

## Frank Miller

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## **Interview with Frank Miller**

**Interviewed by Professor Clarke A. Chambers  
University of Minnesota**

**Interviewed on October 12, 1994  
University of Minnesota Campus**

Frank Miller                               - FM  
Clarke A. Chambers                       - CAC

CAC: This is Clarke Chambers. It is October 12, 1994, Wednesday morning. I'm interviewing, in my office, Professor Frank Miller who has been with the Anthropology Department in the University of Minnesota for a number of years. I'm looking to see when first you came here . . . 1964.

FM: It's thirty years to be exact.

CAC: Thirty years on the nose. Professor Miller has been very active in departmental, college, university affairs, particularly in international programs and we'll get to that later, and whatever other things you think are relevant.

As I have with everyone else, I think it's useful to start with your intellectual autobiography or academic autobiography. How did you get interested in anthropology? How did you get from mathematics to anthropology? How did you get to Harvard? What happened when you got to Minnesota?

FM: It's a pleasure to talk to you, Clarke. I will try to very brief about some of these things. I was born and raised in Quincy, Illinois, on the Mississippi River, and went to public high school. I first came to Minnesota to Carleton College because I wanted to go to a small college, fairly far from home but not east or out west.

CAC: Was your family college educated?

FM: My mom and dad both graduated from the University of Illinois. Carleton had an excellent reputation for international relations, which is why I chose it. In high school, I became interested

in the movement for world government. This was 1947 to 1950 before the cold war had really solidified. There was a lot of optimism about strengthening the United Nations.

CAC: Appealing, particularly, for a young bright kid?

FM: Yes. In connection with that, I subscribed to a journal published at the University of Chicago by the Committee to Frame a World Constitution, which had been created by the president of the university at Chicago, [Maynard] Hutchin, on which was a leading anthropologist, Robert Redfield and a number of other luminaries from there. That was a major intellectual influence in my high school years.

CAC: You knew that he was an anthropologist?

FM: Yes. As a matter of fact, I did; but, I didn't know what anthropology was. I really was more interested in the international relations aspects of it at that time. It was in connection with that that I learned about that movement for independence from colonialism, especially in Africa. I went to college . . . couldn't stand the main professor of international relations. Intellectually, I didn't get along with him. Personally, I thought he was extremely pompous.

CAC: It wasn't still Brynn Jones?

FM: No, it wasn't Brynn Jones. He was retired by then. It was Reginald Lang.

CAC: I see. I had Brynn Jones and I felt that way toward him. [laughter]

FM: He was still an influence; but, he was retired. Capper Johnson was still there though I believe; but, he retired very soon. Then, I discovered philosophy in a freshman course, where we read [Charles] Darwin, [Sigmund] Freud, and [Karl] Marx. I never was assigned those people in any other classes at Carleton; but, I had them in this odd ball freshman philosophy class, which was limited to, like, fifteen people. Through that, I got interested in the philosophy of science. I greatly admired the main professor in that subject, Milets Chopak, who was a refugee from Czechoslovakia. I was greatly influenced by him and, basically, he influenced me to major in mathematics because if I wanted to do the philosophy of science I ought to know some math and I had been good in math in high school.

CAC: On the surface, it would seem that mathematics as a discipline ran counter to your other . . . it would be a tool discipline rather than a substantive?

FM: I was moving away from the more practical side of my interests at that time in international relations and was much involved in reading the philosophy of science. Chopak only could teach one course in it. He didn't get along with the chair of his department very well intellectually, Martin Eschelmann, and he later moved on to Boston University. He recommended I not major in philosophy and do mathematics instead.

CAC: I see.

FM: This all happened my freshman year. Then, in my sophomore year, my then adviser was Philip Phenix, who was the chaplain and professor religion. He was an extremely intellectual person. He'd been a physicist. He'd been noticed by Alfred Einstein, and he became a hotshot statistician, and then he got interested in religion, and went to divinity school, and ended up at Carleton. He said, "Looking at your interests, I would think you would be interested in anthropology. Why don't you take this course?" It was my sophomore year. I was too busy to take it; but, I audited it. It was taught by Herbert Menzel, a sociologist, who was basically a quantitative sociologist who did some of the early work on the use of [unclear] scales. He did a very good job of teaching anthropology. Because of that then, I took a couple more sociology courses; but, they didn't have any other anthropology courses. I continued majoring in mathematics, but intending all along to go to graduate school in anthropology.

CAC: This would be true of many other undergraduate programs around the country, would it not?

FM: Yes.

CAC: There were giants in the field of anthropology going back to the early Twentieth Century, [Alfred L.] Kroeber and so on and so forth; but, it wasn't an established discipline very many places, except at large research universities?

FM: That's right, yes . . . especially not in small colleges.

CAC: I went to Carleton before the war and there wasn't any anthropologist at all. There was only one geographer and he taught world trade. Two basic disciplines and I had no idea about them.

FM: What attracted me to anthropology was, number one, it was the only discipline that dealt primarily with the non-western world or what later came to be called the Third World and secondly, the doctrine of cultural relativism, which was very appealing to me. I wasn't interested really in efforts to do more quantitative research in anthropology, which were not very strong at those times. As luck would have it, the Ford Foundation started a program of graduate fellowships in the behavioral sciences, including anthropology for math majors or other people in quantitative fields.

CAC: Heavens.

FM: So, I was ideal for that.

CAC: Oh, yes.

FM: I won one of those and went to Harvard, where I went to the Department of Social Relations rather than to the traditional Anthropology Department because I wanted the social theory. That was a great interdisciplinary experiment after the Second World War. It had been led by Talcott Parsons in sociology, Clyde Kluckholm in anthropology, Gordon Alport in social psychology.

CAC: You got to work with those three?

FM: Yes, yes, and Gardner Lindsay . . .

CAC: Oh, my heavens!

FM: . . . and Jerome Brunner, the cognitive psychologist. I didn't take any courses from him. He was the most stimulating lecturer of the bunch, of that set of luminaries.

CAC: You really lucked out.

FM: Yes, that was lucky. That was a great place. Intellectually, that was a great place; although, they weren't pulling off the integration of the fields because, basically, the others weren't accepting Parsons's leadership and his empire building tendencies.

CAC: It didn't become truly cross-disciplinary?

FM: No, it didn't. After many years, it basically collapsed and people went back into their own disciplines; but, it was a great experiment while it was happening. We got an interdisciplinary training even though there wasn't a lot of interdisciplinary research among the faculty.

CAC: You end up with a dissertation on medicine in Mexico?

FM: Yes.

CAC: This would suggest a kind of cross-disciplinary . . .

FM: Yes; although, that was partly accidental, too. There wasn't much in the way of African studies in anthropology any place in the country outside of Northwestern, in those days, with Melville Herskovits and there certainly wasn't at Harvard. [unclear] still interested in Africa, there wasn't anybody on the faculty who specialized in Africa. I was given an opportunity to join a project in Mexico. This would be a major study of cultural change in the state of Chiapas, which is now where the Zapatistas Mayan Indian rebellion is going on.

CAC: You kept in touch with that because you go back to it much later in your work?

FM: Yes.

CAC: We'll come to that later.

FM: I chose to concentrate on the impact of modern medicine not really for very thoughtful reasons. I think it was partly because there was a very good anthropologist in the School of Public Health, Benjamin Paul, who taught an excellent course on health in [unclear] perspective. That influenced me a lot. He was on my Ph.D. committee. I had the choice of doing, the way the project was designed at the beginning—although, they never followed through on this really—medicine, or agriculture, or education. I had some chance to take the courses in [unclear].

CAC: Medicine would be the more unusual?

FM: Yes; so, that's why I did that.

CAC: So, you come to the job market in the mid 1960s?

FM: Yes. I think I could tell you many, many stories about field work; but, maybe that's for some other time. [laughter]

CAC: Say something briefly. You're a young person; you hadn't done field work. Now, you have to go to Mexico and do field work?

FM: Yes, it was an education; I will say that. I had been out of the United States only to go to Quebec on a family trip when I was seven years old. I knew German fairly well because I had taken some German. German was my language in college because if I was going to philosophy of science I'd have to have German.

CAC: Sure, right.

FM: I didn't know any Spanish. I took a cram course in Spanish. We had a one-semester seminar on research methods; but, it involved no practice whatsoever. It just involved the professors coming in and telling us how to do a census, how to do mapping, how to genealogies without ever assigning any practical work; so, I really felt very much unprepared. People were wonderfully helpful. One essential part of that was the major force for change in that region in those days was a development program for the benefit of the Indian people operated by the National Indian Institute. This program had been designed and was administered by anthropologists. It was a pioneering program of that sort, the kind that later came to be called Integrated Rural Development; but, that phrase didn't exist in those days. The local regional director in the state of Chiapas was Alfonso Veja Rojas, who had been—quote—discovered by Robert Redfield. When Redfield was doing his research in Yucatán, Alfonso was a school teacher there and became his field assistant later, went to Chicago and got his Ph.D. under Redfield.

CAC: You were there in company on the field team?

FM: I was by myself. The big project did not get its funding.

CAC: I see.

FM: I got a Dougherty fellowship so I could go ahead. My adviser, Evan Vote, made an arrangement with David McClelland, the social psychologist known for his work on the achievement motivation. If I would collect some data for McClelland, do a comparative data in a village newly converted to Protestantism and one that was still Catholic, then McClelland could test his hypothesis derived from a Protestant ethic and the spirit of capitalism essentially, which is non-verbal psychological tests of achievement using doodles and using a ringtoss game because of the risk taking [unclear].

CAC: Ah!

FM: I agreed to do that and McClelland had a big grant so he bought a land rover, a four-wheel drive vehicle for the Harvard project and I got to use that vehicle for the year. I was the first field worker in this project that later became famous and infamous because it was sort of the beginning of the flood of North American anthropologists going into Chiapas. But, I was there by myself. Vote and I did a reconnaissance, two weeks, partly on horseback, to select a village site for me. Then, he left and went back to the academic year and I was there by myself. It was rigorous because, among things, I had a pregnant wife back home in Little Falls, Minnesota, and we had a year and a half old child and she was pregnant again. I was extremely reluctant to leave to do the field work; but, she strongly encouraged me to do so—in fact, essentially insisted on it. Sad to say, substantially later, that marriage broke up. Partly, with her support, I was able to do [unclear].

CAC: One of the hazards of doing interviews like this is that I get interested in things. Evangelicals were strong already in Chiapas?

FM: Already. Yes.

CAC: [unclear] in the 1970s and 1980s; but, I didn't realize they went back that far.

FM: Marianna Slocum, a linguist with the Summer Institute of Linguists, had settled in the neighboring municipio, in a small village in a place called Oxchuc, O-X-C-H-U-C. As a matter of fact, Veja Rojas met her because Veja and his wife were there doing field work. One very rainy night, the rain was pouring down in the rainy season in the summer, there was a knock on the door and there were two North American women there, Marianna and a nurse, Florence Gerdell. There were looking for a village to settle in, for Marianna to do her linguistic work and to develop an alphabet for the [unclear] language and translate the Bible. Veja Rojas and his wife helped them get set up in the field. Although, they didn't have any particular interest in Protestantism, they took pity on these people and, also, there was an attitude in Mexico that the linguists were doing valuable work because they were creating alphabets and actually writing

materials in the Indian languages for the first time. They were the first people to do that. In those days, Clyde Kluckholm told us that three-fourths of the articles in the *International Journal of American Linguists*, which was the journal that specialized in new world languages, were published by linguists associated with the missionaries. They were the people who were doing the basic linguistic work.

CAC: In Chiapas, Indian languages are spoken more than Spanish?

FM: Yes. This was one of the major concentrations of mono-lingual Indian languages.

CAC: And you had to learn that as well as Spanish?

FM: I never succeeded in learning very much. I had to learn Spanish after I got there.

CAC: You could do your work without the Indian . . . ?

FM: Yes. For other reasons, I chose a village that considered itself to be very progressive. In fact, I moved into and I lived in the medical post, a little one-room medical post in that village. It was the only place to live. I moved into it the week that it opened. The village had requested to have a medical post. They had requested that the one-lane dirt road that the government was going to build be relocated so it came through their village. In other ways, they were very traditional. They were very committed to maintaining their community and maintaining their version of Highland Maya Indian culture. By no means were they 100 percent modernizers. In fact, I think I was one of the first critics of the view that tradition was a great barrier to rapid change. I think in many cases, there's very much of a mixture.

CAC: That was being immersed and learning fast on your own.

FM: Yes, I learned fast; but, people were extremely helpful. Veja Rojas was wonderful. He was a very humane person. He was very much like Robert Redfield in that respect, just an extraordinarily humane person and an extraordinarily good scholar at the same time, which is a combination that doesn't always go together.

CAC: In a way, this became a model for your own work as scholar and teacher?

FM: Yes, it certainly did.

CAC: We talk about models . . . they're pretty important.

FM: Yes. I could go on a lot about field work; but, let's move on.

CAC: Okay.

FM: This was in 1957, 1958. I got back home in the summer of 1958. In a few days, I had a telephone call from Carleton College. I had a social science research dissertation writing fellowship to go back to Harvard to write my dissertation; but, I got the telephone call from Carleton College with the tragic news that Dave Okada, who was a relatively young Japanese-American sociologist there, had died of a heart attack. Classes were going to start in about two weeks; so, immediately I called up the dean. As luck would have it, the new dean of Carleton was Philip Phenix, the religion professor. He had gone to occupy the John Dewey Chair of Educational Philosophy at Columbia. He was so versatile. He was hired away from Carleton for that chair and then he was hired back to Carleton as the dean. They were kind of desperate. Although, they had other candidates, I was a shoe-in for that job. I had to make the choice of whether I'd take it or go back to Harvard and write my dissertation. It was appealing. I loved Northfield; although, I thought Carleton was more than a little bit too much self-congratulatory in those days. But certainly, Northfield was a delightful town. It was a good chance to settle down with my family and two young children.

CAC: I'm going to say a statement that you can take as a question. It probably had one of the best presidents of any college around at that time?

FM: Yes.

CAC: That makes a difference in morale.

FM: Yes, it certainly does.

CAC: We're referring to Larry Gould.

FM: The quintessential college president.

CAC: Yes.

FM: He was excellent as a leader of the faculty, excellent internally, and just a stupendous fund raiser, too.

CAC: Larry, as a teacher, was a model for me more than any other teacher I had at Carleton back when he was just a professor of geology and not president. You had four or five years at Carleton?

FM: Yes, I had five years there.

CAC: It gave you time to finish your dissertation?

FM: Yes. I finished my dissertation in the first year and a half, working very, very hard teaching a full-time load.

CAC: Oh, boy!

FM: Then, the University of Minnesota Anthropology Department had gotten a training grant from the National Institutes of Health [NIH], which was in those days putting a lot of money into basic research and training in the social sciences, even though the work wasn't necessarily directly relevant to medicine. It wasn't my research on the impact of medicine in Mexico that they were most interested in, as a matter of fact; but, they wanted somebody do field training here locally among the Ojibwe Indians and there wasn't anybody in the department who wanted to do it. [Adamson] Hoebel and [Robert] Spencer could have done it; but, they weren't interested in the Ojibwe and they weren't interested in taking students out into the field to give them training in the summer time. So, I got an offer from here at the same time I got a National Science Foundation post-doctoral fellowship to go to Cambridge, England, to study with Edmund Leach. Minnesota let me take that. That was 1963, 1964. I came here in 1964.

CAC: This was no implicit cultural commentary on the Ojibwe by Spencer and Hoebel?

FM: It was. Yes, I think it was.

CAC: They were perceived as a forest people?

FM: They were perceived as being in fairly sad shape because they didn't appear to have as much traditional cultural vigor as the Cheyenne or the North Alaska Eskimo. Of course, Spencer had so many interests.

CAC: Oh! all over, right.

FM: All over the world. They, let us say, to put it politely, weren't much impressed with the Ojibwe culture, which I think was partly just a matter of ignorance because I discovered there was a very strong, although very closed off, stream of traditional Ojibwe religion, especially the Midéwiwin, which is called the Grand Medicine Society. It was still very strong at Red Lake in those days and was in the process of being revitalized. It was something that was pretty much closed off to outsiders; but, I learned a certain amount about it just in the course of being there and getting to know Dan Raincloud, Sr., who was taking over the leadership of it from an older fellow. He was a very powerful shaman himself. Basically, I'm sworn to secrecy. I haven't published any of that and I don't talk about it.

CAC: Some of my comments are meant to be in forms of questions and sometimes they're needling.

FM: Good.

CAC: This is the mid to late 1960s?

FM: Yes.

CAC: With feminism and with the Black Power Movement and there was also AIM, the American Indian Movement, so that a cultural interest in tribal culture was running pretty strong? The rhythm was going that way?

FM: Yes, it was. The blistering attack on anthropology by militant American Indians was about to begin. I did the field training school up on the Red Lake Reservation in the summer of 1965. I have neglected to say I had gotten a National Science Foundation and a National Institutes of Health grant to do field work on social change at Red Lake beginning in 1960, after I got to Carleton.

CAC: I see.

FM: That's why Minnesota was interested in me . . . because I had this on-going research program on an Indian Reservation that was well-funded by national agencies and perceived as progressing very well.

CAC: If I'm reading you correctly, there's not a paradox but a dilemma here of a need to understand American Indian tribal culture on the one hand and a resistance of tribal culture to white anthropologists?

FM: Yes, very much so.

CAC: You have to work with both things . . . one's working with you and the other against you?

FM: Yes. There was a lot of resistance. There was a lot of resentment toward anthropologists among knowledgeable and basically the more thoughtful people up at Red Lake. They're not the sort of people who wrote books . . . This attitude was encapsulated in a few years by Vine Deloria, a political scientist, who wrote *Custer Died for Your Sins*, which has one chapter that is really a very witty blast against anthropology, much of it well-taken, some of it exaggerated. As luck would have it, the chapter attacking anthropology was published ahead of the book in *Playboy* magazine and, therefore, was widely read on reservations. That happened a couple of years after 1965. So, this problem was going on.

At the same time, I was getting interested again in international things. What happened at Minnesota was the Office of International Programs had been founded in order to obtain and implement a Ford Foundation grant to help internationalize the university. This effort has been going on for a long time here. The second head of that was Will[ard] Cochrane, an agricultural economist, who was a leading member of the Ag Econ [Agricultural Economics] Department here. He had been John Kennedy's principal adviser on agriculture for Kennedy's campaign in 1960 and then later became, I think it was, deputy assistant secretary of agriculture for research.

CAC: I think Hubert Humphrey was a link there.

FM: Yes, I'm sure he was. That was sort of the head research job in the Department of Agriculture. He was hired back to Minnesota to be dean of International Programs, which is what they called the job in those days. He had heard about me and he appointed me chair of the Latin American Committee.

CAC: So, you're carrying two major responsibilities, one Latin America or International Affairs and the other is the very local, Indian . . . ?

FM: Yes. In the year 1965-1966, I was chair of the Latin American Committee. Then Cochrane decided the effort to involve CLA [College of Liberal Arts] faculty in particular wasn't doing very well, partly because Cochrane himself was an agricultural economist.

CAC: Sure.

FM: He frankly said he didn't understand the CLA faculty. He didn't understand their reluctance to do team research. He just didn't understand that culture; so, he wanted some help along those lines. In those days, you could appoint people without any kind of an elaborate search.

CAC: Right.

FM: So, he just appointed me as assistant dean in charge of on-campus efforts to get the faculty more involved, and especially to get CLA and other faculty working together, and to get some research centers started, and that sort of thing. At the same time, a couple of colleagues and I—Burt Pelto, who later moved on to Connecticut, and Luther Gerlach, who is still here—got a grant to study occupational choice in an industrializing area in Central Mexico. So, I was already working on that. I had gotten that grant before I was appointed assistant dean and I guess Cochrane was impressed with that proposal and how that project was going. Basically, I just abandoned the Indian research.

CAC: But your publications in the late 1960s are out of that?

FM: Yes, I did some publication; but, basically, I couldn't do both.

CAC: You were a busy young man because, in the meantime, you also had to be establishing your own courses and getting socialized to that department.

FM: Yes.

CAC: Could you say something about the department that you came into in the mid 1960s ?

FM: It was a very, very lively place. It had sort of an emerging sort of a classical conflict between the alleged old guard and the alleged young Turks. Ad Hoebel was chair of the department. He was a head in the old sense . . .

CAC: You bet.

FM: . . . in that he was brought here in 1954 as chair and he occupied that job for fifteen years. He very much was from the old school; although, he was very open to input from younger faculty. He wasn't running the department in an authoritarian way. Even so, because of the emerging conflicts of those days, there was a lot of turmoil going on. Ad and I used to have arguments because he favored the U.S. intervention in Vietnam and I did not.

CAC: I know that. He and I were neighbors and that was a terribly sore spot.

FM: It was a very sore spot. It was the days of student power.

CAC: Absolutely.

FM: The graduate program was really building up.

CAC: Young anthropologists were coming in radicalized on all kinds of issues, weren't they?

FM: Yes, except very few of us were people you would really consider to be radicals in a political sense. Bill Rowe, basically, was the only one and he was never all that active in departmental affairs. He was the only classical Marxist in the group; but, he wasn't all that active within the department—locally, he was very active. He was even a member of the Communist Party—or at least was said to be.

CAC: There were other powerful people in that department you came into and pretty free-standing. I can't imagine Bob Spencer taking much guff from anyone.

FM: No. In many ways, Spencer and Hoebel did not get along personally; but, they generally agreed upon major policy issues: resisting demands for student power, not favoring the establishment of a Department of African-American Studies, or the Department of American Indian Studies, or the Department of Chicano Studies, and on issues of that sort. Given all of that, I think the relationships within the department were reasonably amicable. We didn't have major battles. Sometimes, there would be disagreements about the admission of a certain graduate student or that sort of thing. The graduate students, I think, were more polarized against what they perceived as the old guard faculty than the faculty were. Burt Pelto was a guy who always liked a good fight. [laughter] He developed what you might call a coterie of graduate students and he very much favored more quantification in anthropology. I was somewhat sympathetic to that, but not nearly as strong as he was on that issue. He developed this coterie of graduate students and was seen by some of the senior faculty as too much of an empire

builder. When he got an offer from Connecticut about 1967 or 1968, I, as strongly I could, encouraged Hoebel to see if the dean would meet the offer; but, the response was somewhat tepid. They didn't try very hard to keep Pelto. They didn't say, "Congratulations on your new job;" but at the same time, they didn't try very hard to keep him. He moved on to Connecticut. There weren't major battles over the curriculum. It was very much of a holding company for faculty interests. Everybody was free to do their own thing. Nobody made any efforts to stifle the younger faculty or to tell them they couldn't teach certain things. There was a great deal of academic freedom.

CAC: You mentioned the three ethnic studies programs: African-American, Chicano, and America Indian. Anthropology, in the abstract, would have been a good place to find allies for such programs. You're suggesting that was not the case.

FM: Yes, it was the case . . . in that, I was that. [laughter]

CAC: [unclear] you, right. What was your role?

FM: In those days, Fred Lukermann had been associate dean for the Social Sciences in the College of Liberal Arts when I was selected to be assistant dean of International Programs and had been very helpful to me in orienting me to some of the pitfalls and the skeletons in the closet among the CLA faculty. After all, here I was an assistant dean supposed to work with this faculty and I'd only been here two years and I didn't know all that many people; so, Fred was extremely helpful to me. Then, he became assistant vice-president and he was more or less put in charge of responding to all these student movements: student power, Black Power, Indian Power, etcetera. He recruited me to be chair of the committee to consider whether or not a program or a Department of American-Indian Studies ought to be established. It was just in the aftermath of the black student take-over of Morrill Hall. You are probably interviewing somebody else about that.

CAC: That's right, I have. Say something about the composition of that committee, and how you operated, and what you came up with.

FM: The composition because of the ideology of those days was some faculty, some Native American students, and some people from the local Native American community who were interested in higher education issues. As a matter of fact, the faculty were in a minority; it was deliberately set up that way. I think it was no doubt the same for the committee on African-American Studies. I still have a file on these matters; but, I haven't checked it. I've, frankly, been too busy lately to do much homework for this.

CAC: Sure. Did you find this a traumatic assignment?

FM: No, it wasn't. In a way, this committee on African-American Studies was leading the way because it had been established first after the black students had taken over Morrill Hall. Then,

the Indians said, "We want a piece of the action, too." It was a loaded committee really with people who were sympathetic to the whole effort. It was the Black Studies Committee that said, "We want a department and not a program."

CAC: Right. Kind of carrying that story forward a bit . . . of the three, it's really only African- and Afro-American Studies that build strength, right?

FM: Yes.

CAC: The Chicano effort is pretty slim?

FM: Yes.

CAC: The American Indian . . . did you follow that . . . ?

FM: They've had major problems in keeping good faculty members. I think that's the principal problem there. I want to say something else about that committee.

CAC: Okay.

FM: One of the first things I suggested in our discussions was that I didn't see any reason why the Ojibwe language and possibly also the Dakota language might not be used to satisfy the second language requirement. The Indian students in particular and the Indian community members really picked up on that; we were a united front on that issue. One of the major contributions that whole effort has made has been very much not celebrated in the university, not even sufficiently appreciated, that is, to get those languages courses going to prepare the teaching materials. The credit there goes, in combination, to Timothy Dunnigan, who was hired in that department. He happens to be non-Indian . . . Dunnigan . . . he's Irish background. He was the person that the other faculty who had been hired wanted to work on the language issues because he was trained in linguistics as well as anthropology. He worked with the native speakers for many, many years putting together the teaching materials for both of those languages. I believe we were the first university in the country where those languages could be used to satisfy the regular second language requirement. It wasn't the first place an Indian language was taught. Usually when it was taught, it was taught in an extension program.

CAC: Sure. The program hired native speakers to teach the language courses in both of the communities?

FM: Yes, native speakers taught the courses under Dunnigan's direction.

CAC: As I recall, they were all women, weren't they?

FM: Initially, I think they were. Angeline Northbird was one who did Ojibwe. Was it Clara Barstow in Dakota? More recently, Collins Oakgrove from Red Lake has taught Ojibwe.

CAC: Those two language programs have survived?

FM: They have survived.

CAC: Even though the program as a coherent program has not?

FM: Yes. I could say a word about the staffing problems.

CAC: Please do.

FM: I think it's highly significant. First of all, we wanted faculty with scholarly credentials. We didn't want only militant activists as members of these programs.

CAC: Sure.

FM: We were lucky because the most important senior and most published Native American anthropologist in the country in those days—we thought we were lucky, I should say—was Ed Dossier at the University of Arizona. In fact, Dunnigan had studied with him and he was his main adviser for his Ph.D. at Arizona. Dossier accepted a job here. He was really excited about the program. He liked Hoebel. He liked Spencer. He wanted to be in a really good anthropology department as well as operate this pioneering program. He accepted the job and was going to come in a year because he was on sabbatical from Arizona. Then, within a few months, he was diagnosed with a brain tumor and wasn't able to come.

CAC: Oh, my.

FM: He died within a year or a year and a half, I believe, at a relatively young age . . . I suppose early fifties. That was a personal tragedy for his family and very bad luck for the program. Then, Roger Buffalohead had already been hired as an historian. Dossier wanted Buffalohead; so, we proceeded to appoint Buffalohead and he took over as acting chair. Then, later Russell Thornton, the Cherokee sociologist, was brought here as the chair—another coup for the university. The story I heard directly from him was that he was teaching a course on migration and demography, his speciality in Sociology, and he was teaching about the migration of the Native Americans from Siberia. Some of the militant students raised holy hell and they said, "That's only one theory. You have to teach the other theories about our origins on the eastern seaboard." The next day, Thornton resigned as director of the program. He just had no stomach for dealing with those kinds of political battles. He stayed here in Sociology; but, he then later moved on to Berkeley and I think he's gone to Oklahoma . . . I'm not sure. That was another loss. So, time after time, we had good scholars. Excellent scholars were hired in that program.

CAC: [Gerald] Vizenor was here as a creative person?

FM: Yes, he was. They tended to move on. It's [unclear] demand that the university couldn't keep them and sometimes it was for personal reasons. The most reason case is Allen Kilpatrick. He's an excellent archaeologist and a very pioneering person. He's working in images of power among the Aztec and the Maya and I think also among Mississippian peoples based upon the archaeological record. He's a very excellent scholar with good credentials in anthropology. He was hired in the program and he left, essentially, for personal reasons. I gather his wife just didn't like it here. She wanted to go back to a sunnier place and there may be other reasons, too.

CAC: It's not a large pool nationally.

FM: There's not a large pool and it's very hard to keep . . .

[End of Tape 1, Side 1]

[Tape 1, Side 2]

FM: The next part of the story is that in about 1968, Ad Hoebel announced, "I'm going to step down as chair of the department in a year and, then, I'm going to retire three years later in 1972 and that will give you guys plenty of time to get a replacement for me." So, he would be on the faculty for another four years; but, we'd have to find a replacement as chair. There was a big battle within the department on this point. Bob Spencer saw himself as the natural successor to Hoebel. He'd been here before Hoebel. He came here as a young man in 1948 after teaching a short while at Reed College; so, most of his career had been here. He'd been acting chair a couple of times. Unfortunately, he was seen as a very arbitrary administrator by most of the faculty. We had great respect for his erudition, which surpassed our own by far.

CAC: He was one of the most versatile persons I ever knew.

FM: Yes, incredible, just astonishing; but, he was seen as, basically, really hostile toward anything that smacked of student power or any kind of responses to student demands. He was seen as too much on the conservative side in terms of academic issues and, also on top of that, somewhat arbitrary in the way he made decisions; so, he was never really a viable candidate. Eldon Johnson was another possible successor; but, he wasn't a viable candidate either for other reasons. It came down to, basically, a battle between Richard E.W. Adams, Mayan archaeologist, and me. Adams was pretty much allied in terms of policy issues with Hoebel and Spencer. He was younger than I was. He was an associate professor. I'd been promoted to full professor in 1968 in response to a job offer. I was offered a professorship and chair of the, then, growing department at Penn State in anthropology and turned it down for a variety of reasons. I would have turned it down even if they hadn't promoted me here at that time. I was liking my work in International Programs and very much enjoying living in the Twin Cities. I still do; it's a major attraction that we have in keeping faculty.

CAC: This comes up in almost every interview.

FM: Yes, I'll bet it does. I'm sure you feel that yourself. It became a battle between Adams and me. It wasn't exactly an election; but, the deans made it clear. "Easy" [E.W. Ziebarth] was the dean at the time. Lloyd Lofquist was the associate dean for the Social Sciences and really in charge of doing the groundwork to see that a new chair was selected. We did not have a departmental constitution. As part of this process, Hoebel appointed a constitution writing committee, which was headed by Bob Spencer, to write a constitution. Meanwhile, a chair had to be selected. We didn't have a process for having an election for recommending a candidate to the dean.

CAC: This is now 1968?

FM: It's fall of 1968 and early winter of 1969.

CAC: I raise the chronology for posterity. I know that in many other departments the same thing was going on. It's that move from seeking the old boys' opinion of who would be a good chair, on the part of the dean, to a real consultation, sometimes a vote. This was happening in departments . . . that same year, right?

FM: Yes.

CAC: This is systemic. It's not really peculiar to Anthropology?

FM: Yes, that's important. It's part of the institutional transition [unclear].

CAC: Yes.

FM: What Lofquist did was he interviewed every member of the faculty, face-to-face, in his office—a very time consuming process for him—as to who would be their first choice, second choice, and who would be the least acceptable on the other hand. No vote was ever announced; but, he made his recommendation to the dean that I ought to be offered the job. That's what happened very unexpectedly because I was really expecting that Eldon Johnson would become the chair.

CAC: He was later, wasn't he?

FM: He was later, yes. That was very unexpected. Meanwhile, things were very much winding down in International Programs because the university had been one of the last to get one of these Ford Foundation grants. It was \$1.5 million, which was a lot of money in those days. Indiana had gotten, for their first grant, \$3 million or \$4 million and they had gotten a renewal of several million. I visited Indiana as sort of the leading example of how a Land-Grant university in the Midwest, in a provincial town, Bloomington, Indiana, was a leader in

internationalizing. They really were and they did it partly with Ford money, partly because of the commitment of their president. Minnesota was very late to get in on the act and got a relatively small grant. Then after I was hired, Cochrane gave me the job of preparing proposals for a renewal from the Ford Foundation, number one . . . number two, funding from the International Education Act, which was going to be one of [President] Lyndon Johnson's claims to fame in posterity before he got involved in the war on poverty and was, secondly, distracted by Vietnam. The International Education Act, for which all academic institutions were lobbying, never was funded. It was passed, but never funded with a penny. Shortly after I was given this assignment, within months—we started this priority setting process within the university, which was going well—Ford announced they weren't giving any more of these institutional international grants either. So, there was no big pot of money out there anymore. Cochrane lost interest. He quit and went back to Agricultural Economics. There wasn't much money to work with.

CAC: Then, Bill Wright . . . when the [unclear] thing came in there?

FM: Yes. I decided I was going to go back to Anthropology and concentrate my efforts there. Being offered the chairmanship really confirmed that but that was my inclination anyway—and continue my Mexican research. We were still in the field work phases and I hadn't had time to do much field work. I later wrote a book about it based partly on my own field work and substantially on the work of the very excellent graduate students who wrote their dissertations on the project.

CAC: Say something a bit more then about—you're reasonably young and you hadn't expected to be chair and procedures are changing very rapidly at the departmental level—the internal . . . you speak of constitution writing and then you come in, etcetera.

FM: Bob Spencer, I think, really wanted to write a constitution which would restrict the powers of the chair—which I thought was appropriate, as a matter of fact. We were moving to a new system, a much more allegedly collegial system. The constitution was designed for that kind of a system; although, he was still personally resisting having any students on committees. That was a point on which we had a showdown with me and the younger faculty essentially insisting that there be student members of the graduate committee, not for admissions or not for any decisions about other students. For policy setting, there would be a couple of student members for student input, basically a consultative kind of system, which was pervasive in our college and it was becoming more so, and also on the undergraduate committee. We got student membership into the constitution.

CAC: You spoke informally, before we turned the machine, on about Ad's assistance to you.

FM: Partly because of that, things went very smoothly. I was very careful to consult with him. I had a great deal of respect for him. I liked him. He was a great storyteller. He was a wonderful person. We got along personally very well in spite of some fairly bitter disagreements

about the war in Vietnam, about American Indian Studies, and student power in general; but, essentially, we agreed not to agree. We didn't have any public fights about those matters. He was extremely helpful to me.

CAC: By that time, he was a Regents' professor?

FM: He was a Regents' professor. He was the quintessential elder statesman.

CAC: Yes.

FM: He was very supportive of what I was doing. I was steering a fairly moderate course. Generally, I tend to be a moderate person in my personality anyway; so, in the turmoil of those days, I was steering a fairly moderate course. I was no wild-eyed radical. I was trying to keep all sides together and build consensus and I think I was able to do that fairly successfully with his help.

CAC: You were a department of twelve to fourteen at that time?

FM: When I came here, I made number ten. Then, it continued to expand because the student body was growing in general and the demand for Anthropology was skyrocketing in the late 1960s, as you know.

CAC: Were there pressures to hire women in Anthropology?

FM: There weren't. We were very slow. We were very slow on those lines. I would say there weren't any pressures at that time.

CAC: Not from the college . . . not from the culture?

FM: No, there weren't strangely enough. Looking back it, I've always wondered why there wasn't more pressure. We did hire a very good person, Esther Armeté, who was a Chicago Ph.D. She'd come from Argentina. She had worked in Mexico. She had major visa problems coming here; but, she finally was admitted. Then, for personal reasons, she decided to leave and go back and work in Argentina. The strong pressures which mainly were within the department to hire women came along a bit later.

CAC: Janet Spector was really your first hire?

FM: Yes.

CAC: That's late 1970s or early 1980s maybe?

FM: It might have been before the late 1970s . . . I don't remember the chronology of that. I do remember she was hired a year that I was on sabbatical; but, I was in town. I remember participating in the interview when she was hired.

CAC: I raise the question because in Anthropology, I know, as in other related cultural studies, feminism was on it's way, as in literary studies, in History . . .

FM: Very much so.

CAC: . . . in making a major contribution.

FM: Yes. Nobody was really pushing it within the department until a little bit later. Then, there was a strong move to hire more women.

CAC: Would that have been true in the guild nationally that a concern to seek out and promote women was coming to be a priority?

FM: I would say it was. At the same time, there were lots and lots of excellent women candidates available. That has never been a problem like with American Indian Studies with a shortage of really good scholars. It was a natural process literally so. If your priority really is to hire the best person available in a certain speciality, that person is likely to be a woman or just as likely to be a woman as a man. Depending on the speciality, it varies.

CAC: By this time, had you remarried Cynthia or is that a later story?

FM: That was happening as part of this turmoil, as a matter of fact. We are a politically incorrect couple; so, I'm very happy that the new rules aren't retroactive. I was her adviser. She moved away to Connecticut with her husband and three sons. He was employed in the computer business. Then, she did some part-time graduate work at the University of Connecticut. Her story is interesting, too, because she grew up in St. Paul. Ad Hoebel was her adviser when she was an undergraduate. She got her undergraduate degree in 1956. She had a Social Science undergraduate summer research fellowship to do research on child training in the Prairie Island Dakota community and Hoebel was her adviser for that. She knew him well.

CAC: I raise the question not for personal reasons but I know her to be a strong feminist and that would probably have some influence on the way you looked at decisions.

FM: Yes, it certainly did . . . certainly did. She decided to leave her husband. She came back here. By that time, I was separated from my wife and going through a very bitter divorce. It was really a very difficult time.

CAC: Pretty sapping.

FM: She came back here about February 1969. I had just been selected as chair. Then, we started getting involved with each other. It would have worked out very differently in the present climate.

CAC: Yes.

FM: I had a lot of support from my colleagues. It never became an issue that was troublesome in the department for me, although, certainly troublesome for her. She had been one of the star students here as an undergraduate. It was when her youngest child started in the University Nursery School, in about 1965, that she came back to Graduate School. She was the star student after she got back here. She won a National Science Foundation pre-doctoral fellowship. I think, she is still the only graduate student in the history of our department who won one of those fellowship, so she was a star student; but, she'd put herself into kind of a limbo position as a graduate student who was involved with the new chair of the department.

CAC: Yes.

FM: We had a lot of support from friends.

CAC: I understand; but, this adds to the stressful environment.

FM: [laughter] Yes, adds to the stress. It was another issue I was dealing with; although, in retrospect, it wasn't something that popped into my mind that I would even talk about. We came through it; although, we did put a lot of time and effort into building a new family. I had two daughters from my first marriage who were in their college years and then we decided that we'd have our own joint product. That's Emily, who is now twenty-two. We have this blended family of six very interesting, lively people. It took a lot of effort and emotional energy to work all of that out.

CAC: To persons down the line interested in the culture of higher education, these are relevant factors. I ask them not to pry into your family life, except I know Cynthia and her work so it seemed like a relevant observation.

FM: I really do believe that she suffered because of her association with me in that she wasn't taken as seriously as she would have been otherwise. Especially, some of the senior faculty continued to perceive her and identify her as a part-time student. She had a National Science Foundation fellowship . . . the only student in the department who had one. She was still perceived as a part-time student because she was a mom.

CAC: Ahhh.

FM: Her youngest son, Robin, was eleven when Emily was born. Cynthia was still working on her dissertation. Emily was born in 1972. Robin was eleven, Richard was fourteen, and Peter

was about sixteen. My older daughters were about fourteen and sixteen themselves. They weren't living with us; but, Robin and Richard were living with us. Cynthia had three kids at home, a prestigious fellowship, and she was a full-time student, as well as being a full-time mom.

CAC: Very soon she carves out an independent career for herself as an anthropologist and teacher at Hamline?

FM: She does, yes. That's right. That was very much a matter of luck. Luck figures in this. I got my job at Carleton out of luck. I got my job here out of luck. I got into International Programs out of luck. There's some ability involved, too, I suppose. [laughter]

CAC: You won't be surprised at the number of people out of the thirty-eight I've interviewed who have used the word luck . . . sometimes bad luck but it's most often good.

FM: Emily was born on June 15, 1972, my last day as chair. Then, I had sabbatical. In fact, I'd delayed it for a couple of years. That was a wonderful year and Cynthia was continuing working on her graduate program, working on her dissertation. Then, a job opened up at Hamline. It was the beginning of the shutdown of the job market. A full-time job opened up at Hamline, which had a long tradition of anthropology. Leland Cooper had taught archaeology there for many years. Within the soc[ial science] department, they had one anthropologist and they wanted a new person. In September 1973, Cynthia started that job. Emily was one year old. She didn't have the fellowship anymore; but, still she was working on her dissertation. She was a full-time mom and a full-time professor. That was a very busy time. I wasn't chair anymore; so, I had a bit more leeway in my own life. I was writing a book on Mexico. In fact, the book *Old Villages in a New Town*, I wrote my last year as chair and I wrote almost all of it after 10:00 p.m. I'd write between 10:00 p.m. and 2:00 a.m., which is something I can't do anymore.

CAC: It allowed you to get back to your interests in Mexico and Latin America in International Studies?

FM: Yes, It allowed me to get back to my interest in research. I had not gone back to Chiapas, Mexico, for a number of reasons. I got involved in the project in Central Mexico. Chiapas is basically a long two-day drive from there. We'd been back to Mexico a lot for Cynthia's dissertation on migration in western Mexico. We were doing the field work for that in the summers of 1970 and 1971 and working with a Mexican Research Institute of Survey sociologist who helped us collect the data. We were going to Mexico; but, we didn't go back to Chiapas and partly because, I thought Chiapas had really been overrun by North American anthropologists in a serious way. Harvard, and Columbia, and Illinois started an undergraduate field training program, which was wonderful for the students. In fact, Stephen Gudeman, one of the eminent members of our department, had gone to Chiapas—though I didn't know him in those days—on that program as an undergraduate for his first field training in anthropology. Then, the early Harvard Ph.D.s started their own program. Chicago had a big project. Stanford got one started

and the place was just swarming with North American anthropologists. I thought that was really excessive and I had other things to work on; so, I didn't go back.

CAC: But, you did expand your interests into North/South access?

FM: Yes, I did. All of my work has interdisciplinary. The impact of medicine is in a way. The industrialization project included random surveys of households in seven villages which were conducted by the Mexican sociologists that were working with economists that were involved. That was very interdisciplinary. Then, an anthropologist, Mary Almendorf, who was working for the World Bank as a consultant—they were going to start a big push on water supply and sanitation in the Third World and how to make some progress on those issues in impoverished villages—remembered that I had written an article about the impact of medicine, which had some data about the acceptance of latrines. She read it and she said, "Let's have a restudy." She called me up and the World Bank funded a brief restudy. We spent a month there . . .

CAC: Really public health and sanitation?

FM: Yes . . . beginning just before Christmas in 1977. Emily was five and a half years old. I'd been there by myself the first time. Here, I arrive and I finally am a real human being because people knew I had a wife and a child earlier on; but, all they had seen was pictures. Now, I'm a real human being. I arrived in the village with a wife and our five year old, extremely cute, blond daughter who they very much admired. [laughter]

CAC: We had the same . . . just enchanted . . . we took Sarah, who was then six, to Mexico for four months when I was writing. I wasn't doing research in Mexico itself. My, she was our entree to every village we went to. They just would latch onto this lovely, sweet little girl.

FM: Yes.

CAC: Subsequently, she studied Latin American Studies at Carleton and then at Madison [Wisconsin] and now teaches Latin American history at Chapel Hill.

FM: Fabulous.

CAC: I think that the difference of other cultures open up to her at a very early age and she never lost that excitement. Let's get back to you now.

FM: After twenty years, I finally went back to the village where I had done my first field work. That was a very, very interesting experience. When I had been there before, as I said, I chose this village because it was changing rapidly. It was open to change. It was a very small place of fifty nuclear families grouped into thirty-nine extended families and a total population of 220 people, over half of them under age fifteen; so, you're talking about slightly over 100 adults. I had gotten to know everybody on a first name basis. One of the reflections of the village attitude

is that it was the only village in the whole region where all of the girls were in school. They had a grade school there which gave effectively three years of education and all of the girls were in school. Beyond that, by Indian custom, women were not allowed to participate in the town meeting. It was a Land-Reform community created by the Land-Reform operated under the agrarian code. They had a town assembly that elected their local officials. They kept electing the same head man for twenty years. He was another major influence on me . . . Elan Sovaskas, who had been the leader as a young man in the founding of the community, petitioning the government for land as part of the Land-Reform. He was another remarkable person, an astonishingly remarkable person and I greatly admired him. In fact, he's the one that taught me how to do consensus building politics.

CAC: Say something about that.

FM: It's very much a consensus system. The people say that this true today. I have not actually visited the Zapatistas community; but, reporters that have visited them have reported that they use a Highland Mayan style consensus process. It's very much what we consider the Quaker model where the leadership has some ideas about what they want to do; but, they never have a vote. First of all, they sound people out behind the scenes and especially they sound out the people that might be thought to be opposed to what they want to do. They'll modify their plans accordingly. Then, they talk it over. Then, they bring it up in the meeting and, usually, it goes through after some discussion. Sometimes, on one issue, they'd have a couple meetings of two hours long before the opposition, basically, acquiesces. The opposition doesn't have to agree; but basically, they stop opposing. They agree to stop opposing.

CAC: Isn't this a model—or maybe it's a romantic perception—of many tribal communities and workable?

FM: Yes, it's a working model. It can be unduly romanticized. I think sometimes behind the scenes, the leadership may play what we would call hardball politics to get something through. Basically, you have a system where you don't have an embittered opposition that always ends up losing.

CAC: Congress could learn from that.

FM: [laughter]

CAC: I'm thinking of changes in departmental culture all around the university in the 1970s—this is precisely when you were chair and I was chair—that that kind of model, though we didn't know about it explicitly, we were moving toward it . . . awkwardly or lurching, but doing it.

FM: Yes. It does take somewhat more time. It does take a lot more time laying the groundwork, and talking to people, and to make some compromises even in advance in order for the sake of building a consensus.

CAC: Do you have any sense, as a social scientist, what the optimum size of involved population is where it will work? I'm thinking of departmental cultures. It may work better in a department of eighteen than a department of forty-eight?

FM: Ad Hoebel had a very firm opinion about that. In fact, he was reluctant to grow much beyond ten because he said from his understanding of these matters, if you get much beyond ten, then you start getting factionalism. We had to grow bigger because the students were coming. The positions were available. We eventually grew to be seventeen; gradually, we made it up to seventeen. Now, we've shrunk back to essentially fifteen. We did become more factionalized as we got bigger; but, I think it wasn't a matter of size. It was a matter of the social, cultural turmoil that was going on.

CAC: This might be a good time to reflect on that then. Looking at, let's say, the last twenty years in your department or the college more generally, if you want to talk about that—I'm thinking not in changes of administration and political structure but in changes of departments and what they addressed—how does a department evolve? You have to have new courses. You have to have new staff and you expanded your staff, as you said. How do you go about that? Setting priorities . . . the next person you want to hire would be in *zip*, for example. Can you comment on that?

FM: Our philosophy was always to build on what we had. Sometimes, we weren't able to do that and new interests got added that we didn't have before, notably feminist anthropology. Both Janet Spector and, later, Riv-Ellen Prell were heads of the Women's Studies program while they were in Anthropology. That was an important new interest that got added, in response to, basically, national changes and scholarly trends. But, in other respects we tried to build on what we had and sometimes the interests you get are just an accident of the job market. For example, we ended up with strength in Mexico without ever really planning it very carefully. We already had Richard Adams here, the archaeologist, when I came. Then, I was added. Then, we had, at first, a temporary replacement position and we hired Stephen Gudeman who had done his research in Panama. He had gotten his degree under Edmund Leach at Cambridge . . . one of the few Americans to do that. He later did research in Colombia and is now working in Guatemala. We added him. A year later, we added John Ingham and he had worked in [unclear] in Central Mexico. He was a student of George Foster. At Berkeley, he did psychological anthropology. We had an archaeologist. We had me who was working on change in basically local level politics. We had Gudeman who was working on kinship and economic anthropology and Ingham who was doing psychological anthropology. Later, we added Harvey Sarles who had worked in Chiapas in linguistics.

CAC: I didn't know that.

FM: He later moved off to another department. Esther Armeté, the Argentinean woman that I have mentioned, had also worked in Mexico. We had a really strong Mexican group. It was

partly that we liked the idea; but, those just happened to be strong people. We didn't define the job as Mexico only; it was Latin America. We just happened to end up with the Mexicanists.

CAC: The existence of this cluster of really very competent people . . . that then was an attractive feature for graduate students coming here or wouldn't they know that?

FM: Not as much as you would have thought. It's partly my own attitudes. I feel that Mexico has been overdone. It's not only Chiapas. It's so convenient for North Americans.

CAC: Of course.

FM: Transportation is relatively cheap. Archaeologists need government permits. Ethnographers don't need . . . as a courtesy, you want to touch base with your Mexican colleagues but you don't need any kind of official permit to do research. You can do it on a tourist card. It is so convenient. It's so interesting. It's one of the world's great laboratories of anthropology.

CAC: It has, certainly, one of the greatest museums.

FM: Yes, exactly . . . it certainly does. We never really pushed that we wanted to train more Mexicanists. I think we all tended to feel that the world has enough Mexicanists already—especially, the U.S. has enough Mexicanists.

CAC: Was there a shift in the kind of graduate students that came to Minnesota the last twenty, twenty-five years? Were they seeking different objectives? I'm thinking of graduate education and how it adjusts and changes.

FM: A very strong shift. I'd like to backtrack a minute. I mentioned that I was hired here on the NIH training grant. Most of that money went for student fellowships. Hoebel had gotten the grant. That's what built up our graduate program. Before that, there were very few graduate students. We had a number of eminent people who got their M.A.s here and then went elsewhere to get their Ph.D.s. Elizabeth Colson is one person who comes to mind. She was from north western Minnesota . . . I forget where. She came here as an undergraduate and got her master's degree. She went to England for her Ph.D. The graduate program built up very rapidly in the beginning when they got the grant, about 1963, from just a handful to up to maybe forty students altogether. We had money to offer in those days. We had more TAships [teaching assistantships] also before the TA funds were cut so much; so, we could attract people with international interests. Then, Hoebel got the first grant in the country from NIH that guaranteed \$10,000 a piece, on the average, to the trainees to do their dissertation research. This happened in the late 1960s. That's worth, like, \$35,000 today. That's just incredibly good support in today's context. Richard Nixon, as president, did away with that program . . . totally just closed it down, that is, the program of supporting basic research in the social sciences through the National Institutes of Health and the National Institute of Mental Health mechanism. The Ford Foundation already had pulled out. Ford still gives some money to the Social Science Research

Council for the area fellowships; but, there are very, very few. The funding for international research by anthropology faculty and especially graduate students declined precipitously in the 1970s. Our students made a major switch to studying various aspects of U.S. culture.

CAC: Ah.

FM: This is true nationally.

CAC: You think funding was . . . it wasn't an intellectual . . . ?

FM: It's both. It was both. I'm not familiar with any adequate sort of intellectual institutional or social history of this change within anthropology during those times. It was happening at the faculty level also. It was partly a response to constraints and then partly, it was . . .

CAC: Mr. Gerlach certainly changed his research agenda.

FM: Excellent example. That's a beautiful example because he had done his dissertation research in East Africa. In that case, it was, I think, not a question of funding but a change in intellectual interests. Another thing that I think we tend to forget—in fact, I have not mentioned it until this very moment—is the perception of a great urban crisis in the United States . . .

CAC: Ah, good!

FM: . . . in the mid 1960s with the riots and there was even turmoil in Minneapolis in the mid 1960s in Anthropology. A speciality that we called Urban Anthropology began to emerge. It wasn't limited to the U.S.; but it did, you might say, dignify those kinds of interests in a way that they hadn't even existed before.

CAC: Looking at the 1980s and early 1990s, has the graduate program sustained itself, therefore, in Anthropology even though sources dry up in funding?

FM: It was tough going for awhile and it was certainly reflected in our enrollments, which I think were more a matter of the word getting around that the job market for Ph.D.s was terrible. I haven't looked at the figures; but, I think the low point in terms of the numbers of people admitted to the graduate program, that is the people who matriculated, was maybe six entering students back in the late 1970s or early 1980s. In the mid 1960s, when we were building up—we were really growing then—we were admitting twenty a year. That slowed down. We wanted to have about a four to one student/faculty ratio; so, with sixteen faculty, that was sixty-four students total, including those not here anymore but still working on a dissertation. That was a deliberate policy decision. We used the four-to-one figure—and still do. The number of people who actually came declined to about six. Now, it has skyrocketed again. Last year, we had sixteen. This year, we've got eleven or twelve.

CAC: How do you account for that?

FM: I think it's just cultural changes in the society. The job market is not looking very good again; but, for awhile, there was a lot of hoopla about all of these people who are going to retire and there will be, yes, many more jobs. It hasn't happened as much as expected and partly due to all of these cutbacks in higher education.

CAC: People retire and then . . .

FM: They're not replaced, yes. I think what I want to emphasize—I tend get wrapped up in sort of the local institutional details—that what happened in anthropology as a discipline internationally, especially in the United States, was in a response to the changing social circumstances. Another very important factor, which I haven't mentioned, is the controversy over the war in Vietnam and the continuing controversy over the international role of the United States and the opinion, in some quarters, that anthropological research is essentially exploitation.

CAC: Cultural imperialism?

FM: Cultural imperialism. You go off to the foreign country, you mine it for data, and you take the profits home and that country doesn't benefit . . . and major attacks on the complicity of anthropology in British colonialism, in particular . . .

CAC: Yes, yes.

FM: . . . not so much in the United States. I think some of those attacks are overdrawn but to some extent anthropologists did work within the colonial context. The British social anthropologist almost never worked outside of a British colony. They went where it was easy to do research. That was what the funding was for. I don't think they were propping up colonialism actively with their research. I think it would have continued as long as it was going to continue without any help from anthropology; and it would collapse of its own weight because of all the changes going on without much role of anthropology. That's the intellectual context. The rise of dependency's theory in Latin America which swept this to some of the social sciences in this country and anthropologists decided to retreat back home.

CAC: You may correct my next observation, which is a paradoxical one. I'm thinking of disciplines that I know well; I know anthropology not that well but a little bit. Work in Third World countries frequently is a radicalizing experience that is, they become anti-colonial, anti-imperialist with a real vengeance when they come home.

FM: Yes. [laughter]

CAC: Rather than being [unclear], they fly to the other extreme.

FM: Yes, I agree with that.

CAC: Say something about the growth of attractiveness in anthropology as an undergraduate major over the same twenty-five year period. Has that had waves of change?

FM: Yes, from boom to bust to boom again.

CAC: Do you account for that by the same . . . the financial would not be there?

FM: The interest fell maybe more than it should have, you might put it that way.

[End of Tape 1, Side 2]

[Tape 2, Side 1]

CAC: . . . changes of popularity in undergraduate majors?

FM: The number of majors went down to well below 100 and, now, it's been steadily climbing for about ten years and it's up to, like, 210 or more. That includes a major contingent of people primarily interested in archaeology. We're shorthanded in Archaeology in the graduate program and because of that, we don't have a lot of graduate students in Archaeology—we have some very good ones. There's also the interdisciplinary program. It used to be Ancient Studies and now it's renamed Interdisciplinary Archaeological Studies. That's closely associated with our department now. That's being rebuilt now after a low point. From the undergraduates, I think it's a renewed interest in the rest of the world. We went through this period of the cultural revolutionary movements of the 1960s in the United States, the romanticization of the American Indian culture. I think that's still going on in many respects. That pulled undergraduates into Anthropology back in the 1960s and early 1970s. Then, the bubble was burst, so to speak, and the interest declined dramatically. Now, it's built up again.

CAC: And now, in a context of culture pluralism, which is a more authentic and less romantic [unclear]?

FM: Yes, I think so. Also, I think, in some quarters, a renewed interest in the possibilities of some kind of positive cultural change or social reform, although in a context of much greater pessimism about those kinds of issues.

CAC: Ah. You raise a related issue that other people have raised. Someone spoke the other day to me on tape about the melancholy of the professoriate.

FM: [laughter] Yes.

CAC: It's, in part, as this person was describing it, lower morale with lower money and fewer funds and frustrations administratively and bureaucratically. I think really, it's a term of cultural power that there is this disappointment and, perhaps, it's part of the aging . . . You're younger than I; but, it's our generation who knew the 1960s and 1970s and now it doesn't seem so possible.

FM: Yes. I have something to say about that.

CAC: Please do.

FM: I have very strong opinions. As I said earlier, I got one of these Ford Foundation behavioral science fellowships. The rationale behind that, which had been put together by Francis Sutton, who was a sociologist who worked for the Ford Foundation in those days, was that, sure, the quantification of the social sciences would give us the basis for solving social problems. There was a great deal of optimism. I shared that optimism as a college student; although, I didn't share the faith in quantification. I thought a limited amount of additional quantification would be a useful tool for ethnography but not a panacea and certainly not a way to solve social problems. I think it is very, very clear that the grand promises of what the social sciences were going to accomplish have been severely disappointed. They just didn't come through . . . especially the more enthusiastic advocates of, what you might call, positivistic social science, taking natural science as a model in political science. Economics is kind of a special case. It ought to be an object lesson to us that allegedly the most scientific of the social sciences, the economists, wildly disagree, even so, depending on what kind of theoretical orientation they're starting from. If they can't fine tune the economy, how can we possibly fine tune the polity or the society for that matter? I really share in that disillusion; although, I never was a wild-eyed optimist. I still think that concrete anthropological knowledge is useful for improving the way programs, whether they're public sector or private sector, operate. To improve the performance of your development programs in Mexico, they work better if they're designed by anthropologists. It's a tragedy, I think, that that development program, operated by the National Indian Institute, expanded too much, in a way. It was taken to the whole rest of the country. Then, in the economic crisis in 1982, when Mexico couldn't pay its interest on its debt anymore, the funding declined precipitously and it has almost been forgotten. In the press accounts—some of them are very good and fairly deep—about the Zapatistas rebellion, I have not seen that development program mentioned once. In fact, this is the article I'm working on right now.

CAC: Good.

FM: There was a great deal of optimism there. The government was finally doing something to benefit Indians. They were building clinics. They were making modern medicine available. The infant mortality rate plummeted because antibiotics were curing pneumonia. They had smallpox vaccination . . . polio, measles, etcetera. It had a major impact. It also created major population growth. Education was more available. I never did follow up on what I was saying

about going back in 1977. All the girls had been in school but the women didn't participate in the town meeting unless they were widows. When I went back, one of the young women from the village was a teacher working in a different Indian community. Opportunities vastly increased for the Indian population to work their way up. There was social mobility. True, it only benefits those who are able to take advantage of it for whatever reason. It doesn't raise the average level of the impoverished Indian peasants; but, it does create many new opportunities, especially for Indian women. I have lots of friends who are of the upperly mobile families from that village who have moved to the city. One family has eight kids and four of them are teachers.

CAC: One looks at American Indian tribal government, for example, and it's just in the last fifteen years that one sees women being spokespersons and being elected to tribal positions.

FM: Yes. Anthropological knowledge can be useful; but, it's certainly no panacea and I think the social sciences were oversold and they've disappointed the people who used to pour so much money into them. We're in a very different world now.

CAC: How much of it is also—there's an implicit judgment in my question—a disappointment with anthropologists who did work in Africa at the difficulty of modernizing and establishing black run African nations?

FM: I don't have a lot of contact with Africanists myself; so, I can't . . .

CAC: Since we've lost Jim Gibbs that there's not been an influence here?

FM: Yes, right. He hasn't been all that active in research. He was a dean of undergraduate study. I've kept in contact with Jim. He was chair of the department just recently. A year and a half ago, he had an operation for prostate cancer and is doing fine; so, that's good. I would imagine there would be a tremendous amount of disillusionment about the difficulties of nation building and the difficulties of institutionalizing of government that's something other than a military dictatorship.

CAC: I get a sense also—you correct me—that a younger generation of anthropologists and other related social scientists were very hopeful for China and a real radical possibility there?

FM: Yes.

CAC: And that goes sour?

FM: Yes, I think so. You can see that in the literature.

CAC: In that context, I want to . . . from your vita, which is a long and distinguished one

. . . "1983: Fragmented Universities in an Integrated World." A lot of our colleagues, whom I've talked with, talk about fragmentation. What did you mean in that short essay piece?

FM: That was more along the theme of . . . the world is organized into nations and the university is organized into departments and most of the most intractable problems don't respect either set boundaries.

CAC: Ahhh!

FM: That was the theme of that short piece. It was, basically, a plea for more interdisciplinary research.

CAC: Okay.

FM: Now, there's a new push for that I noticed in the Graduate School. I've been kind of out touch because I've been on sabbatical this past year.

CAC: I wanted to ask you about that not for the last year but for the last twenty-five years . . . whether you think specialization has fragmented the university community in many departments and the degree to which that has been compensated for by cross-disciplinary and interdisciplinary programs in the college but at the university level as well. Do you have any sense to make a judgment on that?

FM: I think that it's harder to have an intellectual community; but, in a big place like this, maybe there never was much of one. One of the great joys of teaching for five years at Carleton was that I got to know very well people across the board. I had close friends in English, in Biology, in Physics, and continuing in Mathematics . . . some interest in Psychology, in all fields and there was a lot more interchange. There was a very strong sense of intellectual . . .

CAC: Common enterprise?

FM: Yes, a common enterprise. You have that within your department here or within your wing of your department; but, it doesn't go much beyond that. The interdisciplinary things are just so hard to sustain because the budget is structured around departments.

CAC: It's a real structural problem?

FM: It takes extra money. It takes extra time. I've spent a lot of time working on it. I've done a lot of things I didn't even mention today . . . one of which was try to get more—coming out of my early experience in International Programs, and working with Will Cochrane, and learning a lot from him about the St. Paul campus—interaction between the agricultural scientists and social scientists. Nothing much came of it. I put a lot of time into it. I had a lot of good friends in St. Paul, which I value and I still see, but in terms of institutionalizing any interests,

the barriers of distance, and departmental barriers, and the time constraints, and the fact that there is no reward for it within the system . . . There's only a matter of personal satisfaction—which is the most important thing. I think another related problem is with the renewed emphasis on undergraduate education, which is fine, with a renewed pressure to be more productive scholars, which I feel myself. We're supposed to do better teaching, we're supposed to spend more time with students, we're supposed to do more research, we're supposed to publish more than ever before. I'm working harder than I ever have in my life and I've been working hard all these years.

CAC: I'm going to make a statement that the rewards are more clearly discernable for the research and publication.

FM: They certainly are . . . certainly are. That continues to be true in spite of all the rhetoric about undergraduate education. Personally, I have pulled back. I'm on more Ph.D. committees outside the department than anybody in my department; I'm on far more. I've just finally started saying, "No, I just don't have time to do it." I'm expected to do more research. I need to spend more time writing and I'm spending more time on my teaching. I think it's a real problem within the institution. It's one of the costs of the shrinking faculty that's not adequately recognized perhaps. The students seem to be more and more desperate to get outside members on their committees.

CAC: I've been retired for four years. I'm on still eight or ten second readers in various . . . American Studies [unclear] and so forth and hell! I've been retired for four years.

FM: That's amazing, yes.

CAC: I'm going to put the same question in a slightly different way. This line of questioning arises out of other interviews I've done. I'm trying to check whether it's generally the case or not. With your teaching and your scholarly interests—they do reflect each other in any good career—whom do you talk with about things that are really central, crucial to your teaching/learning role in the university? Where do you find comradeship in that sense?

FM: Since I've been on sabbatical, I've been deliberately out of touch.

CAC: This is over the twenty-five year period.

FM: I've been so much involved in things outside the department. I haven't even mentioned the three years I served as acting director of the Institute of International Studies with a lovely office in this building up on the twelfth floor. That corner office that looks down the river was the office in those days; that was 1982 to 1985. It was notably more difficult then to get people involved in interdisciplinary things because they didn't have the time. I did a fair amount of work with Hans Gregersen and Al[en] Lundgren and others in Natural Resources, especially with economists in the Forest Resources Department, who have an excellent group of students, some

of whom have undergraduate majors in Anthropology and are working on sustainable development. It is a very well-funded program. I kind of ran out of time to work on that when I became chair. I served as chair again from—also unexpectedly—1989 to 1993. I didn't expect to be elected again. Those were hard times. We were fighting the budget battles and my energy was going into trying to . . .

CAC: You bet. All those things sap time and energy.

FM: Yes. I don't spend as much time as I would like to talking to my colleagues about theoretical issues in anthropology and certainly not about teaching. There just isn't the time to do it.

CAC: Would you with colleagues of like interest at other institutions? I'm talking about the national guild of Anthropologists. Are you able to stay in touch with a certain number of them?

FM: I do stay in touch; but, time is limited. I'm not hooked up to electronic mail. I'm going to get on E-mail soon and, then, that will really facilitate that process. I tend to like the telephone if I can't talk face-to-face with somebody. I prefer face-to-face. I tend to use the telephone.

CAC: You will detect where this question is coming from . . . that many people report that their significant intellectual connections are with their own subset of the guild nationally, whatever it is—microbiology or *zip*—than with colleagues even in the same department. I'm testing that.

FM: I would hasten to say that I've learned a lot from people in other departments. I learned a great deal from Vern Ruttan in Agricultural Economics . . .

CAC: Oh, my, yes . . . a towering figure.

FM: . . . with his views about induced innovation. That's certainly a way of looking at the world, a way of looking at changes that is still unfamiliar to most anthropologists—and certainly worth thinking about. Phil Raup is another towering figure in that department. He's sort of a Bob Spencer sort in being extremely knowledgeable about lots of different things.

CAC: The price of corn in Kansas and . . .

FM: Yes, and I've learned a lot from Hans Gregersen, who is a very broad-minded economist in the School of Natural Resources. He's very interested in anthropological approaches.

CAC: But, I hear a frustration in your career in being able to maintain those kinds of intellectual connections?

FM: Yes. Yes.

CAC: You and Cynthia must share a lot at home?

FM: We share a lot, yes. We talk a lot to each other. That's my most significant intellectual connection, as well as personal connection. One thing that I have noticed very dramatically . . . I did mention the last year I served as chair previously, 1971, 1972, when Cynthia was pregnant with Emily that I wrote that book. It was a short book but I wrote that book about the new town in Mexico, industrialization in the new town mainly between 10:00 p.m. and 2:00 a.m. Last night, I was working until midnight but I seldom work after 10:00 p.m. anymore. I don't feel I should have to. My work week is shorter because it ends earlier in the evening than it used to.

CAC: It's part of the aging process.

FM: Sure. I don't feel the melancholy that some people do. I think the funding problems in the university are increasingly taking their toll. There's a death by a thousand pecks. It certainly shows up in many ways that aren't very visible; but, I think it's really sapping the vitality of the institution. But, in my own work, I have a great deal of enthusiasm. I'm back in Chiapas and focusing on the continuing conversion to Protestantism, something very few anthropologists have studied there. I find that fascinating. The Zapatistas rebellion is another. My own research is very much in response to what happens in the world. I study contemporary change while it's happening. Conveniently, important new things keep happening such as the Zapatistas rebellion to make me have to try to figure out what it all means.

CAC: It may be wise to end on that more affirming note. Before I do, are there other things that we haven't touched upon that you would like to share, having in mind people who will listen to this five, ten, twenty years from now?

FM: [sigh] I'm very much talked out so nothing new pops into my head at the moment. I'm sure it will later in the day.

CAC: We've covered a wide variety of subjects and it's been very informative. In posterity's name, I thank you very much.

FM: You're very welcome. It's been great fun. Thank you.

[End of Tape 1, Side 1]

[End of the Interview]

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