

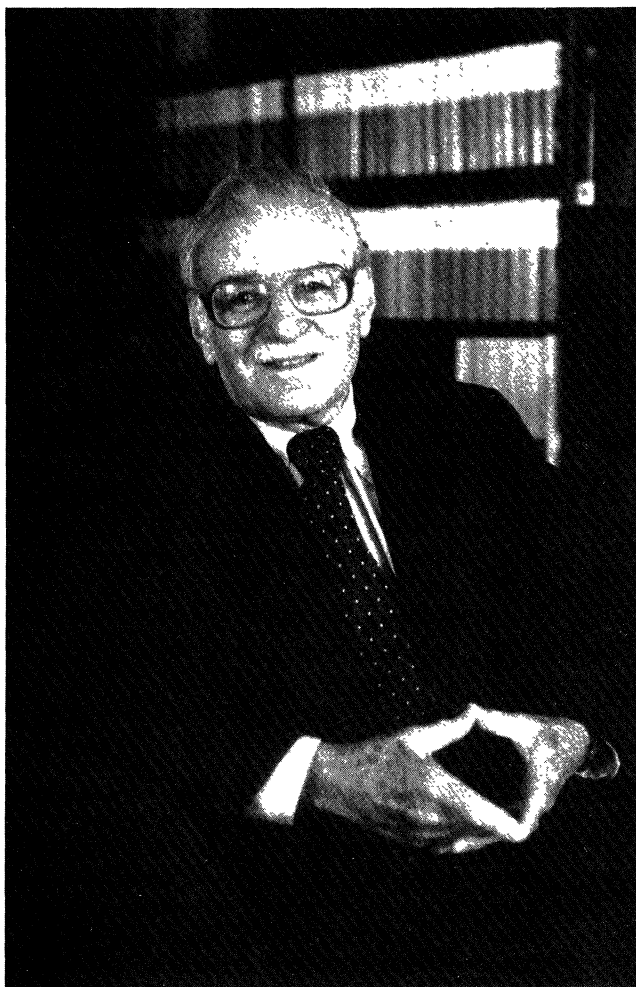
Reports

A Conversation with Sol Tax¹

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Chicago, Ill., U.S.A. IX 86

Introduction [RAR]: Sol Tax, founding editor of *CURRENT ANTHROPOLOGY*, was born in Chicago, Illinois, October 30, 1907, and grew up mainly in Milwaukee, Wisconsin. As an undergraduate at the University of Wisconsin at Madison he studied anthropology with Ralph Linton and for his Ph.B. degree (1931) submitted a thesis entitled "A Re-interpretation of Culture, with an Examination of Animal Behavior." His first intensive ethnographic experience and the beginning of a lifelong association with American Indians was his participation in the Summer Ethnology Program at the Mescalero Indian Reservation, directed by Ruth Benedict, in 1931. He conducted research for his doctoral dissertation (1932-34) among Central Algonquin peoples, focusing on questions concerning the history and meaning of kinship. During this work he developed the egoless kinship chart and the notion that kinship relations were based on accommodation among universal rules and principles present in small societies (1937*a*). (This anticipated the development of the componential analysis of kinship [see Coult 1967].)

After defending his dissertation, Tax was employed as an ethnologist by the Carnegie Institution of Washington under the supervision of Robert Redfield. In October 1934 with his wife Gertrude he began what eventually amounted to seven years of intensive research on the economy, ethnic relations, and world view of the Lake Atitlán area of highland Guatemala (see Tax 1937*b*, 1941, 1953; Rubinstein 1991). Returning to the United States after a further year as visiting professor at the Instituto Nacional de Antropología e Historia in Mexico and joining the faculty at the University of Chicago, he resumed his work with North American Indians. Beginning in 1948 with a project originally intended to serve as an ethnographic field school, he developed an approach that made research and practical assistance equal imperatives in anthropological work. This approach, called action anthropology, incorporated the principles (1) that people should be free to make their own decisions and hence mistakes and (2) that the proper role of the anthropologist is to facilitate communication and decision making rather than direct it (see Tax 1952*a*,



Sol Tax. (Photo courtesy of University of Chicago.)

Gearing 1970, Gearing, Netting, and Peattie 1960, Hinchshaw 1979, Rubinstein 1986). Action anthropology engaged Tax's research and creative efforts among Native Americans and to some extent characterized his approach to other areas of his career.

At the request of the Wenner-Gren Foundation in 1958, Tax designed and then for 15 years edited a journal that he conceived of as a community of scholars with which he could work in the style of action anthropology. Thus he emphasized self-determination and discussion in the development of the journal, eventually introducing the innovative and since-copied "CA☆ treatment," whereby articles, comments on them by qualified colleagues, and authors' replies appear together in a single issue.

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Tax's career has been the subject of a biographical article by Hinshaw (1979), a more recent series of reflections, with bibliography, by Tax himself (1988), and a five-hour videotaped interview that is part of the archival history program of the Wenner-Gren Foundation. The conversation that follows is taken from a series of tape-recorded discussions held between July and September 1986. Manning Nash participated in several of these discussions.

RAR: I'd like to hear more about some of the organizational challenges you've faced—perhaps you could begin with those you encountered when you were organizing and editing *CURRENT ANTHROPOLOGY*—and how your solutions were consistent with your goals as an anthropologist.

ST: I like to think that the method I used came out of the philosophy that I don't do anything unless people want it and will help. I'm reminded of two teen-age peculiarities of mine that may have influenced my life and career. One of these was a wish to rise above my too-ordinary talents. Physically small and shy, I compensated and had some success in helping like-minded peers to do together good things that we couldn't do separately. The other was my conviction that people generally wish for the betterment of mankind, which could also be one of the goals of science. With such an illusion, what else for me but to organize a community of anthropologists?

RAR: So in a sense the journal is a community-development effort, and you start with a "felt need"?

ST: That's right. Any idea that I had myself was not to be taken seriously unless somebody else had the same idea or accepted it quickly. I kept my ears open, obviously—I'd say something and see what response it got. And in many cases things that worked very well turn out, as I trace them back, not to have been my ideas at all. For example, you'll remember that Associates in *CURRENT ANTHROPOLOGY* were a community of scholars, and institutions could be Associates just as individuals could. In a country like the Soviet Union, the category of Institutional Associate was very important, because individuals could only join and be confident about it if their institution was involved. Now, as it turned out, there were relatively few Institutional Associates, and after a year or so I suggested in the Letter to Associates [a regular feature reporting on policy decisions under consideration] that it didn't seem worthwhile to retain the category. I proposed that we abolish it and as usual asked for responses. Almost everybody responded, "Yes, of course, you're right, go ahead," and I was about to do that when I got a single letter, from Czechoslovakia, saying, "If you do this we'll all have to quit." So, whatever the vote was—say, a hundred to one—the one person won, and I explained in a subsequent Letter to Associates that there were some places in which it was necessary to have the institution involved.

RAR: Perhaps I'm reading too much into this little story, but it seems that the moral might be "Pay special attention to the unexpected."

ST: Absolutely. . . . But my approach [action anthropology] was something more complex than responding to felt need—it involved stimulation, providing avenues by which people could act where they couldn't have otherwise.

MN: When did you first think of addressing an international community rather than just American anthropologists?

ST: I helped edit the proceedings of the Wenner-Gren Foundation's international conference in 1952 [published as *Anthropology Today* (Kroeber 1952)], and seeing the importance and the pleasure of people from different disciplines and different countries meeting together very much impressed and inspired me. But before that I'd been involved in an international nutrition project in Guatemala that constituted my war work, and I'd had the experience of teaching in Mexico City, where everything I knew was new. There was no such thing as social anthropology there—or anywhere in the United States, either, for that matter; it was an England-to-Chicago invention, so to speak—and social anthropology was what they were dying for, they were so tired of the historical archaeological approach. Social anthropology means that you're dealing with a society, and since the Mexicans had been through a social revolution and those who were refugees from Franco Spain were all revolutionaries, they interpreted social anthropology as, in part, anthropology for use—applied anthropology. I took ten students from there to Chiapas for five weeks, and after we'd been there just a few weeks they asked, "Can't we do something for these poor people?" I told them we had a lot to learn before we could begin to know how to help. The best of them went back to the field and went on to become very well-known anthropologists.

I took a mockup of the cover of a proposed international journal to the IUAES Congress in Philadelphia in 1956, thinking that if I could arouse any interest in it we might get something started. When I wrote the history of *CURRENT ANTHROPOLOGY* [see CA 6:238, 242–69] it occurred to me that though the idea seemed to come out of the conferences two years later it was possible that I had just been kidding myself—that I had had the idea of a journal all along.

RAR: How was it that you were so close to the Wenner-Gren Foundation?

ST: Back at the university from fieldwork in Guatemala and Mexico in 1946, I met Paul Fejos in Washington as a fellow member of the National Research Council's Committee on Latin America. As head of the recently founded Wenner-Gren (then called the Viking Fund), he enthusiastically offered to support a conference of Mid-

dle American scholars who would prepare a status-of-research report in New York to be presented at the 29th International Congress of Americanists there in 1949. All turned out so well that in addition to *Heritage of Conquest* [1952*b*] I was asked to edit the proceedings of the congress, for which I arranged commercial publication of three successful volumes [Tax 1951; 1952*c*, *d*]. All of this was done just before the Wenner-Gren's own two-week international symposium that resulted in *Anthropology Today* and the *Appraisal* volume [Tax et al. 1953], which were also published commercially by the University of Chicago Press. Later I also arranged production of the Wenner-Gren's great volume edited by William L. Thomas, Jr. (then Dr. Fejos's assistant), *Man's Role in Changing the Face of the Earth* [1971]—the result of the first “environmental” conference, which I had the honor to attend. It too was international.

CURRENT ANTHROPOLOGY started out as something else altogether. The Wenner-Gren Foundation had inaugurated a yearbook to follow up on the 1952 symposium, and one volume, an in-house affair [Thomas 1954], had already appeared. When the foundation asked me to take on the task, I said that if my colleagues in the department would tolerate my doing it I would but that I intended to find out what the profession wanted done. It took me about a year to free myself enough to start traveling, even in the United States. I was busy with our new cooperative core curriculum, I was in the midst of developing the international Darwin centennial celebration, I had action anthropology going on with the Fox, and at the same time I was chairman of the department. I knew that to develop this idea I would need help, so I had the whole department meet with me and Paul Fejos in my home one evening to discuss whether I ought to take on this task—which would require a great deal of travel if the proposed encyclopedia was going to be fully international and interdisciplinary. The members of the department said that they would cover my absences.

Then I held a series of conferences with colleagues extending over more than a year to determine what the yearbook should contain. The idea was that over a period of ten years that material would be gathered and we would have a sort of encyclopedia of anthropology. But this required a table of contents of sorts, and most of the discussion was about how to break anthropology into pieces that could be reassembled in encyclopedia form. After going from place to place and finding different views as to how anthropology should be subdivided, I went on to Europe, and there I found considerably different ideas and a lack of interest in many of the things that the Americans had talked about, as you might imagine. Finally I brought the leaders of all the earlier conferences—Raymond Firth was a leading figure—together at Burg Wartenstein, which was just being inaugurated as the Foundation's conference center, to discuss what should be done.

Anthropology was changing and moving—much more complex than we had once thought, and impossible to put into a straitjacket. Something so completely fluid and living couldn't be subdivided any more than a per-

sonality could. We finally decided to stop asking what anthropology was and agree that it was the sum of what people who called themselves anthropologists wrote. This was necessarily a changing thing, and it needed to be handled in a journal rather than a yearbook. A journal wouldn't have to reject any good idea because it wasn't someone's notion of what anthropology was, and a changing anthropology would simply mean a changing journal. Everything would be presented as points of view about problems, with discussion—nothing set down as truth. We wouldn't have to make any decisions at all as to what would be in the journal beyond the decision whether a piece was worth publishing. I wanted CURRENT ANTHROPOLOGY to be a free market; I didn't even want a group of advisory editors. Given good people, everyone was equal, and policy advice would come from the whole community.

RAR: Considering the variety of definitions of anthropology all over the world, how did you decide what was worth publishing, and how did you choose discussants for CA☆ treatment?

ST: For other editors these might seem like difficulties. I myself decided from the beginning that these decisions had to be made by the community as a whole. I told myself, my staff, and my colleagues, “In the Bourse you see a man on a high ladder writing down the prices of the moment. Surely they aren't *his* numbers; they come somehow from the thousands of monetary transactions all over the world. So CURRENT ANTHROPOLOGY represents what anthropologists of all varieties are learning, teaching, writing, and discussing.” Our job was to reflect all this by asking appropriate samples of colleagues.

In those precomputer days, we had only our McBee sort-cards of information supplied by Associates. All decisions came through the operation of that too-clumsy system, which was being tested as we used it. Our prime rule was to “listen seriously” to whatever was sent to us; and of course the section of the journal called “Our Readers Write” appeared on purpose on the very first pages of CA for everybody to see. We saw that to print answers, suggestions, and especially complaints was our most important obligation. This was our town meeting. The Wenner-Gren Foundation, along with everybody else in anthropology, understood from the beginning to the end what we were doing and at some midpoint in my editorship surveyed our Associates independently and found that indeed we were reflecting the community in all its variety. Whether succeeding editors have found ways to do the same—even without “Our Readers Write” so prominently placed or thoroughly carried out—is a question for the changing and growing world community.

In the pre-issue we sent out explaining what the journal was supposed to be, we indicated the kind of freedom of choice that would be involved and the idea of having a community making the decisions—partly in response to specific requests for advice on policy and partly simply through its contributions to the journal's content.

Obviously, the first rule had to be that every letter to the editor be published, so that everyone could feel that if someone was writing to the editor in an attempt to influence policy everyone else would see it. We worked like this, then, from the beginning, and I'm sure it's right to call it another expression of action anthropology—here involving not a small community of Indians but a world community of scholars.

RAR: Do you feel the same way about applying "action anthropology" as a label to the conferences that you put together?

ST: Probably not so much the purely scientific ones—I think of the week-long conference included in the 1959 Darwin centennial celebration [Tax 1960] and the conferences "The Origin of Man" and "Man the Hunter," both in 1965, and others in Washington (and one each in Bucharest and in Cairo) while I directed the Smithsonian's Center for the Study of Man from 1968 through 1979—but certainly "The Draft" in 1966 [Tax 1967] and especially the 18-month series of weekends with neighborhood leaders resulting in the book *The People vs. the System* [Tax 1968]. And there's no doubt about the action orientation of the Ninth International Congress of Anthropological and Ethnological Sciences in downtown Chicago.

Because of CURRENT ANTHROPOLOGY and my many visits to many countries, by 1968 it was no surprise that I was asked to be president of the Union and hold the 1973 congress in the United States. It happened that I spent the 1968–69 academic year in California, and it was in San Diego that I met with Peter de Ridder, the head of Mouton Publishers in the Hague, and arranged for him to publish the books that I expected to come out of the congress. He offered me \$200,000 in advance royalties as I explained my plans to use the congress to make new books that would cover the whole field of anthropology as we were coming to understand that.

To bring the world community of anthropologists to Chicago became one object. To produce important new books in the whole of anthropology became an equally happy prospect. To plan for this became the task of our small staff of CURRENT ANTHROPOLOGY, and it was obvious that we would have to make it a worldwide cooperative effort, with anthropologists within a few hundred miles of Chicago becoming a helpful committee. Margaret Mead managed to stop in Chicago on every occasion she could in her own busy schedule, and we arranged meetings accordingly. We used the advance royalties for three purposes: to reproduce the thousands of papers submitted to the congress and to mail copies to all the relevant people according to the subject matter; to arrange for simultaneous translation in five languages for all the days of the congress—the largest use of the world's best translators attempted up to that time, and something that would have been impossible if the United Nations had had work for them during the week of the congress; and to provide funds where necessary to help about 1,000 scholars to come from all parts of the world. We eventually did it all, and editors of each

of 91 volumes did their work over the next several years. But it was the congress itself that was of course the exciting part. We had arranged for study by all of the ethnic varieties of Chicago, and we also had commissioned a new opera by Gian Carlo Menotti, and we had a special group of high-school youths from all over the world brought together by a selected committee of Chicago high-school juniors, who did it all with only a little help from their teachers—Youth for Mankind, they called this.

A clearer example yet is the American Indian Chicago Conference, which brought together probably 800 Indians from all over the country [see Ablon 1979, Lurie 1961]. The Schwartzhaupt Foundation in New York had helped to support the Fox Project along with various other projects involving North American Indians. One day the head of the foundation came to me and said that it was going to end and that the board thought that they should have a final report on their American Indian projects. They wanted to know whether I would write a report on what the United States should do about the American Indian problem.

I told him that I thought the Indians themselves should do it. If you stop to think of it, the idea that they had never been asked, so to speak, "What would you like to have happen to you?" is kind of a crazy thing. So I said I'd do it, but in another way—I'd try to get the community of North American Indians as a whole to answer the question "What ought to happen to us? What should we be doing in the changing world?" And he agreed and gave me \$10,000 to help me to do it. The University of Chicago had some money from the Ford Foundation to run conferences, and I asked for some of that money to bring the Indians together in some way, on their own terms, to see if they could do some decision making about their future. And the university said yes.

It happened that the National Congress of American Indians was to have its 1960 annual meeting in Denver two or three days later, and when I got to Denver I had two telegrams waiting for me telling me of this funding. I knew the Indians of the National Congress, and they knew me—I had once been their major speaker at a meeting in Oklahoma, and they had published my speech as a fund-raising device. I showed those telegrams worth \$20,000 to friendly officials and suggested that we could have a conference for Indians to try to speak for themselves and see what came out of it.

They first had to go to other leading officials and get their approval, and there was a lot of suspicion, naturally, of everybody. But there were enough friends to be able to say that it might work. So I sat down to lunch with two or three of them and proposed that the conference be run entirely by a steering committee of Indians. The only thing that I wanted to do was be sure that the steering committee was representative. "Tell me who all your enemies are, as well as your friends, and we'll try to make up a list of people who should be invited to be part of the steering committee. I want intelligent, reasonable people, but they should be of different views." They worked on it, and they brought me a list.

Meanwhile, I said, there was one other thing: if they

were going to end up with a program or document of any kind from the conference, they would have to begin with a model. I suggested that they might stay in Denver two or three weeks after our meeting and prepare a mockup of a document that might be a model. They did this and produced a document that (because they didn't give it a name) I called a charter.

My student Bob Rietz knew enough about Indians that he could tell what their reaction would be to every word, and he was afraid of words. He knew that the document somehow had to be innocuous from the Indian point of view. He said that the major problem was going to be that if I sent out this document the one that was going to be produced at the conference would be bound to follow those lines. To avoid this, he took the original copy and marked it all up—put all kinds of comments on it—so that the Indians would know that this wasn't the final document, that they weren't being asked to rubber-stamp a document. Next I had to get lists of all the tribes and peoples, and I went to lots of sources, the Bureau of Indian Affairs and others. I ended up with 4,000 names (besides white friends of Indians, anthropologists, who were added later), and I sent each of them this document with a letter about the conference. For the letterhead I decided to call it the American Indian Charter Convention.

While this was going on, I had appointed a steering committee and invited them to a meeting in Chicago, and at the annual meeting of the American Anthropological Association in Minneapolis I had a special session for anybody who was interested in the plan. I explained that it would work only if we could get some help from anthropologists working with Indian communities all over the country. I asked them to bring these people together, help them to criticize the document—to discuss what they wanted to discuss. Our colleagues cooperated wonderfully. They put their own effort, money, and time into getting Indians together at meetings, trying to explain the proposal, getting their confidence.

Members of the steering committee began traveling around, and I had to give them some money to do that, too. For example, Ben Bearskin, from Chicago, the single urban Indian on the committee, himself spent weeks traveling around Wisconsin and Minnesota talking about the conference. Back home, we were writing progress reports, and this became a great thing. Without Rietz I couldn't have done it decently, because he knew how to write innocuously; he knew what wouldn't work. In these reports we talked about the steering committee—who the members were, what decisions they had made—and about all the regional meetings. And then, of course, there began to be a flow of letters—people complaining about one thing or another or saying what we ought to do. I printed them happily, to show that anything went. These progress reports became a great source of new information.

When the steering committee arrived for its first meeting, Nancy and Bob Rietz and I were the only non-Indians present. Just the Indians sat around the table; we sat in back and said, "Anytime you need anything, here we are." So we listened, and, sure enough, two com-

plaints came: First, "Who made up this name? What is this 'charter'? What is this 'convention'? Are we going to be like the NCAI and have a convention every year?" I was taken aback; the stationery was all printed. They asked, "What should we do?" And I said, "Make up another name." And eventually somebody had the bright idea of calling it the American Indian Chicago Conference—which didn't change the initials.

The other thing that they began to be bothered about was who had appointed them—why them and not others. This was a problem in political science, and they stewed about it. What right did they have to make any decisions at all? For whom, and for what? Finally someone had the idea of appointing others to come to the second meeting so that they at least would know that they had been appointed by other Indians. So we got to a second layer, and the project became legitimized. I learned a little something there. And mind you, all this was their own discussion and their own conclusions. I didn't do anything; I just listened.

All I did in the end was make arrangements—the dormitory space, the meals, and so on. With the idea of subsidizing travel to the conference, I went to Union Station and inquired about holding a meeting of representatives of different railroads to find out how we could get bargain tickets in quantity. It turned out that Union Station in Chicago was the center of the country as far as railroads were concerned, and every railroad in the country had a sales representative there. So, believe it or not, I found myself chairing a meeting, so to speak, of about 40 railroad people who were exceedingly interested in the fact that Indians were coming from all over the country to Chicago, that they ought to come in trains, that there ought to be some way in which they could gather, and what would be the gathering points. When the critical point arrived of choosing one of the three railroads serving the same place, however, I learned a lesson about our society: they all got up, and that was the end of our meeting. They said, "We couldn't choose among the railroads. We belong to them. We can't advise you any more. Go ahead, choose one." Nobody wanted to stick his neck out that far against his business interests. So we didn't get very far on that, and I began to think of other ways to subsidize travel. The only fair way to do it, I thought, would be the distance equalizer. We would give people full room and board when they arrived in Chicago if they had come from far away, whereas if they had come from only a few miles away they could pay for their food. And they understood this very well, and it worked out nicely. Obviously, we ran into a deficit later because so many people didn't pay their bills, so to speak, and we had to do it, but it worked out all right.

RAR: The American Indian Chicago Conference was the outgrowth of a lot of experience in action anthropology and other kinds of interactions with Indians as well as learning on the spot. So the method is flexible—

ST: The method is entirely flexible.

RAR: The philosophy is constant, but the techniques shift depending on the context?

ST: I think about the philosophy often, you know, worry about it, naturally in retrospect. One thing that has become so much a part of me that I can't separate it from my personality, no matter what, is that I never make a decision until the last moment—something might change that would make a better decision possible five minutes from now. The journal, the conferences, started with ideas in my head, and I would have dropped them immediately if they hadn't met with an enthusiastic response. A philosopher named Diesing who was a student of ours studied the Fox Project and showed that the philosophy wasn't really unique to me but came out of the group [Gearing, Netting, and Peattie 1960]—but I became so imbued with it that for the rest of my life I was stuck with it, so to speak. I won't tell my wife what I want for supper.

RAR: That can make getting Peking duck difficult.

ST: Yes, but if I may say so, I've lived an extraordinarily, to me, lucky life. And since I know people, many of them, who lead less fortunate lives, I can't help but think that whatever good things have happened to me in the world are some combination of good fortune, however you define it, and, presumably, some skills that made that possible.

MN: In the 50 years you've been an anthropologist and watched the discipline grow, what do you see as continuities and changes?

ST: I'm sure that the continuities are more important. Thinking of the 1820s–1840s, when it was first called ethnology and then anthropology, as its beginning, I would say that from my point of view nothing essential has really changed. From the beginning, one aspect of it was trying to end slavery or save the world in some way—in other words, there was the idea that anthropology is pursued for some good purpose. At the same time, it was understood that in order to do anything at all you had to know much more about the whole world. The growth of museums at the time contributed to the unity of the field, because in the museum you have a geographical framework but all of the parts of a culture—language, physical and material aspects—are considered together. The original theory was that to understand how peoples came to be what they are today you had to know about all these things—how languages diverged, how cultures originated and diffused, the nature of physical types, etc., in other words, the history of the world. We talked about a banyan tree with overlapping branches, and there were different ways of straightening things out; we've gone from the historical to the much more psychological, sociological, and culturological, but the task has always remained the same.

The major change I see is the increase in the number of anthropologists. When I talked to Ralph Linton after his first lecture at the University of Wisconsin and told

him I might be interested in changing my major to anthropology, he said that it was a good field—there were only about 50 professional anthropologists. I suppose the figure 50 wasn't exact, but it really was a small group. When we were very few we were in communication in a much more personal and direct way. When we grew from a few hundred to many thousands, we obviously had to change the form of our communications. I'm pleased to think that I had something to do with calling attention to the universality of anthropology—that it isn't just Europeans and North Americans studying other peoples but people studying themselves and talking back, so to speak, to anthropologists all over the world. The spread of anthropology among the colonialized peoples of the world has had a great effect. The conference that resulted in *Anthropology Today* created a feeling of unity on a new worldwide scale.

MN: It's interesting that you see these continuities. The last line, you remember, of Tylor's [1871] *Primitive Culture* is that anthropology is a reformer science—if not action, then a critical stance has always been part of anthropology. And the multiplication of numbers has been astronomical since the 1930s. And most people now take it for granted that anthropology is no longer a Euro-American enterprise. I wonder if that kind of diffusion will have any effect on the way we work—what our problems are.

ST: We only began to pay attention to the women's side of life when women began to take notice and tell us that we had to. And again, I had been all my life a great liberal and a nonracist—there's no use arguing about it, I was—but at every step I had to learn from blacks (then Negroes) from their point of view. Institutional racism was a concept that had never occurred to me—the idea that people of goodwill could be part of institutions that made equality impossible. Only the person who has the experience is going to be able to see the particular truth of it and point it out to others. To the people of the colonized areas who are becoming anthropologists it's clear that we've been seeing everything from an evolutionary point of view, and even if we become antievolutionists we still act on the basis of those hierarchies. I remember a conference I had in Brussels in connection with the planning for CURRENT ANTHROPOLOGY. There was a whole roomful of anthropologists just about to return to the Congo, and they were all confident that that country wouldn't go the way of India and Indonesia—that everything was going to be quiet there. It wasn't more than a few months later that the bloodiest of all anticolonial revolutions broke out there, and everyone was completely surprised.

MN: It wasn't only anthropologists, either; I recall our leading Africanist political scientist at the time saying that there would be no smoother transfer of power anywhere than in the Congo. So it was a Western blindness.

ST: People of other cultures and other classes are going to begin to make us change our views. And we'll have

to go back to the theory and say that the reason we described people's ways of life the way we did had something to do with ourselves and the theory we were thinking of.

MN: Theory tends to be just argument among people who live in universities rather than related to the phenomenal world out there. It's the continual academic locus of anthropology that gives it a language and problems that wouldn't occur to anybody who wasn't living in that environment.

ST: That reminds me to say that another of the big changes in anthropology is that, at least in the United States, we can no longer be as academic as we once were. There aren't enough jobs in universities, and there are more professional anthropologists employed outside them than there are inside. This too is going to change our theory and our problems.

MN: What do you see as continuing to be distinctive of anthropology as an enterprise? I don't agree with Goethe about the blurring of genres so that all intellectual activity is like all others; I just think you need a new pair of glasses.

ST: I think of Margaret Mead as going from one society to another and always finding something sensationally interesting. Anthropologists can do that. It's a matter of bringing new perspectives to a problem that introduce ideas that no one else would have thought of. The anthropologist will always bring the broader cultural perspective. What we have to offer is not some set of things that are or are not so but a series of ways of thinking about the complexities of the world. Speculation of one kind or another has always been part of the game. Whether or not the particular phenomenon is true is something that can be demonstrated once the question has been raised.

MN: What most people seem to be doing today recalls an older American tradition once called cultural anthropology—here locally called symbolic anthropology—that gives little attention to society or history. The Berkeley approach is being revived even here in the home of social anthropology. How do you react to that?

ST: I would never have been able to accept a change in the direction of dealing with culture only as symbols instead of, as we thought of it, ways of behaving; in fact the tradition was there in the mind and behavior was in terms of it, but never perfectly so. But I think of this change, which you're trying to make more of than I would, as much like the fad for culture-and-personality or psychological anthropology. When I joined the faculty here basic personality structure was a great thing; it wasn't new to me, but it was a great thing, and the people I worked with were doing it. In five or ten years it was a thing of the past. It would be interesting if we could measure the effects of things like that and compare them. I believe in following them through as far as

they go, but eventually they'll need to be dropped. Things tend to become popular and then be forgotten, generation after generation; you'll always have some things that are fashionable. There are very few new ideas, obviously, in as complex a field as this one, and the old ideas tend to be remolded slightly in different ways. We haven't succeeded in making anthropology cumulative in the way that medicine is; when I was young I dreamt that someday we would.

MN: We might talk a bit about mentors and influences on your thought.

ST: If I'm a disciple of anyone's it would be Linton's. He was interested in the nature of civilization, past and present. He dedicated *The Study of Man* [1936] to the next civilization; he thought that this one was going to end the way others had ended. When he went out of town I would occasionally read his lectures for him, and when the star basketball player needed tutoring to remain eligible for athletic competition Linton was asked to find someone to work with him and chose me. I was successful in that, and he passed the exam well. The exam, incidentally, was an interesting one; the assignment was to outline the course. I think it was a good educational device, and I've used it in my own work. My aim in my graduate work was always to get the whole thing, whatever it was, down to a one-page outline.

I continued to see Linton after I went on to Chicago, because Gertrude lived in Madison. He read me *The Study of Man* chapter by chapter as he wrote it, and we argued about it and discussed it. I won't say that I contributed anything, but he tried things out on me and I may have had some interesting effect on the results, who can tell? He was very much interested in Radcliffe-Brown, as everybody was—all the anthropologists in the United States were up in arms about this person saying everyone else was wrong. At first Linton wasn't sure whether I was going over to the enemy, so to speak. My job was to interpret for him what Radcliffe-Brown was teaching us—and when you interpret something, you're the messenger who gets blamed for it. Eventually, through these visits, I arranged a great debate at the University of Chicago between Linton and Radcliffe-Brown on the proposition that laws are possible in anthropology. Linton was worried that he wouldn't do well. Radcliffe-Brown was his suave self with his monocle and fine manners and British debating ability, and he knew he was up against a difficult opponent. Nevertheless, the debate came out very well. Nobody was disgraced. Linton had worried unnecessarily; he held his own, and all of us involved were satisfied to have heard both sides, neither of which we followed completely. Some years later, Linton asked me, "Sol, when you arranged that debate between Radcliffe-Brown and me, whom were you trying to get?" This was characteristic of his reputation as paranoid. I was exceedingly fond of him.

MN: And here in Chicago?

ST: Obviously, I came under Redfield's influence as much as anybody's. His intellectual habits were very well suited to rational discussion. He was so much brighter, knew so many more things than I did that of course it was frightening to me, as it was to many students. He'd say, in that lawyer's way, "What is it you really mean?" and then figure it out and say it better than I could have. When we became friends we came to respect each other's ways, and I was no longer frightened of him. I found that I was able to solve practical problems of interest to him—how to get information, how to do things, how to deal with people. In one of my first papers on Guatemala [1941] I suggested that the gradations of folk culture that he had found in Yucatán didn't occur in Guatemala, and it shocked me that friends treated this as opposition to Redfield—disproof. Redfield and I had worked on these things together, and we saw the differences. I wasn't writing anything that he didn't know all about. It never occurred to me that we were in any way opposed in our views. Of course, he did like the country better than the city, and with Louis Wirth I favored the city.

I didn't know Radcliffe-Brown as well as I knew Redfield. I was a new graduate student, and I was determined as every student is to learn what the master was saying. He was interested in a great many things—philosophical ideas, ideas about language and culture and so on, that were a little different from what I had heard, but especially of course he was a sensation here because he believed, or at least acted as if he believed, in the eventual discovery of laws of human society. This was anti-Boasian, because the people who believed in laws had been the people who made those evolutionist mistakes, and American anthropologists, even Redfield, obviously didn't accept the notion that there were these laws; they were questions rather than answers. Since I was explaining him to Linton, I really came to understand what he had to say, and by the end of the quarter I was enjoying it but not necessarily accepting it. I said so in the first paper I wrote for him. I learned what kinship was all about from him. I took all his courses, and we never discussed fieldwork until the day before we were going to the field—it was always theory and reminiscences of one thing or another on a worldwide basis. When we said, "We're going to the field in a couple of days, what are we going to do?" he said, "Buy a large notebook, and open it in the middle and start a genealogy, because you never know where it's going to lead you on either side." In my Ph.D. thesis I used his principles and went off in another direction, and he liked that.

The experience that I had with all my professors—Fay Cooper-Cole, too, who was the most generous person possible, never political, always thinking of the good of the community and the individual student and a great peacemaker—was that they were not in the least self-serving. Radcliffe-Brown is considered to have been a great egoist who thought a lot of his theories, and this is true, but he never let this interfere with his discussions with students. One could argue without any danger of his taking it personally. I never had the feeling

that anybody was after anything but understanding and pure intellectual discussion. This is an ideal that I never saw violated in our department. And with these role models I was never tempted to behave any other way myself.

MN: What sort of curriculum would you have for training professional anthropologists today?

ST: In the immediately postwar era, we developed a curriculum for the new graduate students who were arriving at the university. It had three major courses—Human Origins, Peoples of the World (which Fred Eggan took charge of), and Culture, Society, and the Individual—on which we all worked. It's one of the things I'm most proud of; it was way ahead of the field in its time and was widely distributed not only in this country but in Europe. If I were to create a curriculum today, it would be more along the lines of the experience with CURRENT ANTHROPOLOGY—with discussion as its distinctive feature. It's always seemed to me that a good university is a place where good students from different backgrounds come together and teach and learn from one another. The professor is there as a sort of umpire; he has almost nothing to do except make minor decisions. The rules of the game are there—the ideas are there. Where good students are learning from each other, they will tell you what anthropology is. The teacher may point out a book on the subject that might be worth reading, help students to make judgments, keep the discussion free (perhaps not permit one student to take up all the discussion time). So rather than produce a curriculum I think I'd organize the students in this way.

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of antiquities and "exotic" objects; military campaigns, colonizing projects, and attempts to establish formal governmental administration generated ethnographic data of all kinds, and in various regions of the country learned circles took an interest in precolonial antecedents. *Hispanistas* and *indigenistas* embarked upon their national projects, and the conflict between church and state, on the one hand, and between liberal concepts and indigenous organization, on the other, contributed ethnological and historical data and reflections.

An especially important role in the amassing of this kind of knowledge was played by the Museo Nacional (founded in 1825 and significantly reorganized in 1865), which accumulated archaeological, ethnographic, and linguistic materials of all kinds, attracted experts in the field, and published the first specialized periodicals. In the last decade of the Diaz dictatorship scientific positivism, with its evolutionist orientation, made an impact on the emerging anthropology, contributing to various attempts at the systematization of empirical materials, research, and teaching. In 1910, on the eve of the revolution, the Escuela Internacional de Arqueología y Etnología Americana, cosponsored by the governments of the United States and Germany and strongly influenced by Franz Boas, was founded in the capital. This institution did not survive the revolutionary period, and other nuclei of anthropological teaching and research were also seriously affected by the social, political, and military convulsions of the second decade of this century.²

NATIONAL DEVELOPMENT AND THE QUESTION OF THE INDIGENES

Although little effort has been made to elucidate the transition, there is broad consensus that Mexican anthropology was consolidated as a modern scientific discipline in the twenties.³ Significant steps in this process were the important pioneering study of Manuel Gamio on the population of the Teotihuacan Valley, the creation and disappearance of a whole range of governmental organizations oriented toward the study of the indigenous population and the extended debate over the prospects for its participation in the postrevolutionary nation, and the founding (in 1937) of the Sociedad Mexicana de Antropología, (in the same year) of the Escuela Nacional de Antropología e Historia, (in 1939) of the Instituto Nacional de Antropología e Historia (which later incorporated the Escuela, for three decades the only institution for anthropological training in the country and to this day the only one that shelters all the anthro-

2. See García M. (1987–88: vols. 1 and 2) and, for general reviews of the development of Mexican anthropology, Lameiras (1979) and Vázquez L. (1987). Palerm (1980:35–64) offers an interesting perspective on the paradigmatic sequence with regard to the interpretation of the rise of the Mesoamerican civilizations.

3. See especially García M. (1987–88:vol. 2) and also Olivé and Urteaga (1988) and Instituto Nacional Indigenista (1988). Comas (1964) succinctly describes the applied anthropology of the time and offers a selection of texts.

A Panoramic View of Recent Mexican Anthropology¹

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From a certain point of view, anthropology in Mexico originated in the first contacts between the European invaders and the Americans of the region and extended throughout the three subsequent centuries of violent incorporation of the latter into Ibero-European Christian civilization. In fact, the accounts of the conquest (of which the conquerors' are virtually all that have survived), the reports of royal and religious administrators, and the documents of the long controversy over the human status of the Indians still contribute, along with the results of archaeological investigation, to the reconstruction of important aspects of the pre-Hispanic history of the area and its development in the colonial period. After Mexico achieved political independence at the beginning of the 19th century these materials were enriched considerably: foreign and national travelers crisscrossed the country recording their observations in letters, reports, and sketches and assembling collections

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