

An SfAA Oral History Interview with Charles P. Loomis

Conversation with one of the SfAA's Founders

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Interview by J. Thomas May and Peter K. New.

Charles P. Loomis was one of the small group of founders of the Society and served as its president, 1949-1950. Trained as a sociologist he was awarded the Ph.D. from Harvard in 1933. He served as the president of the American Sociological Association. His research program was focused on rural life and agriculture. He was very much involved in policy research for the United States Department of Agriculture during the New Deal in the early 1940s leading up to the creation of the SfAA. Following World War II he served as the chair and then research professor in Michigan State University's Department of Sociology and Anthropology. He was one of a number of sociologists that were important to the early development of the SfAA. This interview was done by Tom May and Peter New in 1979 at Las Cruces, New Mexico and edited by John van Willigen. The transcript apparently was edited by Loomis, and I assumed that the text in parentheses were added by him. Through Tom May, the transcript is now part of the Society's oral history collection at the Louie Nunn Center for Oral History at the University of Kentucky Libraries.

MAY: I think that for our purposes we might now move to some discussion around the topic of your contact with the people who became known as the 'founders' of the Society for Applied Anthropology. You have provided us with an excellent description of several contexts for this contact—the Department, your own intellectual interests, etc. I assume, as a corollary of that, that the travel and the commitment to keep up with your profession and the bringing of these academic scholars to the Department furthered that. Perhaps you could now give us your recollections of your initial contact and discussions around the idea of formalizing the group into an organization.

LOOMIS: I wish that I could do this better than I can. It is hazy. And I can't place everything in a time orientation. But, one of the important developments in the Department of Agriculture (and I don't know if it is on the Kelly Interview or not) was that M. L. Wilson happened to be in Rome at one time. He was looking in on a FAO office. He was standing in a corridor, and he began talking with a young fellow, Ralph Danhoff, who had just finished a Ph.D. thesis at the University of Michigan on the Coulee, or Boulder Dam (later this became the Hoover Dam). And they talked about this. I can tell you the results of the study but it is probably oversimplified. These

construction stiffs are really tough guys and you have all sorts of problems that result when you get a bunch of people of this type a long way from the city. So just to have order, they developed a rigorous system of 'cards'. If you had a card, you could be there. If you didn't have a card, they ran you out. Well, you lost your card if you misbehaved. This was one of the cases of a real totalitarian operation. Well Ralph described this and he had followed that longitudinally. According to him, they never were able to bring that back to a democratic system. It was impossible.

And that fascinated M. L. Wilson. He must now hire this young man. So he brought him in and he became one of my colleagues. We were going around the country (again, this points to the kind of freedom that we had). M. L. wanted us to go to talk to the people at Harvard; the people working in industrial relations—[Elton] Mayo and [Fritz J.] Roethlisberger and all of those. So we would go up and interview these people and come back and talk with M.L. about this.

I think that it could have been in some of those excursions up to Harvard before I came that I met with Connie [Conrad] Arensberg and Eliot Chapple and came to know of similar interests. I am just not sure about this. But this could be. In any case, when I was made visiting professor, in those years, Eliot Chapple and Connie Arensberg, and I think Bill [William F.] Whyte, were there at the time. You see, I think that Bill was doing his street-corner society study at that time. Somehow they invited me to join that group. They invited me to this, I didn't initiate it.

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LOOMIS: I was so interested in my own publication work, and I was trying to find a way to make what we wrote have meaning for activities that were being carried on. And Chapple and Coon's book (Principles of Anthropology) fascinated me, because it is very usable in many ways. At least it can be talked about with applied people. You probably know that Chapple and Connie Arensberg, had this thing, one of the early small group analytical devices where you look through a one-way mirror and you have these things going and you watch the interactions and you study and you do statistical analyses of the interaction. Well, I was tremendously interested in this. That is about all that I can recall.

MAY: Your recall is that these excursions brought you into sustained contact with the group for your own interests, as well as acting as an outpost for M. L. Wilson.

LOOMIS: Yes, it was for my interests particularly with Mayo, and Roethlisberger and those people. I don't think that Roethlisberger and Mayo were even in that original

group. There was an overlapping there. And they knew of this interest. "Why was Loomis involved in this?" So, that could have been the reason why they invited me. Also, they were interested I suppose in looking at this rural sociology. You see, there would be this relationship between applied anthropology and rural. And it may have been that I was just a rural sociologist and had been doing these studies. And, of course, at one of the early meetings, I presented the Dyess Colony Project data I mentioned earlier. I presented other things at their meetings, so that they immediately saw the applicability of it.

Then I think that the reason why I became President was because quite early, when I saw how this thing was developing, I wrote all of the rural sociologists and told them what we were trying to do in applied anthropology. And we got quite a large number of members who were rural sociologists. It could well have been just Chapple and Arensberg. Especially Arensberg would have known because of his and Kimball's rural Irish study. I think that they probably knew of kindred interests. But it wasn't from Washington or from other contacts.

MAY: So that when they invited you and encouraged your interest, it was essentially a Cambridge-Harvard group.

LOOMIS: Well, yes, in the early days. But then, they were on the outreach, and Margaret Mead came in. If Bill Whyte was not already in it, he was there. They were trying to think of people who would have a similar interest. And they wouldn't think, as Eliot always said, that they had to go out and beat the bushes to learn something about human behavior.

NEW: Would it be fair to say that this early group of Chapple and Arensberg and some of these people formed the nucleus of the Society in part to broadcast their own work?

LOOMIS: No, I didn't ever get that feeling. It could be. You could say, "Well, look, this is what we are doing. You ought to see if it has wider use." But they never... Well, the instrumentation of these discussions was certainly there. We certainly learned about that over and over. So, one could say that.

But, as some of your other interviews will show, there was this difficulty between Eliot and Lloyd Warner and that came into relief. The Warner thing is going in one direction and Eliot and people like George Homans who want to keep things in hard-nosed, specific terms so that you can really talk about prediction went in another. This

was an interest of several of those people, and that is the reason why something like sociometry would have a meaning. There was a core of that. But of course, Margaret Mead did not fit that too well, and there were others like that.

MAY: Was there any other organizational outlet or support mechanism for people doing applied rural sociology at the time which was at all analogous to the group in Boston?

LOOMIS: I don't know among the anthropologists. They knew one another and there was certainly a number of people like Ned [Edward H.] Spicer and these many anthropologists who were working with Indian problems and the Japanese projects, who were in interaction with one another and came in later. But, I don't believe that there was any group. Also Sol Tax at the University of Chicago could be mentioned. He stayed clear of the Society.

MAY: I think that we left off with your description of your leave from the Department of Agriculture in 1942 to go to Harvard. You mentioned that you were there for two years. Could you summarize the chronology of what followed for us?

LOOMIS: The position at Harvard was a fill-in for Zimmerman, who was on leave with the Army. It was for two years. Then I came back to the Department of Agriculture, and almost immediately, I moved out of the Bureau of Agricultural Economics into what was called the Office of Foreign Agricultural Relations (OFAR) in the Department of Agriculture. In other words, I had developed this international interest and some connections which led to work especially in Latin America as the war unfolded. I was there in the Office of Foreign Agricultural Relations until I went to Michigan State.

MAY: What year was it that you went to Michigan State?

LOOMIS: That was in 1944 and I remained the Head of the Department of Sociology and Anthropology until 1957.

MAY: So that the period in the Office of Foreign Agricultural Relations was fairly brief?

LOOMIS: Yes, it was. I was there for a little more than a year, during 1943-44.

NEW: Could we now spend a little more time on your second round back at Harvard? Just before lunch break, you had mentioned that through M. L. Wilson earlier, you had met Professors Chapple, Arensberg, [F. L. W.] Richardson and those people. Also, that this related to your own interests, and those of Wilson in industrial relations.

When you then went back to Harvard, you also mentioned that they invited you to meet with them, and you gave some talks. You indicated your growing interests in the interaction thing that Chapple was working on. If you recall, I would like for you to discuss what was it that got the group to say, "OK, let us form this thing or organization called the Society for Applied Anthropology?"

LOOMIS: Just to correct one thing. I think that in my developing relations with the people who did later form this group, say Freddie Richardson, Elliot Chapple, Homans and the others, that I was just trying to feel how I had come to be invited into the group who were to form the Society for Applied Anthropology. This was not a strong contact. My coming back earlier was largely with people that they knew. For instance, Freddie Richardson knew Roethlisberger extremely well. And of course, Homans did too. And I had come back largely from Wilson's interest in Roethlisberger and Mayo, working on industrial problems. But they knew... I just thought that that might have been a way that they became aware that I had similar interests. I did not know them well before we formed the Society.

But moving then back to your other question of how the impetus for the development of this organization took form, I have to admit that I was not a driving force in this. Really, Freddie Richardson and Elliot Chapple were the main driving force on that, as I saw it at the time. But they had immediate support in Connie Arensberg and others who knew one another quite well. I don't think that Sol Kimball was in it at this time. But you see, Arensberg and Kimball, who did the Irish study, were tied in to a group there that was called the Junior Fellows. It was related to the club that Whitehead, Henderson, Homans and these people were members of. It was important to them and this provided one nexus that made an immediate group. I think that you probably know of the development of it there on campus led by Henderson.

NEW: This is the Society of Junior Fellows.

LOOMIS: Yes. I had no connection with that group at all. But these other people did. As a matter of fact, that is the group that was so active and involved in so many ways with the analysis of Pareto's work. I was there at Harvard at the time but not a part of that as Homans and Merton were. I don't know to what extent Arensberg and Kimball were in that particular thing. But they were in this Junior Fellows group that I was not in at all.

I don't think that anybody had any decision to make about this. I think that it is just one of these things that happens. That here comes somebody in from the outside and he doesn't know of or get these connections. I think that this was the way it was. But whether it was this way or not, I think that Henderson had no use for Sorokin. And Sorokin, of course, was in charge of my work. Henderson had no use for any of Sorokin's people, or for sociology in general.

He thought it was hogwash. That would have been the way it remained unless a colleague of his, William M. Wheeler told him, "You should read Pareto." And when he read Pareto, he saw that Pareto, unlike other sociologists, made some sense to him. And that then began the seminar on Pareto that was to be so important in Parsons' and Homans' development. And it tied in, to a certain extent later, with the Junior Fellows.

NEW: Now in the early days, how long were you involved in the activities of the Society of Applied Anthropology?

LOOMIS: Chiefly as one who attended meetings, presented papers, and talked with these people during this period. I think that we may have had two annual meetings which I as a charter member attended during the period while I was at Harvard. I had quite a good many informal talks, one way or the other during this period with Chapple and Richardson and others.

I can give you one example that comes to mind. We met someplace and they happened to say, "Well, let's go over to the Business School" I think that this was Freddie Richardson. We went to chat with Roethlisberger. I can remember coming out of that meeting, and Freddie, who was the only one of the group who really ranks with the Boston 400, said that this class stuff, all of this emphasis on class, was a "bunch of shit." I can then remember that Roethlisberger said, "Well, tell me what does motivate society if it is not class. If class is shit, what does motivate society?" I remember that. That gives you some sense of the kind of interaction. It wasn't only on applied anthropology, it was on general problems.

I just really can't claim to be one of the architects in the early years of the Society except that we in rural sociology were always doing what they claimed that they were doing. They knew that.

NEW: You also mentioned that in the early days you made quite an effort to get the rural sociologists to join the Society as well. What did you feel, in those days, would be the benefits for rural sociologists to join with this group?

LOOMIS: I felt, and still do, that this interdisciplinary relationship when it is understood the way that it should be (especially with anthropology, sociology, and social psychology) can approach practical problems more effectively than a single discipline can. But, you have to also see this against the background of M. L. Wilson seeing it this way in the Department of Agriculture, and bringing in colleagues who were trained in this.

The first thing that I did when I went to Michigan State University which then had a Department of Sociology, was to get the name changed to the Department of Sociology and Anthropology. This was something following the developments at Harvard, too, of Social Relations. Well, of course, I was approaching it differently than was Harvard. But I felt that specialists really could not deal with these applied problems.

MAY: One of the things that I am interested in concerns your own interest in agriculture and rural sociology, as opposed to those people in the Society who were more interested in urban and industrial problems. Am I correct in thinking that the central thrust of that group was toward industrial and urban problems, as contrasted with rural problems?

LOOMIS: You are thinking of the Society?

MAY: Yes.

LOOMIS: I don't believe so. Now Bill Whyte might be over on the urban side. There was quite a great interest in it. But then you had Margaret Mead and you had Arensberg and Kimball who had done the Irish Countryman. No, I don't think that it was that really.

MAY: During the period, 1942-44, were the people who were beginning to participate actively and form the Society on the fringe of the conventional disciplines, in terms of what we know about career patterns for rural sociologists or industrial sociologists? Now, admittedly, this is something of a naïve question.

LOOMIS: Well, I don't think that it is naïve at all. It is crucial because there is some real theoretical background for such a hypothesis. I think that I would answer it by saying "yes, but maybe for a different reason than you think of. I think that the people who are center are often busy with the center. And the people who are brilliant and starting are in quest of new ways of looking at things, and also of improving their rank. So I would think it might be that. I would say that there probably is some of that kind of relationship.

Now I am thinking chiefly of my own colleagues in sociology. But it would also go over to Eliot Chapple and a combination of anthropology and what not. And also to Richardson. This would be true. Those people were on the make and able. All of the big shots were up here, and much of the difficulty was that those slots that they wanted to be in were filled.

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Retired general Haim Eshed, 87, who headed up the Defense Ministry's space unit for nearly three decades, alleged the existence of an alien alliance spanning the galaxy in a bewildering interview with Israel's Yedioth Ahronoth newspaper. "The UFOs asked not to advertise that they are here, [because] humanity is not ready yet," Eshed told the paper on Saturday, adding that US President Donald Trump "was on the verge of finding out, but the aliens in the Galactic Federation say: wait, let the spirits calm first." Like this story? Share it with a friend! Follow RT on. Trends:Israel news Space exploration. medium to educate, preserve oral history, and convey cultural norms to the indigenous people. (Verbina & Damodaran, 2013). Nigerian communities used storytelling as oral discourse, since millennia in Nigeria in the oral tradition, which continues even with the evolution of written stories. Finally, language is a bank of knowledge since proverbs and cultural history are included in both the oral and written tradition. Although European colonizers renamed lands, bodies of. Charles Williams- Interview by Stanley H. Hyland and Linda A. Bennett; published in the May 2013 Newsletter Applied Anthropology in Service to One's Community: A SfAA Oral History Interview with Charles Williams. Charles P. Loomis- Interview by J. Thomas May and Peter K. New; February 2013 Newsletter SfAA Oral History Project: Conversation with One of SfAA's Founders: An interview of Charles P. Loomis. Frederick L.W. Richardson- Interview by J. Thomas May and Peter K. New; published in the November 2012 Newsletter Present at the Founding of the Society: The SfAA Oral History Interview with Fre Oral historians have traditionally favoured the face-to-face interview and discouraged remote interviewing. It involves a different skill set, and it is often difficult to build rapport from a distance, to be sensitive to mood changes, to provide non-verbal feedback, and to establish the relationship needed for a successful oral history interview. Interviews recorded via telephones and web services are often of poor audio quality, result in digital files that cannot be archived, and present challenges around data security, and signing and storing interview documentation. In some cases an oral history interview might function as a coping mechanism for the interviewee and help them to process their experiences of the pandemic. In this case an interview might be beneficial. Interview with dr. Charles Lundquist interviewed by Stephen P. Waring July 7, 1992 UAH/Huntsville, Alabama. 1. Waring: I'm talking to Chuck Lundquist. We're going to talk about Space Sciences in the 1970s. I arrived on a Monday morning and had an entrance interview with Rocco Petrone, Marshall Director. That lasted until about 9:00 or so. As I was leaving that meeting, the person that had been acting director, Dr. Walter Hausserman, gave me a memo, or a fax, that had just arrived, assigning Marshall the responsibility for planning the Skylab operations to observe Comet Kohoutek.