

An Interview with John V. Murra

Introduction by JOHN HOWLAND ROWE*

JOHN Murra is an anthropologist who has devoted much of his professional career to studying the historical ethnology of the Incas and other Andean peoples of Ecuador, Peru, and Bolivia, using historical sources and working by historical methods. In the course of this work, he has edited a number of documents of the greatest importance and interest to historians, done a substantial amount of archival research, and worked harder than anyone else to bring historians and anthropologists together on three continents and to keep them talking to one another. What has sparked his archival research is his conviction that in order to learn more about Andean historical ethnology, we need to turn up new sources. He has always had a special interest in economic problems. What Murra would like to do is find out how the Inca state, or another Andean polity, worked in the years immediately preceding 1532, particularly in the economic realm. He has depended on historical documents for this endeavor. I have referred to what he does as "historical ethnology." His "vertical archipelago" hypothesis has stimulated research and discussion among scholars interested in the Andes from Ecuador to Bolivia.

In 1963 Murra received a major grant from the National Science Foundation for a three-year project to provide anthropological context for the 1562 *visita* of Huánuco. The idea was to recruit a team of archaeologists, ethnographers, an ethnobotanist, and other specialists to do research in the area covered by the *visita*. As Murra said to me, previous area study projects had developed out of the interest of an archaeologist or an ethnographer in a particular area, and then a search was made for historical documents relating to the area. Because of the spotty preservation of historical documents, it usually turned out that there were few documents available for the area chosen. The Huánuco *visita* provided an opportunity, possibly unique in Peru, to organize an area research project in which the historical documentation, or at any rate the part of it that

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Murra was most interested in, had already been found. For historians, the most important part of the project was the publication of a new and accessible edition of the *visita*, and the accumulation of context information for it.

After the conclusion of the Huánuco project, in 1966–67, Murra was a postdoctoral Associate of the National Academy of Sciences, working at the Smithsonian Institution. In 1968 he was appointed Professor of Anthropology at Cornell University, a post he occupied until his retirement in 1982. Always restless and seeking new personal contacts and intellectual stimulation, Murra served as a Visiting Professor at Yale in 1970–71; in 1974–75 he had a research appointment at the Institute for Advanced Study at Princeton; in 1975–76 he taught in France, at Nanterre and Paris; in the spring of 1977 he taught at the Instituto Nacional de Antropología e Historia in Mexico; in 1978–79 he spent a sabbatical leave at Seville, working at the Archivo de Indias; in the spring of 1980 he did full-time research in Lima; and in the spring of 1981 he was Visiting Professor at Johns Hopkins.

Retirement has not led to any lessening of Murra's scholarly activities. In 1982–83, he was consultant to the Banco Nacional de Bolivia, assigned full-time to the Museo Nacional de Etnografía, La Paz; and in 1983–84 he was a Guggenheim Fellow.

Named Lewis Henry Morgan Lecturer at the University of Rochester in 1969, Murra has served as President of the American Society of Ethnohistory (1970–71), President of the American Ethnological Society (1972–73), and President of the Institute of Andean Research, New York (from 1977 to 1983). He currently serves on the editorial boards of *Histórica*, Lima; *Historia Boliviana*, Cochabamba; and *Chungara*, Arica; and the *HAHR*.

Murra excels at intellectual discussion and for years has been much in demand for such affairs in Latin America, in Europe, and in the United States. Thus he has maintained a broad network of contacts in Andean studies.

Background

In Rumania, where I was born, among my peers, literary vocations rated high; we read in at least two languages, Rumanian and French. At 17 I was translating John Dos Passos's *U.S.A.* trilogy, at a time when it had not occurred to me that I would ever visit this country. At 15, I had begun steady work as a stringer for the sports pages of a daily newspaper. By the time I finished my secondary education, I had a byline and marginal pay. Becoming an adult sportswriter was plausible to me but unacceptable to

my elders. Had the opportunity not arisen to attend the University of Chicago (about which more later), I might well have compromised, as did many of my classmates, by attending law school, which in prewar Rumania, as in Spanish America today, one entered directly from high school.

In the end I never studied in a Rumanian university. In my last year at the lycée, I was expelled for belonging to the Social Democratic youth, a legal organization; eventually I took my federal *baccalauréat* examinations as a privately prepared student. Meanwhile, first in Rumania and later in Croatia, my father found me apprentice jobs in paper factories. Raised in an orphan home, he had gone to work at age 12; his durable, if never realized, fantasy was to be the first cigarette-paper manufacturer in the country. I was meant to be his educated technician.

Work in the paper factories was a significant educational experience; I like to think that it prepared me for ethnographic fieldwork. I met the first generation of Balkan industrial workers: most of them, mountain-dwelling peasants drawn away from their farms. Those in Croatia worked a twelve-hour shift, then took twenty-four hours off, which allowed them alternate daylight to keep up with their farms. In both countries, I was routinely asked by my co-workers to their homes—talk was of crops, harvest ceremonies, the land reform of 1918. But they also knew of trade unions, which were legal in Rumania but clandestine in Croatia. Rumanian millhands were all men; in Croatia, which I perceived as “European,” women also worked at the plant.

Noting such differences seemed as natural as the continuous awareness of ethnicity: Serbs may have spoken the same language as Croats, but my co-workers constantly dwelt on the differences. The ethnic gulf between them was so great that I cannot think of a context in which it was ignored. But then, I had been prepared for drawing such distinctions since childhood. Only gypsy women sold corn on the cob and no one else carried burdens on their heads; yoghurt arrived at the house every afternoon and only Bulgarians delivered it; sweets were baked either by Saxons or Greeks. Hungarians my age frequently spoke Rumanian, but no Rumanian I had met would admit knowing Hungarian, even though three million native speakers of that language lived among us. At 18, I had no idea that drawing such distinctions could be a trade, that one made a living contemplating ethnic diversity.

Several short stays in jail during 1933 and 1934 were also educational: prisoners segregated themselves not only by ethnic group but also by political creed. Decades later, José María Arguedas's jailhouse novel, *El Sexto*, seemed touchingly familiar. Hitler's coming to power encouraged the native Iron Guard to demand ethnic purity—one heard many ver-

sions of what this implied in so multiethnic a country. Once I spent a month in a provincial jail, the only "red" among twenty-five or so Iron Guardists who had just assassinated the prime minister. I escaped some of the beatings coming my way when my connoisseurship of soccer players and tactics became evident.

Unexpectedly, and thanks to no efforts of mine, all this ended in December 1934, when I went to Chicago to attend Robert Maynard Hutchins's university. My father's brother, a virtuoso of the double bass, had come to the United States a decade earlier, playing in a gypsy band, and had ended up with the Chicago Symphony. Reading in the *Chicago Tribune* how radical a place the university had become under Mr. Hutchins, my uncle decided it was just the place for a restless youth. In 1934 I would have gone anywhere, given a chance, since there was no particular place I wanted to be. All I knew was that studying the chemistry of paper at Grenoble, my father's preference, was not a genuine alternative. So Chicago it was.

One activity I had never contemplated before 1935 was academic research: before coming to the University of Chicago I had never known anyone engaged in such work. I must have known that the endeavor existed in the adult world nearby, but in Rumania, the only country I knew, an academic career had not loomed as an option.

I drifted to the social sciences, whose very existence beyond Marxism had been vague in my mind; both anthropology and history were required of juniors—my status in college, given my French-style *baccalauréat*. I thought the required courses were slow and dull; my grade average in college was systematically a low C. But the worldwide, comparative scope of anthropology was a revelation; as taught by Dr. Fay-Cooper Cole, a specialist in Philippine ethnology, it never lost a historical dimension. Cole had studied with Franz Boas and had created an anthropology department at Chicago, bringing to it such diverse stars as Edward Sapir from Ottawa in linguistics, and A. R. Radcliffe-Brown from Australia, in social anthropology. I admired the man and the organizer in Dr. Cole; through him I met the kind of Middle Western American whose integrity and intellectual fairmindedness were without antecedent in my experience. Another man who, in retrospect, played a decisive role in my becoming a professional anthropologist was the Florida archaeologist Charles H. Fairbanks, who later organized the field in his home state.

Much of their influence, however, remained subliminal, even after I graduated in June 1936, just short of my twentieth birthday. That year, nothing in academic life compared with the urgencies of politics. That autumn, when recruiting began on the nation's campuses for an international brigade in the Spanish war, I was ready to join up. And I did just

that, which is how I learned Spanish and eventually became a student of the Andean world.

By the time I managed to return to the University of Chicago, almost three years later, my interest in politics was waning. Few experiences will do as well as participating in a modern civil war to explore the realities of "democratic" centralism or the strength of national and ethnic ties over class ascription. As a polyglot, if subaltern, member of the International Brigade staff at Albacete, I witnessed how decisions affecting thousands were made by non-Spaniards who had brought their rank and authority from outside the republic, from their respective central committees. If at the front military leaders were frequently promoted in the field, and some were superb commanders, their political counterparts were overwhelmingly hacks. Of the British, Canadian, and United States commissars I served during the first year of the war, only one stands in memory as able to carry out his specialized functions: the Pennsylvania Croat, Steve Nelson, whose much too pale and defensive, oral history-style autobiography was brought out a few years ago. He deserves better.

The active residue of two years of staff and front line experience was an appreciation, shared by few academics, of the talents involved in military skill, and an enduring admiration for the Spanish people; had the republic won, I doubt that I would have returned to the United States.

By November 1938, the bulk of foreigners on our side who had come from democratic countries were repatriated; the majority of the volunteers, however, were subjects of dictatorial regimes in Eastern Europe, the Balkans, Italy, and Germany. We were not admitted into France until February of 1939 and were then interned behind barbed wire on the beaches east of Perpignan.

After some six months in several camps, I managed to return to Chicago. Anthropology now emerged as more than an avocation; it had a new sheen, particularly as Dr. Cole had welcomed me back. It now seemed to deal with major human alternatives, unfathomed in my earlier activism. Periodically since, over 45 years, I have rediscovered this relevance and it is a magic and exhilarating experience.

*How did your interest in ethnohistory begin?*²

We were not aware when I was a graduate student at Chicago of ethnohistory as a formal subdivision of anthropology, though many in the United States practiced it. In the 1930s Fay-Cooper Cole had initiated a study of native Illinois and one could earn an unheard of \$1.00 an hour if one were able to read and evaluate French accounts of the state's aborigines. Although some of my teachers at Chicago thought a concern with

the history of preliterate populations was a doomed exercise, aimed, usually, at validating much later priorities or ethnic interests, this did not deter Fay-Cooper Cole, Fred Eggan, another one of my teachers, or me.

As we saw our task in the early 1940s, it was essentially a historical one, though practiced independently of existing history departments. We welcomed information about the Otoe, the Illinois, or the Shawnee, and beyond our area about all preliterate ethnic groups the world over. For the state of Illinois, this information could be acquired through excavation or the scrutiny of the *Jesuit Relations*.¹ Earlier, when Cole had studied the ethnology of the Philippines, he drew upon archaeology, early eyewitness Spanish accounts, or the comparison of ethnic groups still living in the highlands of that archipelago. In the United States, anthropology grew out of an interest in peoples like the Illinois or the Igorot, not from commitment to a particular scholarly tactic.

In 1943, Wendell C. Bennett, then at Yale, commissioned two articles on the indigenous groups of Ecuador for the *Handbook of South American Indians*. Donald Collier prepared the one dealing with the archaeology of the Northern Andes; he had directed a study in the region the previous year. I was asked to cover the early eyewitness European accounts dealing with the same peripheral part of the Andes. It was my first venture into Andean ethnohistory—at a time when I had only a minimal acquaintance with the sources. Eventually Bennett became my other, if extracurricular, teacher—one who shared my interests in the Andes and perceived somehow that I would enjoy puzzling out the writing of sixteenth-century observers. Like Cole, whose pupil he had been ten years earlier, Bennett welcomed information about his region, be it archaeological, historical, or contemporary ethnography.

At the Library of Congress, where I read these sources, I was lucky to meet José Antonio Arze, the Bolivian scholar, and Alfred Métraux, the instigator behind the *Handbook*, and in many ways its senior editor. The three of us pondered Inca social and economic organization. We would debate the “mode of production” we thought had prevailed in Cusco before 1532, even while we agreed that the sources available were too limited for a confident verdict. None of us was an “ethnohistorian”; trained in very different countries, what brought us together was the dependence on sixteenth-century eyewitness and administrative accounts. Eventually I decided, erroneously as it turned out, that the Inca state was a feudal one, a notion it took me until 1955 to withdraw formally.

1. Reuben G. Thwaites, ed., *The Jesuit Relations and Allied Documents: Travel and Explorations of the Jesuit Missionaries in New France, 1610–1791*, 73 vols. (Cleveland, 1896–1901).

Could you give us some idea as to the extent of your travels abroad? In the field of ethnohistory have there been any foreign contacts of particular importance to you?

Not counting as "travel" my migration to the United States and soldiering in Spain, the decisive trip was going to Ecuador in 1941, as an assistant to Donald Collier, of the Field Museum. The purpose was an archaeological study; Collier had done research in the Andes before; his aim was to explore the northern reaches of the Chavín Early Horizon (some centuries B.C.).

This original aim had to be abandoned when the Peruvian army invaded southern Ecuador that year, but we readjusted quickly: there was so much to be done and the sponsoring body, the Institute of Andean Research, acquiesced. Looking for alleged Maya influences in the Northern Andes, I learned to ride horseback and to doubt my vocation as an archaeologist; I also discovered that familiarity with Balkan social structure was useful in Spanish America. The most significant consequence of Ecuadorean fieldwork was my discovery of Andean civilization as a major human accomplishment and that I would like not only to study it but also to be its advocate.

Soon after I returned from Cañar and Quito, I was asked to teach, replacing people who had gone to war (the wounds from Spain had kept me out of World War II). I enjoyed the sudden responsibility, particularly as Dr. Cole (and Chicago) gave me ample leeway as to how I did it. When the war ended, the SSRC awarded me a fellowship to return to the Andes, this time as an ethnologist, inquiring into the "anomalous" economic success story of the Andean peasants in the seven hamlets surrounding the city of Otavalo.

When ready to leave, I discovered that the United States government would not let me travel, nor would the Justice Department tolerate my naturalization. The Spanish war and earlier associations flagged my file. My petition was repeatedly rejected; even when the federal Circuit Court ordered my naturalization in 1950, the State Department withheld my passport until 1956. So my return to the Andes was delayed and some good fieldwork years were lost.

This led, however, to an interlude in the Caribbean: some years teaching at the University of Puerto Rico when that institution gained the right to offer its courses in Spanish, plus several summer field seasons elsewhere in the Caribbean, especially Jamaica and Martinique. All of this was fun and also instructive, but I knew all along that I belonged in the Andes; in 1958, I began work in the archives of Cusco, while enjoying a sabbatical year from Vassar College, where I had received an appoint-

ment in 1950. For the last twenty-five years I have pursued Andean evidence in the archives of Sucre, Seville, Buenos Aires, Lima, and Madrid. I plan to continue.

It was during 1958–59 that I met my first Andean colleagues since J. A. Arze. Don Luis Valcárcel, whose *Memorias* appeared recently in Lima, was the first to use “ethnohistory” to describe the study of sixteenth-century eyewitness accounts of the European invasion. He discovered these sources in the first decade of this century, at the University of Cusco, and has spent a lifetime studying them and producing improved editions of some accounts. Another was doña María Rostworowski, the most imaginative Andean scholar in the use of ethnohistorical records: even her earliest work is full of insights that remain insufficiently explored. In 1960, I tried to attract United States scholarly attention to her work by translating and publishing an article with her explanation of royal incest among the Inca in the *Southwestern Journal of Anthropology*; it had no echo in this country. She has since concentrated on the history of Andean coastal ethnic groups and has done it extremely well. Finally, I would like to recall the work of Emilio Choy, whose familiarity with the sources and the relentless questioning to which he submitted them was a revelation. We may have disagreed about the interpretation of the sources (Choy was convinced that the mode of production of Tawantinsuyu was a slave-holding one), but his knowledge of the empirical data was first class.

Of the various places you have visited, which made the biggest aesthetic impression on you?

Undoubtedly, the Andean massif and the *puna*, in all their majesty. I have been crossing and recrossing them for forty years, but they have never lost their power to awe me.

I wonder, though, if this awe is an “aesthetic impression.” As I gaze at the Andean landscape, the challenge it posed for Andean man and his ability to achieve a dense population and high productivity in such extreme physical circumstances are always with me. So it may be that “aesthetic” is the wrong category, and then I have no answer to this question.

Have there been any particular writers—philosophical, historico-philosophical, or even creative writers—who have influenced you in an important way?

In youth and through the 1940s, Marxism was the methodology that helped me most. Hence the concern with divining the right “mode of production” to describe the Inca, which stoked the debate between Arze, Métraux, and myself in 1943. Ten years later I formally withdrew the

“feudal” characterization I had recorded in the *Handbook*—not because I had additional data that encouraged me to change my mind, but because the very question, the strong-arming of preindustrial societies the world over into Friedrich Engels’s limited array of human possibilities, had become unproductive.

The route I took involved two separate paths: my participation in the Spanish war had distanced me from Stalinist orthodoxy; more important, I discovered the great monographs of British social anthropologists. Here were field studies of African kingdoms, invaded and overthrown very late in the nineteenth century so that oral traditions and ideologies of the pre-European past were still available as a living, motivating force. The work of E. E. Evans-Prichard on the Azande, of Rattray, Danquah, and Fortes on the Ashanti, of Max Gluckman on the Barotse and Zulu was a revelation.

No one had taught this material at Chicago since Radcliffe-Brown left in 1937, so in 1944 I suggested to Dr. Cole that we might offer such a course. I had never been to Africa but I was convinced that African studies would grow in importance to the people concerned and also for comparative purposes, once the war was over. If one wanted to understand precapitalist, preliterate polities in all parts of the world, which motivated my study of the Inca state, one had to ask new questions and take account of the new data from outside the American continent.

I have never worked as hard as I did in preparing my first “African ethnology” course and few other efforts have given me as much satisfaction. I have taught it since at the Universidad de San Marcos, in Lima, at the Universidad de Puerto Rico, and at the Université de Paris-X (Nanterre), but also at Vassar College, at Columbia University, and the New School for Social Research. I did not become an Africanist, in the professional sense, but I have kept up with the *Cahiers d’Etudes Africaines* and the *Journal of African History*, with the work of Jan Vansina and Kwame Arhin, Paul or Laura Bohannon, Françoise Héritier and Ivor Wilks.

In some anthropological circles, one hears frequently the argument that the British social anthropologists had no historical concerns, coming as they did from a confessed ahistorical perspective. I thought this was irrelevant, given the results, and in the case of Evans-Prichard’s Azande studies or Gluckman on the Zulu, even untrue. What Bronislaw Malinowski and Raymond Firth had achieved before 1940 was a new standard for ethnographic fieldwork: familiarity with the vernacular languages and repeated contact with the people studied.

Given the recency of the European invasion, in the 1930s one could still find many who remembered their first European; colonial policies had affected but not obliterated the struggles for royal succession or the

multiple accesses to land or people; anthropologists reported on such matters even if they professed to be uninterested in "history." In the works of Melville J. Herskovits, Audrey Richards, Sigmund Nadel, or Isaac Schapera, I thought I discerned materials that would help my search for historical regularities. The changes since the invasion had indeed been profound, but they were frequently handled by local people in local contexts, particularly in the vast parts of Africa where there had never been any permanent European settlement.

One thing I noticed at the time, which I also consider pertinent, is that the generation of anthropologists trained or influenced by Malinowski at London did not include a single English male—they were New Zealanders, South Africans, Central Europeans, even North Americans. A goodly percentage were women and Jews. What they achieved set a new and very high standard for fieldwork that was to become the model for the rest of the world.

I was prepared then, and still am, to go along for a generation or so of ahistorical postures in anthropology if the result is based on painstaking fieldwork conducted in the local language. And by 1960, even British social anthropologists had decided that their procedures were compatible with diachronic concerns. In 1960, also, African historians formally rejected "ethnohistory" as a label for their endeavors.

A further, comparable (if separate) strand, was the work of M. I. Finley or Jean-Paul Vernant, studying the history and anthropology of the classical world. The debates they conducted in the 1960s paralleled our concerns over Aztec and Inca "modes of production"; eventually, they, too, went beyond sectarian concerns. A good example of the kind of "ethnohistory" of the slaveholding societies in the early Mediterranean that I find helpful in the Andes is the recent festschrift for Sir Moses Finley, *Trade in the Ancient Economy*.

It is within these perimeters that I would locate my theoretical orientation to the Andean data. The historical materialism that nurtured me has not vanished; it has been internalized and can be seen in the kinds of priorities I assign to the study of rights in land, macroeconomic complementarities and mechanisms of exchange, to the relations between state and ethnic polities. Critics have noted that I make no contribution to Andean religion, symbolism, or to wider structuralist concerns, and I plead guilty.

As I reread this, I suddenly wonder if I have dealt at all with the question. I should have started, perhaps, by stating that I do not read philosophers. The temperaments closest to the form of inquiry that I find congenial and can stand rereading have been Montaigne and Rousseau, Marc Bloch and Raymond Aron, Harry Stack Sullivan and Frieda Fromm-

Reichmann. The work of fiction that I thought "mine" from about 1935 to 1955 was *La Condition Humaine* of André Malraux. None has replaced it since.

Your considerable reputation as an ethnohistorian is based to a large extent (though certainly not exclusively) on your studies of the colonial period. How do you yourself see the long-term consequences of the colonial experience in Latin America? How far has the colonial legacy been reflected in the national and republican eras?

I think the question gives me too much credit. It assumes that my version of "ethnohistory" includes the colonial period. It does so only indirectly, insofar as the absence of Andean writing forces me to rely almost exclusively on colonial sources, the eyewitness accounts of European observers of the Andean world.

Were archaeology in the Andes to enjoy the national priorities it deserves, had we more sources by Andean writers like Waman Puma,² had Ludovico Bertonio left us the ethnographic description implicit in his dictionary,³ we would not be as dependent on colonial records. But since the sources in Aymara and Quechua are so few (compared even to those in Nahuatl), one is forced to rely on the observations of Europeans. Here I should note that most ethnohistorians active in the Andes do not have at their disposal an adequate record of the relevant Spanish institutions and of the colonial framework erected in the Americas. I think of Silvio Zavala's studies of the *encomiendas* or of mining, James Lockhart's analysis of the men of Cajamarca, or Josep M. Barnadas's account of the early years in Charcas, which make a beginning but deal with Andean politics only tangentially. For example, we do not have a good study of the *reducciones*, the protocols of those who conducted them, or the resistance to this successful imposition of "strategic villages" upon the Andean landscape; Alejandro Málaga Medina has made a good start⁴ but much more work, in collaboration with anthropologists, is needed to rescue the ethnographic information inevitably contained in such protocols.

The long-term consequences of the colonial experience in the Americas were not only destructive but, in N. Wachtel's term "destructurating." The total physical annihilation by 1560 of the natives of the heavily populated and highly civilized Andean coast is but one dimension of this experience.

2. Felipe Waman Puma de Ayala, *Nueva coronica y buen gobierno* (1615).

3. Ludovico Bertonio, *Vocabulario de la lengua aymara* (1612).

4. Alejandro Málaga Medina, "Las reducciones en el Perú (1532-1600)," *Historia y Cultura* (Lima), 8 (1974), 141-172.

In the altiplano, where a small percentage survived, protected by the altitude, one notes nevertheless the early disappearance of the macro-organization: the network of some 25,000 kilometers of highways, their construction and maintenance; the managerial skills reflected physically in the many thousands of giant warehouses with stores provided for the bureaucracy and the armies; the ideologic and administrative devices allowing so multiethnic a state to endure: all this is gone and cannot be redeemed for world history without a major, conscious, and practical investment by the Andean republics.

In this context, it is impressive to note how many continuities at the local level are still functioning: recent studies have documented the vitality of the Andean heritage in agriculture and animal husbandry, in religion and cosmology, in the perception of the natural and manmade landscapes, in the iconography of the major Andean art, textiles. I am not indifferent to the study of the European heritage but am more committed to the utilization of the Andean legacy, knowing full well that they are frequently inseparable.

If you were to single out one of your books as your most important contribution to the field, which would it be and why do you rank it above the others?

I have produced two kinds of work. First, there are interpretative analyses of the Andean accomplishment, among which I find *Formaciones económicas y políticas del mundo andino* satisfactory since it is more up to date, both as to data used and my own ability to fathom the Andean world.

Within that collection I prefer "El 'control vertical' de un máximo de pisos ecológicos en las sociedades andinas," because it approaches an explanation of Andean success, in circumstances where European historiography and stage-building are rather helpless. Of all my work, this is the essay that has provoked most debate in the Andean countries; "ecological complementarity" may have implications for practical policymaking. Italian and Rumanian versions of this essay have been published, but the English translation has not found a home.

Second, I have also stressed the need for new sources and better editions of the old ones. Beginning in 1964, I have published several collections of administrative, litigation, census, and other "raw" data, with indexes and analytical comments. Among those I have edited, I am fondest of the most recent one: Waman Puma's *Nueva coronica y buen gobierno* [1615], which took more than twenty years to get ready since it was so difficult to find the right translator for the Quechua texts, Dr. Jorge L. Urioste. Rolena Adorno was co-editor.

What projects are you currently pursuing?

During 1983–85 I hope to complete the following: an updating, with new data and commentaries, of the “ecological complementarity” essay mentioned before, elaborating dimensions left unstated in 1972, to explain Andean dense populations and high productivity. It should make a small book, part of the Colección Mínima of the Instituto de Estudios Peruanos, which is celebrating its twentieth anniversary.

A book-length study, in English, and addressed beyond the narrow specialist reader, of Andean ethnic polities and their relations to Tawantinsuyu, the Inca state.

Further examples of better editions of old sources for the history of Qollasuyu, which later became the Audiencia de Charcas. One will offer the text, with indexes and commentary, of a house-to-house inspection of a state coca leaf installation in the lowlands, at Sonqo, and is to be published by the Museo Nacional de Etnografía, La Paz. The other (in collaboration with Gunnar Mendoza, Tristan Platt, and Thierry Saignes) will be a selection of litigation, census, and other descriptive materials about Aymara polities of what became the Norte de Potosí.

Once these are ready, I aim to start on a new project: the pursuit in the Spanish archives of two men who had an excellent understanding of Andean society: Domingo de Santo Tomás, the Dominican bishop of Charcas and author of the first grammar and dictionary of Quechua, and Juan Polo de Ondegardo, his chief public adversary, the attorney for Carlos V and Felipe II. I imagine that this pursuit will finally transform me into a historian.

What do you consider to have been your major influence on your students—undergraduates, graduates? Do you feel that you have had notably greater success with one group than with the other?

Between 1944 and 1963, I taught mostly undergraduates, who usually were not anthropology majors. At the Universities of Chicago, Puerto Rico, Yale, and Vassar College, I stressed ethnographic detail and human diversity but also the relevance of this variability for the understanding of the indigenous past, before the European invasion. Alfred L. Kroeber (particularly his *Handbook of California Indians*) and Paul Radin were my elders frequently referred to in this endeavor. I had discovered their work on my own—it was not stressed by my teachers in Chicago.

I made a good undergraduate teacher. My social science colleagues frequently felt that I exaggerated the potential range of cultural variability and I admit that I find the diversity of human solutions more intriguing than the narrow classificatory range perceived by others. When a recent

reviewer, a former student, called me in exasperation "almost Boasian," it did not register as the put-down it was intended to be.

The people and regions I stressed in my teaching were Native North America, the Andes, and Africa. Some of the undergraduates at the four institutions went on to professional careers; at times, I thought I could discern how they elaborated my approach in their own work, but there is no identifiable cluster of investigators from those first twenty years. What I offered was anthropology as a commitment and a pedagogical gambit more than as a theoretical position.

Among the few opportunities I had to teach graduates were the African ethnology courses at Chicago, where the presence of colleagues like Mark Hanna Watkins or St. Clair Drake, made an important difference in the classroom. My real debut in teaching professionals came when I returned to the Andes. At San Marcos, in Lima (1958–59 and 1965–66), and at the Universidad de Chile, in Santiago (1965), everyone was professionally oriented and there was less interest in my kind of comparative ethnology. While I deplored narrow professionalism, the new obligations pressed upon me reconsiderations of how I approached anthropology. The utter absence of translations into Spanish of the great monographs of British ethnology made my usual methods of teaching inoperative and put a different stress on the kind of lecturing one could offer. Some of the people I taught then are collaborators and friends still today, but our shared understandings are quite different from those I have with United States anthropologists.

From 1968 to 1982, I taught at Cornell University; from 1974 on, I did this only during the autumn semester. Most of my students now were postgraduates and I have been aware of a more limited effectiveness: as A. L. Kroeber noted, no anthropologist should be asked to teach after 50. I was not only aging but also irritated with post-Sputnik changes in United States anthropology: the tenfold increase in our numbers, the lack of interest in Native American civilizations, the rejection of the concept of culture. It made me less attuned to student needs and obsessed with trends in Andean research. Some excellent theses on the Andes were produced at Cornell during this period; I am proud of the fact that virtually all were published in the Andean republics. Still, the fact is that during this period I directed at least as many theses at other universities in the United States and in Europe as I did in Ithaca. Early in my tenure here, I suggested to my colleagues that any anthropology department should assume a responsibility for the training of qualified researchers from the regions of the world where they did their fieldwork. This notion seems more acceptable today than it was fifteen years ago.

The one new, and I hope successful, teaching task I undertook at Cor-

nell was to offer a history of United States anthropology, viewed as an institution and a craft, not the usual survey of ethnological theory. In this country, our discipline has developed along paths unmatched elsewhere and since the vast majority of all anthropologists in the world today are North Americans, they exercise influences and meet resistances that deserve cross-cultural attention. So far, I have been unable to offer this course in the Andean region but I have tried it out in France.

Would you care to reveal to us your philosophy in selecting, training, and placing your graduate students?

I would distinguish between those from the Andean republics and those from the United States and Western Europe. The latter usually selected me. Early in my tenure at Cornell, I suggested the admission of two United States candidates, long associated with my research and publication efforts in the Andes. They were turned down since the department "did not welcome shops." It is true that they were neither very young, nor did their graduate scores reach into the 800s, but each had years of field and archival experience. I had been confident they could benefit from graduate training and, in turn, would contribute unique dimensions to Andean tasks.

So I have been content with the occasional United States or Canadian graduate student who picked me and could also satisfy the computer's criteria for admission. Some are now creative professionals. Only two of them have come from the Andes and both were upper-class, well-connected, and able to find non-Cornellian sources of funding. They were also unable to speak the Andean languages; in fact, they found it more difficult to learn them than did United States graduate students. I mentioned above the theses on Andean topics written elsewhere in the United States and Western Europe, especially France.

The greatest disappointments came when excellent candidates from the Andes could not meet the tuition and admission requirements in this country. While working in the Andes, I have frequently met young people who needed and could use the concentration afforded by graduate school. The first Andean candidate we brought to the United States came to the University of Chicago in 1943; it was much harder to achieve it then, but the process continues to be difficult.

In 1966 a group of us tried to establish a graduate school at the Universidad de La Plata, to train anthropologists from all five of the Andean republics. In 1972, Angel Palerm was able to bring a group of young scholars from four countries to a comparative seminar in Mexico; in 1973, Luis G. Lumbreras and I assembled most of them again for a field seminar

around Lake Titicaca; in 1977, the Fulbright Commission helped us bring a group of younger people from these republics to Ithaca for an *otoño andino*.

Usually such efforts were viewed as a personal hobby; only when the Camelot scandal loomed threateningly, did the American Anthropological Association set up a committee on relations with our colleagues in the hemisphere, with Gonzalo Aguirre Beltrán and myself as co-chairmen. Our training and publications' recommendations (see *Anuario Indigenista*, 1967) were ignored.

Since you ask about methods of selection, I will reach as far back as the Spanish war, when I worked for the cadres commission. I discovered that I had accurate intuitions about who would benefit from officers candidate school. Pure serendipity. I have no idea what criteria I used: the more mysterious since I was so scared at the front that I could lead no one. I have grown the same confidence about predicting success in the training of Andean scholars. An executive of the Ford Foundation in Lima inquired once about how he could objectify my criteria—after all, the foundation did not want to rely on my judgment alone. I could not enlighten him.

As to placement: in better times, one could plot, choose, and help people toward museum, archival, or teaching posts. Those times coincided with my familiarity with the social structure of United States anthropology. I learned most of it by watching two masters: Fay-Cooper Cole and Wendell C. Bennett. Twenty years ago, I knew personally the vast majority of practicing anthropologists, enjoyed the trust of many, and could foresee, at times, the needs of their institutions, which is, after all, the secret of good placement.

In recent years, most of the above no longer applies. Much effort has to be invested in securing any kind of job. In the last five years, I can think of only one candidate who, miraculously, was placed in the perfect job for him with my help; he had waited for it for several most marginal years at a noted state university.

What do you consider to have been your greatest single satisfaction as an anthropologist-ethnohistorian?

The greatest personal satisfaction was the almost accidental discovery of the Andean world—before the fieldwork there I had heard of it the way an anthropologist “knows” about Tibet or Lapland, from the clichés one picks up in graduate school.

Once I had done the fieldwork and become acquainted with the sixteenth-century literature, there were direct, intimate responses, the shock of discovery, and recognition. While such personal, emotional com-

mitment may affect one's work adversely, in my case I think it helped a scholarly pursuit, particularly during the "dry" years (1947–56) when I was prevented from returning to the Andes. It also helps that I think of my work as involving not only recovering the past, but also as relevant to the future of the Andean population.

Latin American ethnohistory has come a long way since you entered the field. Would you care to single out one, two, or three of your contemporaries who you feel made particularly important contributions to the field?

I can think of two dimensions where the work of others has inspired and taught me. First, there are those whose familiarity with and skill in handling sixteenth-century sources I admired. Since those of us who came to Andean history from anthropology frequently had no serious training in historiography, dexterity and elegance in using such sources is not to be taken for granted. Gunnar Mendoza, John H. Rowe, María Rostworowski, and Nathan Wachtel are colleagues from two generations and four countries whose work and insight I use consistently.

Second, there are those who may well be unconcerned with adding to the corpus of Andean sources, but who offer us analyses and interpretations that help us ask better questions. Andean societies have long been the object of "socialist," "feudal," and other European fantasies—as late as the 1940s books appeared in the United States suggesting that Thomas More's *Utopia* was written from eyewitness accounts. Going beyond Eurocentric interpretations while meeting American, African, and Pacific civilizations head-on has been an exhilarating but also very difficult task. Here I would list Angel Palerm and Friedrich Katz for Meso-America, Sidney Mintz for the Caribbean, and M. I. Finley for the classical world.

Do you have any feelings about the directions the field is moving or any suggestions of how you would prefer to see the field develop?

In 1970 I published an inventory article in the *Latin American Research Review* and in the *Revista del Museo Nacional*, Lima, in which I also suggested some of the directions where I thought our efforts could be directed. During the next decade, similar inventories and recommendations were published by Franklin Pease García Yrigoyen and by Frank Salomon. An important new dimension, the roles of women in Inca society, has been added during the 1970s by Irene Silverblatt.

We have had some success in locating new sources, particularly administrative, litigation, and census records: for example, Wachtel's discoveries

in the Cochabamba archives,⁵ Pease's materials from the *Collaguas*,⁶ or Sánchez-Albornoz's and N. David Cook's demographic analyses.⁷ One of my suggestions from 1970, however, the recuperation of missing eyewitness accounts for the sixteenth century, has not received the attention it deserves: for example, the "lost" history of the Inca by Cristóbal de Molina, the second half of the Betanzos interviews in 1548 with the surviving royals in Cusco, or the manuscripts of Diego Alvarez.

The collaboration between ethnohistory and archaeology is developing slowly but is now a reality: thus the study of an Inca administrative center at Huánuco Pampa by Craig Morris⁸ or of the Inca road system by John Hyslop are now at the threshold of publication. Ethnohistorians will someday be retained by archaeologists on a routine basis.

One direction into which ethnohistorical work has flown successfully during the 1970s, which I did not stress in the inventory, has been its articulation with contemporary ethnology. R. T. Zuidema's reading and interpretation of the sources has stimulated work at the University of Illinois on the Cusco region. Ethnoastronomy, irrigation, and community organization have all shown unexpected but suggestive continuities.

What still remains to be done and attracts few contributions is the comparative work, both with Meso-America and with other continents.

What do you believe to have been the role(s) of historical writing in the evolution of Latin America?

I do not feel competent to deal with this question beyond noting the redefinition proposed by Franklin Pease, and accepted in the new master's program of FLACSO, at Quito, who argues for an *Andean history*, which would overarch the indigenous and the colonial-cum-republican dimensions. I think this parallels the rejection of "ethnohistory" by African historians at Dakar in 1960.

Would you care to compare the roles of United States anthropologists and historians and their counterparts in Latin America and Europe?

Anthropology in the United States, much more than history, differs from the European varieties. Our clustering of anthropology to include archae-

5. Nathan Wachtel, "The *mitimas* of the Cochabamba Valley: The Colonization Policy of Huayna Capac," in George A. Collier et al., *The Aztec and the Inca States, 1400-1800: Anthropology and History* (New York, 1982), pp. 199-235.

6. Franklin Pease C. Y. et al., *Collaguas I* (Lima, 1977).

7. Nicolás Sánchez-Albornoz, *Indios y tributos en el Alto Perú* (Lima, 1978).

8. John V. Murra and Craig Morris, "Dynastic Oral Tradition, Administrative Records and Archaeology in the Andes," in special issue on history and archaeology of *World Archaeology* (London), 7:3 (1975), 269-279.

ology, linguistics, human biology, historical ethnology, and the comparative study of civilizations is shared in almost no European country. It was my experience lecturing in France, Britain, or Rumania that sent me to the study of the history of United States anthropology, to which I have referred above. The unifying idea for all those separate tactics was seen clearly in 1846 by Henry Rowe Schoolcraft: anthropologists in the United States were concerned in those days with Native Americans. Everything about their past, biology, languages, beliefs, or political and social organizations mattered to the student of Native Americans. While the United States anthropologists have drifted away from Schoolcraft's priorities, these are still seen as dominant in many parts of the Americas.

Do you have any suggestions as to how we in the United States should go about improving our relations with the social scientists of Latin America?

In 1967, in the wake of Camelot, a conference met at Burg Wartenstein, sponsored by the Ford and Wenner Gren Foundations. It was attended by four United States and four European anthropologists, but the majority were Latin Americans, invited from all countries where there was an active anthropological community.

We focused on the discrepancy between training and research, which seemed the sensitive point at the time. Our colleagues felt that they shouldered most of the training of local investigators in exceptionally difficult circumstances—the inadequacy of libraries, shortages of full-time faculty, instability within the universities and museums. While they frequently were forced to neglect research, this was carried out by foreigners, coming with foreign funds, frequently insulated from local scholars and national priorities. This situation has improved obviously over the last fifteen years—especially Latin American participation in the deliberations and decisions of the SSRC or the Inter-American Foundation; the establishment of Fulbright commissions.

The main grievances vented in 1967, however, remain beyond the national differences in aims and style. Foreign scholars find it difficult to take an interest in local institutions and journals; they do not always serve on local editorial boards or accept appointments at local institutions. Unlike Japanese or German students I have known at the Universidad de San Marcos, ours do not obtain local degrees or publish their monographs in translation. Here also there has been some improvement, but the high visibility of United States anthropologists south of the border provokes questions and sometimes leads to the prohibition of their fieldwork.

In the future, I expect ethnohistory, as an approach if not a label, to play an important role in bringing together historians and anthropologists in both the Andes and Meso-America. As in other formerly colonial areas,

the label may become unacceptable but the inquiry, stressing the diachronic sweep of Native American institutions, seems assured. Such a history cannot proceed without the conceptual tools developed by modern anthropology: lineage and moiety, dual kingship, matriliney, redistribution as an alternative to trade in exchanges, "nesting" in social organization, the role of kinship in early class societies are all here to stay as will be the systematic use of the vernacular languages.

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