anthropology had developed several years

before this, and had more to do with the political opposition of mostly Marxist anthropologists who objected to using ideas based on evolutionary theory drawn from Darwin's theory of natural selection in explanations of human behavior. Nevertheless, there always were cultural anthropologists who did scientific work and even those who had an abiding respect for Darwin's views, but they became a minority within the profession when postmodernism and radical activism gradually replaced common sense, tolerance, and even civility in some branches of cultural anthropology.

***Iannone: The biological basis of sociobiology does need more physical evidence, though, doesn't it? Prof. Wilson has indicated that.***

***Chagnon:*** I think you may have mis-phrased Wilson on that point. The "biological basis of sociobiology" is firmly established in a theoretical sense. However, the general field is new enough that additional studies of animals—including humans—are, of course, needed to increase the robustness of the findings thus far, particularly in areas such as inclusive fitness theory. More researchers should apply their efforts to these and similar problems. However, this is no longer a controversial science.

***Iannone: Is it fair to say that your vision is also materialist, in a different way from Marxism? You say in your book: "It was as if the last two bastions of opposition to the theory of evolution by natural selection were fundamentalist fire-and-brimstone preachers and cultural anthropologists." In a New York Times article, the statement becomes more sweeping: "The last bastions of resistance to evolutionary theory are organized religion and cultural anthropology."***

***Chagnon:*** It is not clear to me that the above two statements I made are different. All scientists are "materialists" in the sense that they subscribe to the principle that there is cause and effect in the world around us and that the world has a material reality that can be detected by human observers. It is possible to be a materialist without being a Marxist, but lately I'm beginning to suspect that many Marxists have abandoned materialism in favor of postmodernism, whose adherents seem to believe that a real world *does not exist* and that there are merely arbitrary "ideational constructs" of human minds. Since these mental constructs do not have a material basis and are arbitrary, they



cannot be verified by repeated observations by neutral parties because there is no such thing as a “neutral” party nor a real, material world. So, it is possible for Marxism to be rooted in antimaterialism and therefore non-materialist Marxism is a logical but not very useful possibility.

***Iannone:*** *There were missionaries, both Protestant and Catholic, among the Yanomamö who made the earliest contact with the native people, even before you. But you don't say much about the effects of Christianity on the people in your book. Why is that?*

***Chagnon:*** I tried to spend little time in Yanomamö villages where Christian missions—both Evangelical and Catholic—had been established. There were over twenty thousand Yanomamö living in some 250 widely scattered villages. I was interested in discovering the demographic, political, and social patterns that were found in the most remote villages. While studying acculturated mission villages is useful, the opportunity to study villages with no meaningful direct contact with outsiders was a nearly unique opportunity. My field study lasted for some thirty years and villages that were completely isolated when I first began studying them eventually began to have contact, particularly with the Catholic Salesian Missions, who constantly pressured them to move out to navigable portion of larger rivers that the Salesians used, the Mavaca River in this case, closer to their main mission. This had unfortunate consequences for Yanomamö mortality rates and general health in several cases, which I publicized in the *New York Times* and the *Times Literary Supplement* of the *London Times* in 1993.

***Iannone:*** *You had good relations with the local missionaries, both Protestant and Catholic, for years, but then the Salesians began to oppose and undercut you, adding to your woes, getting you into trouble with South American anthropologists and politicians. Why was that?*

***Chagnon:*** There were very few mission groups in contact with the Yanomamö in 1964 when I first arrived. Members of the Evangelical New Tribes Mission made first contact with several Yanomamö villages located on or near the Orinoco headwaters where this river was small enough that the Yanomamö could cross it easily during the dry season. The Salesians only took an interest in the Yanomamö as a consequence of the presence of the New Tribes Mission and aggressively began a campaign to drive them out of

the area, which they eventually did in the late 1990s. However, in 1964 the only active Salesian mission was located at the mouth of the Ocamo River—after they successfully forced the original mission group, the New Tribes Mission, out. This happened several times at several locations.

I generally got along nicely with both the evangelical New Tribes missionaries and the few Salesians who were there in the mid- to late 1960s, and I still correspond regularly with a few of the New Tribes missionaries who remain my friends today. The single Salesian priest I became good friends with, Padre Luigi Cocco, was replaced—against his will, by a younger, more aggressive batch of Salesian priests in the mid-1970s—and eventually returned to Italy, where he died in 1980.

As my answer to your previous question indicated, my relationships with the Salesians worsened over time until they became very antagonistic. In the late 1990s, the Salesians filed clandestine complaints against me with La Oficina de Asuntos Indígenas—the Venezuelan Indian Commission—through the male converts they were grooming to be the new political leaders, making it appear that these “leaders” reflected the collective view that it was the Yanomamö themselves who did not want me to return to their villages. This happened at about the time I published the two criticisms of their policies in the *New York Times* and the *Times Literary Supplement*. One of the Salesian spokesmen, a priest named Donald Delaney, privately assured me—several times—at the annual meeting of the American Anthropological Association (AAA) held in 1994 in Washington, D.C., that the Salesians would “get even” with me for my attacks on them in the world’s two largest English-language newspapers. This particular meeting of the AAA included at least two sessions intended to “reconcile” my disagreements with the Salesians and, hopefully, to lead to better relationships between us.

***Iannone: Patrick Tierney’s Darkness in El Dorado (2000), was probably meant to be another spectacular exposé of long-accepted scholarship along the lines of Derek Freeman’s criticism of the work of Margaret Mead. Among other things, Tierney accused you of instigating the violence you documented and even of causing a measles outbreak among the people—charges shown to be without merit. It was a shoddy piece of work by Tierney, although some of it had been published in the New Yorker, which is ordinarily meticulous about documentation. You reveal in your***

***book that some of your academic foes knew it was poor scholarship, but lent it credence hoping it would harm you. Who were some of the offenders and how do they explain themselves now? Was there any fruitful scholarly debate over your work or just campaigns of vilification?***

**Chagnon:** It is quite possible—given the dates of some critically important incidents in the sequence of remarkable events that happened in the last few years of my active academic career (say between 1994 and 1999)—that Patrick Tierney’s *Darkness in El Dorado* was an important component in how the Salesians orchestrated “getting even” with me for my attacks on them in my two 1993 publications. This would make for an interesting thesis by some historian of science who asks: Did the Salesians commission Tierney to do a hatchet job on Chagnon’s career? Why did Tierney abruptly stop working on his completed manuscript for Viking Press—a book that focused on how the illegal Brazilian miners devastated the Brazilian Yanomamö—and change the theme to how geneticist Dr. James V. Neel and Prof. Napoleon A. Chagnon, by conducting Nazi-like experiments on the Yanomamö to test their reprehensible eugenics and sociobiological theories, caused a measles epidemic that killed hundreds if not thousands of Yanomamö?

For the eleven-year period following the publication of *Darkness in El Dorado* there was almost no “fruitful” discussion or debate about Tierney’s accusations against me and Neel. None of the elected officers of the AAA (or those anthropologists who were appointed by them) who presided over my “inquisition” and that of Neel (who died before Tierney’s book was published) ever contacted me during their witch hunt to ask my opinion of the claims they made against me or my explanation of the events. For this reason I consider the American Anthropological Association an unprofessional and reprehensible organization because it seems to operate on Orwellian *Animal Farm* principles that while all anthropologists are equal, some anthropologists are “more equal.” Alice Dreger, a historian of science, drew attention to this in a well-researched article in *Human Nature* in 2011. Her forthcoming book, *Galileo’s Middle Finger*, will discuss other instances of academics being left to fend for themselves and being largely abandoned by the academic associations that should have defended them from attacks by their peers who were presumably “more equal.”

***Iannone:*** *Yes, she showed Tierney's charges to be baseless and faulted the AAA for spreading his falsehoods and failing to defend real scholarship. Anthropology as a discipline appears to be in trouble, and your career has helped bring that to light. First, there are the longstanding false premises, Marxist and Rousseauian, that may well have tainted much of the work of the past. You mention that some of your own professors held what you now see as erroneous views. The work of Margaret Mead, doyenne of anthropology in its populist heyday, was exposed as largely mistaken. On top of all that, the discipline is now infected with postmodernism, cultural relativism, thoroughgoing politicization of research and scholarship, and outright political activism on the part of anthropologists. What are the effects of all these influences on the discipline as you see it?*

***Chagnon:*** The attacks on Margaret Mead by Derek Freeman have recently been reexamined by anthropologist Paul Shankman, who finds Freeman's critiques to be unfair, inaccurate, and unscholarly. Whether this will play any major role in resurrecting Mead's former high repute remains to be seen. As someone who has been unfairly targeted, I think it is always a good idea for other people to take a second look at the accusations when they come largely from a single source.

Of all the trends you mention, I believe that "outright political activism on the part of anthropologists" is the most dangerous and destructive. This is primarily because such political activism is almost invariably accompanied by attacks on specific academics whose scholarship is ruthlessly falsified and whose integrity and ethics are not only called into question, but shamelessly denigrated by academically jealous colleagues or by associated political activists, especially personnel of some NGOs, whose objective is to dominate the future political importance of the native peoples. Worse yet, implausible as most of them are, these false claims are made without criticism or rebuttal by those who use similar tactics, and then repeated in other publications until they are accepted, because the perpetrators and their confederates do not operate by the rules of established academic standards. Soon they spread by a law of their own being, like debilitating viruses. A good example is, in part, Sahlins's indignation at my election to the National Academy of Sciences, which seems to rest largely on the falsehoods about me and Neel published by Tierney in the excerpt that the *New Yorker* published of his book.

***Iannone:*** You do say that the vehement opposition you faced over many years took a toll on you and your life. But your election to the National Academy of Sciences must seem like a mighty vindication of your career after all the difficulties you've had. Not all your foes have been silenced, however, and you certainly name names in your book. Does the fight continue? And what is ultimately at stake?

***Chagnon:*** My election in 2012 to the National Academy of Sciences was a great honor and, hopefully, a reflection on the quality of my academic research and its significance in science. While it should not simply be taken as a “vindication for the wrongs inflicted on me,” many people also look at it in that way and, to a certain extent, this is justified. My election to the National Academy of Sciences was immediately followed by two events of considerable importance to me personally and professionally. First, I was appointed as an adjunct research professor at the University of Michigan, where my years of accumulated data on the Yanomamö will be placed into an archive and made publically available to qualified researchers in other universities. Second, I was offered a position at the University of Missouri in Columbia with the title “Distinguished Research Professor and Chancellor’s Chair for Excellence in Anthropology.” I will be working on my Yanomamö data there, organizing and annotating it, and publishing data-filled articles with my University of Missouri colleagues, my new postdoc, Shane MacFarlane, and my longtime research partner, Raymond Hames of the University of Nebraska.

It is probably correct that not all of my foes are silenced and the struggle on behalf of science and the freedom to engage in it continues. In this connection I received word as I was working on this interview that a major foe of scientific research among Native Peoples, Steven Corry, the director of the NGO Survival International—possibly *because* of my election to the National Academy of Sciences and the resignation of one of my former Marxist professors, Marshall Sahlins at the University of Chicago, to protest my election—announced a forthcoming new major attack on me, Jared Diamond, and Steven Pinker because we three allegedly perpetuate “the myth of the brutal savage” in our recent publications. I invite the readers of *Academic Questions* and the members of the National Association of Scholars to follow this story and, perhaps, weigh in on this interesting forthcoming inquisition. Many of those opposing what I have done among

the Yanomamö and who have vilified me and others are in fact among the named people I mention in *Noble Savages*; Steven Corry is one of them and a classic example of an activist who is doing well by doing harm.

***Iannone:*** *My discipline of literature is also a mess, but the literature remains, along with some superb literary criticism. What is the solid structure of anthropology that still holds through all the turmoil? You say that uncontacted or relatively uncontacted tribal life is fast disappearing. What are the legitimate areas of study for anthropologists today?*

***Chagnon:*** There is some hope now that American cultural anthropology will survive in an altered form through all of this turmoil. A recently formed section of cultural anthropology called the Evolutionary Anthropology Society attracts those cultural anthropologists who might otherwise simply leave the American Anthropological Association and possibly cripple it even further. Members of this new section engage in the methods of science and also use evolutionary approaches in their research as distinct from denigrating it.

While “uncontacted” tribes have disappeared, there are many fascinating studies to be done among traditional farming and herding people as well as enclaves of distinct ethnic subgroups in major cities of the industrialized world.

***Iannone:*** *Maybe someone should study the tribe of anthropologists and the ongoing warfare within it.*

***Chagnon:*** This is getting too long. Perhaps I’ll take this up another time.