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Amazonia and the Origin of the State: An Interview with Robert L. Carneiro (1927-)

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Robert L. Carneiro (1927-) is the Curator of South American Ethnology at the American Museum of Natural History in New York. He earned a B.A. in political science from the University of Michigan in 1949. He continued graduate study in anthropology at the same institution, where he was a student of Leslie White, the well-known evolutionary anthropologist. In 1953-54, with his wife Gertrude Dole, he conducted fieldwork among the Kuikuru of the Xingu National Park in Brazil. That work, among the earliest systematic fieldwork to be carried out in the Xingu River basin, provided the basis for his Ph.D. dissertation, "Subsistence and Social Structure: An Ecological Study of the Kuikuru Indians" (1957). After receiving his Ph.D. in 1957 and briefly teaching at the University of Wisconsin, Carneiro moved to the Department of Anthropology of the American Museum of Natural History (AMNH), in New York, where he has remained ever since. In 1960-61 he carried out fieldwork among the Amahuaca of the eastern Montaña region of Peru, and in 1975 returned to the Kuikuru. He is adjunct professor at Columbia University and author of five books and numerous articles. Carneiro's work explores the origin of the state and the evolution of society. He is a member of the National Academy of Sciences.

Janet Chernela, interviewer, has known Robert Carneiro since 1970 when she worked as his assistant in the Department of Anthropology of the American Museum of Natural History. The interview process took place via email, while the interviewee was in Rhode Island and New York and the interviewer in the Amazon and Maryland. The "virtual interview" process, as they called it, would permit Dr. Carneiro to answer each question in writing at his convenience and to edit responses. He was also encouraged to participate in the question generating process, and did so by adding a sixth question regarding writing.

JMC: Do you think there is anything relevant about your background or upbringing that led you to anthropology?

RLC: In my pre-high school years there was nothing that drew me to anthropology in particular. But we had at home the great eleventh edition of the Encyclopedia Britannica, which I prized then—as I do to this day. I teathed on it. It made me a bit of an intellectual snob in that I looked down on my schoolmates, who had only the Book of Knowledge at home to use for their school reports, while I had the Britannica! That marvelous work introduced me to the wonders and pleasures of scholarly research.

Another similar influence was Ridpath's ten-volume History of the World, profusely illustrated with Gustav Doré's wonderful steel engravings of the great events in history. I loved to pore through it, just looking at the illustrations. But Ridpath's also served to arouse my interest in history, especially of the Middle Ages and of Ancient Egypt. I suppose this work can be said to have stimulated two different interests in me. One was in other cultures, present, but especially past. The other was an interest in the change in things over time. These two interests were greatly amplified when I got into anthropology. Moreover, I sup-

pose that this early interest in history was one reason I was so easily “captured” by cultural evolution once I entered anthropology.

Without much systematic thinking about it, during my high school years I was drawn to a kind of “secular determinist” view of things. I had a completely non-religious upbringing. Neither of my parents was religious, although it was a subject never discussed at home. My parents never tried to mold my thinking about religion, one way or the other. So you might say that, in that regard, I grew up “like Topsy.” I never had then, nor do I have now, any supernatural or “spiritual” component to my thinking. This attitude, of course, was further reinforced once I began studying anthropology.

JMC: What was the state of anthropology as you came to know it? How did your understanding of what we call Anthropology change over time? Please include data about your bachelor’s and graduate studies.

RLC: My undergraduate major at the University of Michigan was in political science, but as a senior I took a couple of courses from Leslie White and discovered that anthropology was what I was really looking for. I graduated in 1949 and after being out of school for a year, working for my father and coming to realize that the business world was not for me, I returned to Michigan for graduate work in anthropology in the fall of 1950.

At that time, anti-evolutionism was still firmly in the saddle in cultural anthropology. Michigan was the only place where evolutionism was being taught as a valid and fruitful approach in the study of culture. I distinctly recall that at anthropology meetings it was Michigan against the field. We Michigan graduate students had knockdown, drag-out discussions with graduate students from other departments, especially Columbia, Chicago, and Northwestern. But the fact that the numbers were strongly against us didn’t faze us. We were sure we were on the right side of the argument and that evolutionism was bound to be reaccepted into anthropology. Moreover, although Leslie White never urged us to do so, we graduate students felt we were the spearhead of the movement that would eventually lead to the rehabilitation of evolutionism.

Although White was clearly the leader in the resurgence of evolutionism, we recognized that he had two important allies in this struggle: Julian Steward and V. Gordon Childe. I had read Steward’s article “Cultural Causality and Law” back in New York when it appeared in 1949, and was strongly impressed by it. From it I learned (among other things) the word “diachronic.” To help prepare me for returning to Michigan and beginning graduate work in anthropology in the fall of 1950, White urged me to read Childe’s *Man Makes Himself* to see how much the evolutionary approach—especially from a materialist perspective—could teach us about the dynamics of history.

And indeed we had the satisfaction of seeing evolutionism become increasingly reaccepted into anthropology. The turning point—if one has to be picked—was probably 1959, the year of the Darwin centennial. That year there were countless conferences and symposia among many sciences in which the great intellectual achievements that evolutionary thinking had wrought were being celebrated. And American anthropologists, it seemed, began to ask themselves, “How can an approach that has proved so successful in all the other sciences continue to be spurned by cultural anthropology?” And in fact for a time evolutionism returned in triumph to anthropology.

But much to my surprise and dismay, this triumph turned out to be short lived. Beginning around the mid-1970s, the tide began to turn and a new anti-evolutionism emerged. The old cultural relativism and humanism, out of which the original anti-evolutionism was born, were apparently not dead but only dormant. It reawakened and began to spread its wings—or should I say its tentacles? And in this reassertion of its opposition to evolutionism it was greatly abetted by the newly-sprouted philosophy of postmodernism. The result was a new and reinvigorated anti-evolutionism—a good deal more virulent than its predecessor.

I keep being reassured by sympathetic colleagues that the influence of postmodernism in anthropology is waning, but I have still to be convinced that it is. Although, as a firm believer in the operation of natural selection—on ideas as well as on organisms—I believe that in time postmodernism’s failure to yield positive, tangible results will lead to its demise. In the meantime we will apparently have to continue to put up with “personhood,” “mime-

sis,” “alterity,” and similar words with which postmodernism’s cornucopia of terminology so readily overflows.

One more striking trend in anthropology that I’ve noticed since I entered the field has been its fragmentation. I suppose, though, that this was inevitable as more and more anthropologists were being turned out, fewer and fewer primitive societies remained, and as it was found that the anthropological approach could be applied successfully to all manner of things, trivial as well as important. One has only to look through the pages of the Anthropology Newsletter to see the number of splinter groups into which the Association has been divided.

JMC: What were your considerations in electing to work in Lowland South America?

RLC: When the time came to do ethnographic fieldwork, my choices seemed to narrow down to two areas of the world: New Guinea and Amazonia. I did not want to be the sixth ethnographer working in a Pueblo village in the Southwest. I wanted a native village as nearly pristine as possible. And it seemed to me that those two areas afforded the widest choice of the sort of village I was looking for.

JMC: Did fieldwork in Amazonia influence your theorizing issues of social evolution and state formation, and, if so, how?

RLC: As an undergraduate at the University of Michigan I had majored in political science so that I already had a special interest in political organization when I went into the field. But I had never concerned myself specifically with the origin of the state. Thus I did not have that problem in mind when I first set foot in the Kuikuru village in 1953. Now, having been trained under Leslie White, what I *did* have was a deep interest in theory, especially as it related to cultural evolution, which of course was concerned with the origin and development of things.

I should explain my attitude toward theory. I felt strongly that the function of theory was to explain fact. The vague and vacuous theories that emerged from the pen of social scientists like Talcott Parsons had no interest for me. Moreover I felt strongly that the best theory came from immersing oneself in the facts. Phenomena, it seemed to me, had locked within them, the secrets of their origin and development.

That was the general understanding I had of the relation between theory and facts when I went into the field. Little did I imagine, though, that the ethnographic facts I was about to encounter and wrestle with would lead me, step by step, and over the course of time, to formulate a general theory of the origin of the state.

To the extent that there was a reigning theory of state origins at the time I undertook fieldwork, it was what I called the “automatic theory.” This amounted to the belief that once agriculture came on the scene, allowing a certain proportion of the population to be divorced from primary food production, it would lead inexorably to a series of evolutionary steps culminating in the rise of the state. What I was learning in the Kuikuru village, however, cast serious doubts on this theory.

Now, I already knew something about the Inca Empire and thus was I able to compare it with what I was learning about the Kuikuru. Even with their simple system of manioc cultivation, Kuikuru agriculture was more productive than that of the Inca, whether measured per unit of land or per unit of labor. The “automatic theory,” then, could not very well be true. Something more than just agriculture was involved in giving rise to the state.

Let me make it clear, though, that the theory I came to formulate did not occur to me full blown while I was in the field. Still, my field experience was essential in providing me with the ingredients which would lead me to propose an alternative to the “automatic theory.”

This is not the place to go into the theory in detail. Suffice it to say that environmental circumscription was the key to it. It was lacking in Amazonia, but present in the Andes. Environmental circumscription changed the outcome of warfare from a process of “fight and flight,” so typical of the encounter among Amazonian groups, to one in which conquest, subjugation, and amalgamation was its usual result. Thus, in the Andes—unlike in most of Amazonia—autonomous villages, in time, gave way to chiefdoms, and, in more limited cases, chiefdoms gave way to states.

Now, if the theory seemed to account for the rise of states in South America, could it do so for other parts of the world as well? Was it a *general theory*, in other words? When I began looking at other areas where early states had emerged—the Nile valley, the Tigris-Euphrates valley, the Indus valley, the valley of Mexico, etc.—the circumscription theory seemed to hold.

Thus the partnering of theory and fact, which I had long espoused as a matter of principle, had borne fruit when applied to actual cases. A theory, the germ of which had first been planted in a tiny village in central Brazil, had ultimately given rise to a theory with a claim to having solved a general anthropological problem.

JMC: You are well known for your work on Kuikuru manioc cultivation and your integration of that work into your formulation of theory.

RLC: As soon as I became familiar with the system of Kuikuru manioc cultivation I began to question some of the prevailing notions about what was, and what was not, possible for a society subsisting under such a system. For one thing, the Kuikuru case showed me that slash-and-burn did not necessarily force an Indian village to move every few years. The Kuikuru had maintained their village in the same locale for close to a century. They had moved the actual village site three times, but only a few hundred yards each time. And the moves were for ritual reasons, not ecological ones. For example, one such reason was coming across the bones of old burials when digging fresh graves. That disturbing occurrence had indeed precipitated one of their moves.

When the Kuikuru were first seen by Europeans in the 1890s their village contained about 200 persons. When I first studied them in 1953 their population was 145, but when I revisited them in 1975 their population was down to about 115, 50 or so of their members having moved to the Yawalapiti village over fears of witchcraft by a Kuikuru widely reputed to be a sorcerer.

It also used to be said that a swidden society could not produce a surplus of food. Well, the Kuikuru produced a substantial *seasonal* surplus of manioc, and could easily have produced a *yearly* one had they felt any need to do so, which they did not.

It was also commonly held that swiddeners had little or no leisure time after subsistence. Well, on the average, Kuikuru men engaged in subsistence no more than about four hours a day, and therefore had loads of time left over, much of which they devoted to performing ceremonies—of which they had some seventeen. The women, though, were a different story. They played a large part in the harvesting of manioc, along with the men, and when it came to processing the poisonous roots, they did all of it.

Finally, it was stated by Betty Meggers that tropical forest villages could not exceed 1,000 in size. Well, I was able to show that the Kuikuru could have supported a village twice that size, and without its having to move. Moreover, this surmise appears to have been corroborated by Michael Heckenberger's archaeological surveys and excavations in the Kuikuru region, which revealed prehistoric villages of at least 2,000 persons. Several of these prehistoric village sites were surrounded and—presumably protected—by an extensive system of trenches, which, I was sure, had been built for defensive purposes. More recently these trenches have been mapped and further described by Michael Heckenberger.

In 1960 I presented a paper at an AAA meeting in which I disputed Betty Meggers on a number of these points. I can say that in this regard my surmise that under favorable ecological conditions Amazonian villages could exceed 1,000 in population has become generally accepted by Amazonianists.

After working with the Kuikuru I tended to think that other Amazonian Indians were pretty much like them in these regards. But my subsequent fieldwork among the Amahuaca in eastern Peru in 1960-61 showed me how wide off the mark this view was. I won't go into all the differences, but one major one was that the Kuikuru lived close to rivers and lakes which were abundantly stocked with large fish. As a result, they had come to rely almost exclusively on fish for their protein, and did virtually no hunting.

The Amahuaca, on the other hand, who lived in headwater country, where the rivers were narrow and shallow and had relatively few fish, did very little fishing; hunting being almost their sole source of protein. It is well known that hunters deplete the game in sur-

rounding areas in a relatively short time. Consequently the Amahuaca not only had much smaller settlements—30 in size as against 145—but had to move them frequently.

All of this led me to see that in ecology lay the major determinants of village size and settlement pattern—and therefore of social organization as well. And only fieldwork could have provided the solid evidence for this conclusion. Again, we see that fact underlies theory, and, when examined carefully and with the appropriate perspective, fact gives rise to the soundest theory.

Another observation I was led to make is that there is nothing like *comparison* to enhance one's understanding of phenomena. I was fortunate in having studied two very different Amazonian societies. It gave me a perspective I otherwise lacked.

JMC: I think of your writing as fluid and readable at the same time as it is academically rigorous and elegantly argued. Would you like to comment on your writing?

RLC: I am very much aware of how central writing is to my work in anthropology. And if a semi-autobiographical piece like this is to reflect who I am, then something about writing should appear in it.

Writing was always stressed at Horace Mann, the prep school I attended in New York. The school made students *want* to write, a desire I took with me when I started as a freshman at the University of Michigan. I must confess that I took out to Ann Arbor a suitcase full of books on English literature. And—to continue the confession—I kept that suitcase under my bed and never opened it!

At Michigan I took a couple of writing courses, trying to learn how to write better. But what really taught me good writing came from reading the essays of Leslie White, which had been collected and published together in *The Science of Culture*. There is not one essay among them that I haven't read at least twice. White's writing really stood out. It was clear, crisp, and pungent. *That's* what writing should be, I told myself, and began using it as a model for my own efforts.

White lectured as he wrote, but there was something else about his scholarship that impressed me. When discussing Boas or Lowie or Kroeber he didn't simply *allege* that they had said such-and-such, or even paraphrased their remarks. He quoted their very words. He had compiled an extensive file of quotations on all manner of anthropological topics which he used liberally, citing chapter and verse, so that, should the student wish to explore a statement further, he could easily do so. I quickly saw the value of this and began my own file of quotations, which I keep expanding to this very day. It is an enormous help in writing on any topic that interests me. I don't have to start from scratch; I already have an accumulation of relevant material on hand I can turn to.

With these files as a basis, my writing has the attribute of being peppered with quotations. When you look at a page of my writing you will find it bristling with names, dates, places, etc. Indeed, I regard a printed page with no names or dates or places, etc, as a *literary desert*, arid and barren.

Because of my heavy reliance on quotations—on the accumulation of the words and ideas of others—I have come to regard myself, not so much as a *writer* but as a *stone mason*. I quarry my stones, I dress them a bit, I assemble them in some kind of order, put a little mortar between them, and voilà, a structure emerges.

My aim in writing—at least some types of writing—is to exhibit the words and ideas of those who have written on the subject I'm dealing with, in the process inserting ideas of my own to see if they can contribute to furthering the discussion.

Ruth Bunzel once told me she composed what she was going to write entirely in her head before she put down a single line. Harry Shapiro, my first boss at the American Museum, also was said to make his first draft his last. I don't understand people like that, but I envy them. I don't have that kind of mind. I revise. I have to revise. In fact, for me, writing *is* revising. Two chapters of my book, *The Muse of History and the Science of Culture*, for example, went through nineteen drafts. The average chapter in any one of my books went through between nine and twelve drafts before I considered it done. Or at least as good as I could make it. Revising gives me the satisfaction of seeing a piece of my writing—far from perfect as it may be—assume a little better shape each time around.

This incessant revising has a fringe benefit. I've never experienced "writer's block." How come? Because I never worry about what I put down on paper the first time, no matter how ragged and shapeless it may be. Not to worry, because it will be improved the second time around...and the third...and the fourth...and so on. Thus I take considerable comfort in stories like that told of Anatole France, who was said to spend all morning taking out a comma, and all afternoon putting it back in.