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The following is an unrehearsed interview with John W. Bennett for the University of Kentucky Libraries Society for Applied Anthropology Oral History Project. The interview was conducted by Robert L. Canfield on July 18th and 22nd, 2002 in St. Louis, Missouri.

[An Interview with John W. Bennett]

CANFIELD: Hello. Hello. There we are. There we are. We're . . . we're getting something now. All right. This is Bob Canfield and I am talking with John W. Bennett, and we are discussing his involvement in applied anthropology, and we will be . . . and we're meeting in my office at eleven o'clock in the morning on July 19th. I guess, there we go.

BENNETT: How was that?

CANFIELD: A little bit more. Say . . . let's just say a little bit more, John. We're okay.

BENNETT: Oh, way . . .

CANFIELD: Yeah, all right.

BENNETT: All right. We're talking about applied anthropology, and I hope also, action anthropology, which in some ways was more interesting. How did it come through?

CANFIELD: That's it. We're . . . we're on. That's it. Let's go. I . . . you remember Van Willigen's original question was, how did you get started in . . . in applied anthropology? And we . . . we'll go on from there.

BENNETT: Is that . . . did he actually use those words?

CANFIELD: He actually asked us . . .

BENNETT: Well, you see, my problem is I never had a consciousness of being an applied anthropologist, nor did I feel that I was getting started in applied anthropology. I just did what was necessary at the time. I would like to point out that the 1940s were a period of war . . . a time of war, and at the University of Chicago it was assumed, as a matter of fact . . . taken as a matter of fact, that graduate students in the social sciences would eventually become involved in the war in some way or another. We were all antifascist . . . I mean anti-Hitler I should say, so that it was taken for granted and that we would do some kind of war work. Now, which . . . what I'm try . . . what I seem to be implying is that war work is applied anthropology, but I want to say that we didn't conceive of it as such. We conceived of it . . . conceived of it as useful work related to the . . . the world situation which was in [inaudible], and also useful work to defend democracy against the European fascists and sooner or later, although this was a more quixotic attitude, against the European communists. But that's another story. At any rate, the work I did was directly related to the war. I can remember that when . . . when the war started in 1939, I think the first thing that I did . . . well, Herb . . . Herb Passin and I were together on this, and Passin and I . . . the first thing we did was go to Washington and see what was available in the way of wartime work. Now, we had intro-
...

CANFIELD: What year was this?

BENNETT: We're talking about 19- . . . when the war . . . 1939, the fall and winter of 1939. Well, winter of 1939. Actually, the trip we took to Washington may have been early spring 1940, you see, but the point was we didn't waste any time and nobody else at Chicago in the graduate field in social sciences wasted any time. Not everybody did what we did and so on. We were encouraged to do this by Lloyd Warner. Lloyd Warner was, of course, the Professor – to use his . . . the title that he preferred – Professor of Comparative Institutions at . . . in the Department of Anthropology. He had been trained at Harvard by the Harvard Business School crowd that were interdigitated with anthropology at that time, and his consciousness was, I think, the same consciousness that I eventually adopted. You don't really define anything as applied or pure; you simply do what's . . . what's important. If it has theoretic- . . . it should have theoretical implications, but . . . but you do . . . also should have practical implications. It . . . it . . . it should be carrying out something useful and . . . for society, you see. In other words, we didn't have a clear-cut concept of applied anthropology. I think Lloyd Warner probably did, but the point was his influence was more ideological than it was professional. And so Passin and I had been working with Lloyd on and off, and as we considered his course in Comparative Institutions to be probably the most important course in the department next to Redfield's course called the Folk Society, which was miles apart from Warner. But that was the . . . that was the great thing about Chicago; you had every conceivable shade of proto ideo- . . . ideology as well as every conceivable shade of anthropological theory represented there at that time. You must remember that Chicago was a unique environment during these years. This was the Hutchens-Adler era at Chicago, the era of

the great books and the era . . . in other words, of all sorts of innovations in the teaching of social science and history and so on. This was the era of the famous College of Chicago, capital “C.” The undergraduate program had been bifurcated, taken away from the graduate program. The graduate program was the university, but the undergraduate program was the college and had a separate teaching staff. Eventually there was overlap between the two teaching staffs, but for a time there, undergraduate teaching was done by people with . . . even some had master’s degrees. It was a question of their articulateness as teachers that was important, whereas the graduate faculty, the . . . these were scholars, guys that write books and so on, you see. So that it . . . we were influenced at Chicago by . . . by . . . by great minds, let’s put it that way, and to some extent by great books. Even in the anthropology department at Chicago we were encouraged to take courses in statistics – this was the early „40s. We were encouraged to take courses in political science and economics, and we were encouraged to explore other social sciences other than an-. . . anthropology, and we could get credit in our course program in anthropology for these courses. We didn’t . . . we didn’t have to take them on our own, so to speak. We could add them to our . . . to our curriculum . . . our official curriculum as our . . . our . . . advisors conceived of it, you see. So this was an atmosphere in which social science was . . . was re-. . . in the first place, there was a concept of social science. This . . . Chicago probably had the first formal official use of the term “social science.” It was the social science division or the Division of Social Sciences at Chicago, and that included everything including anthropology. And the dean of social . . . of the social sciences for many years was an anthropologist, Robert Redfield. It didn’t matter what kind of a social science scientist you were. The point is, it was social science, and social science was

conceived by most people and most faculty as a mixture of . . . of relevance and also theoretical stuff. Relevant . . . I'm not using the word "applied" now, you see? I'm using the word "relevant." In other words, what you do is relevant to society. You don't apply what you know, which is . . . see . . . well, you do what you think is necessary to do and it has full theoretical and application, you see, in . . . in . . . inherent in the way you do it and what you do, you see. Now, this, I think, is terribly important because this mindset, this attitude, stuck with me the rest of my life. That is, no matter what I did, whether it was defined by outsiders as applied or theoretical, I regarded it as social science. That's what I was doing, you see. I didn't . . . I didn't discriminate, you know, and that also counts for something else. I think everything I did that would be classified by somebody as applied anthropology also had theoretical interest, and I've been reminded of that many, many times, you see. The fact that my work can't be stuck . . . can't be put in either applied or theoretical because it has implications and aspects of both.

CANFIELD: Okay.

BENNETT: Now, this attitude developed at the University of Chicago, there's no question about it. Now the interesting thing is that we were aware in the courses that we took from various anthropologists and sociologists at Chicago, we were aware of something out there at Harvard called applied anthropology. And then in the early 194-. . . I mean 1941, the organization was founded and they started a journal called *Applied Anthropology*, and I can remember that we regarded this as a kind of cult formation. That is, I think our attitude was, "Well, so what's all the fuss about?" It should be applied, you know; it should have some mix . . . do something that has relevance for the world, for . . . for men, for humans, you know. Now, the interesting thing is that Lloyd Warner, I think,

had the same attitude which, I think, tells you something again about Harvard. You see, the . . . the genesis of applied anthropology at Harvard was really something much more general than, I think, it became at . . . in later years. In later years, applied anthropology began to emerge as some kind of a field, a special field, and some people like . . . like with . . . like Laura Thompson, for example, tried to make a discipline out of it which never worked because it's so heterogeneous in the principals and ideas that are brought to bear on things that you can't possibly make a discipline out of it, you see. Well, . . . but she tried to. This is in the 1950s though, ten years after the period I've been . . . I've been talking about now. I think that people at Harvard in the very beginning saw the applied anthropology that they . . . they were using that term in a very different way from the way it was . . . it was conceived in later years. See, here was the thing. The ultimate disciplinary origin . . . origins or academic . . . let's call it academic origins of applied anthropology were in the Harvard Business School. Now, what was going on in the business school to warrant this? You had a number of people at the business school, Eliot Chapple and this guy, Roethlisberger, and Dixon and other people who were enamored in social science. They felt that . . . that business and economics should have a sociological or a social dimension. This was their point of view and this was expounded in a whole series of books and research monographs by the people at that . . . in the Harvard Business School. And some of these became very famous. That is, they . . . they influenced the . . . the . . . the academic standing and the . . . and the academic structure of business schools all over the country for a while, anyway, until economics began to reassert itself as the dominant discipline for business. But they were conceiving of . . . in other words, they were conceiving of . . . of research, economics and . . . and

business organization – particularly business organization – as a kind of social science. And this was implemented in the famous General Electric Study. The General Electric Study was the chef d’oeuvre of these people, and Chapple was, of course, the principal author of that study. Eliot Chapple regarded himself as an anthropologist, and Eliot Chapple represented to these . . . to the eco-. . . economists and business people in the Harvard Business School as a kind of social scientist. In other words, the word anthropology really in use at that time in the business school meant social science. It didn’t necessarily mean what you and I would’ve called anthropology, the study of tribal people. But on the other hand, at Harvard, anthropology did exist in the pure form in the Peabody Museum. Now, if you were going to patronize it, if you were going to bring anthropology into the business school as they were doing, it would have to be in a modified form or they would have to also acknowledge the existence of another kind of anthropology, namely ethnology and so on, you see – the Peabody Museum, another department of Harvard – so that you had kind of a hybrid situation emerging. I call it simply the Interdisciplinary Era. The Interdisciplinary Era meant that anthropology was combined with sociology and economics and even business organization, and that is the attitude and the methodology that was translated into research in the war, you see. In other words, people like Lloyd Warner had been imbued with this kind of interdisciplinary attitude. He could call himself an anthropologist with a clear conscience, not only because of this fusion of social science principals that could be named anything, sociology or social science or anthropology. But to please the Peabody people, he did a study of the Maranganji, an Australian tribe; about as primitive a bunch of humans as you can get, you see. In other words, that was his accreditation. He never

did anything else in that field. Then he went to Yankee City, see, and Warner became the author of four volumes of research on an . . . an American community . . . American industrial and seaport community in New England, the famous Yankee City Study which was [inaudible], Massachusetts, as you know. That profoundly influenced all of social science at that time. Now, this was a . . . a . . . I . . . this was an expression of the attitudes of that time. Now, it . . . in other words, the . . . the attitude that I'm talking about, this sort of interdisciplinary attitude, you don't discriminate among the social sciences, you just do them. You can call them what you want but you do them, you see. And I think that I certainly got this attitude at Chicago. I got it from Warner, but I got it from the general atmosphere. Chicago was moving in the same direction in the social sciences as Harvard was. I think it was something going on all over . . . in all the major universities. I don't know about Berkeley. Berkeley didn't really come on scene until ten years later, as I remember right. It was not much in the ,40s and ,50s, it was about the middle of the ,50s, I think, that Berkeley began to expand, you know, and become kind of a . . . well, one of the three or four major American universities. Well, . . . so this . . . so I'm . . . when . . . what I am trying to say is, yes, I got involved in what . . . what people later in the ,50s and ,60s called "applied anthropology," but not because it was applied anthropology, because it was an in- . . . was interdisciplinary social science, and I felt that was the only kind of social science to do, okay?

CANFIELD: Now, John, what was the connection, then, to your . . . to the Southern Illinois Food Ways Project?

BENNETT: Well, that was an example of what I've just been talking about. See, Margaret Mead was made chairman of the committee on food habits, which was a

National Research Council Committee, and it was founded . . . it was set up deliberately by people in the National . . . in the . . . in the National Research Council because it was assumed that when the war started . . . when we got into the war we'd have to ration food. Now, it never happened. We never had to, but the point was that if you're going to have to ration food, you better find out what . . . what kind of preferences people have in order not to . . . in order not to have trouble or not to limit people's taste or consumption or whatever. So a program was set up and . . . to do research on food habits. There had been some ancestry of that in British social anthropology. See, that's where . . . that's where the . . . the first food habit studies were not done in the U.S., they were done in Britain, in Africa, you see.

CANFIELD: Now, this is the Audrey Richard's . . .

BENNETT: Yeah, Audrey Richard's stuff. And there were . . . and . . . and I think she did two monographs. Now, this . . . Margaret Mead was very much enamored of this stuff, and Margaret Mead saw all these avant-garde . . . Margaret Mead was not . . . let me insert something here. See, Margaret Mead was certainly not a member of the Harvard Business [chuckling] School.

CANFIELD: Right. Right.

BENNETT: So she didn't emerge, you know, in this interdisciplinary atmosphere the way Eliot Chapple and Lloyd Warner and those guys did, see. But Margaret Mead, throughout her entire career, always tried to grab the limelight. She always wanted to be associated with the most avant-garde ideas and practices in the discipline, you see. So Margaret Mead grabbed into applied anthropology right from the beginning, whatever it was being called then, you see, and there were other . . . other people involved there.

Another way to put it is, it was sort of “Washington Anthropology,” you see; they’re the . . . that was another version of this interdisciplinary attitude. I’ve been emphasizing the Harvard Business School but, actually, you have the . . . the same attitudes growing up in Washington because of the war . . . the coming war, you see. And Margaret was very much involved with a lot of these people in Washington who wanted to make use . . . the New Deal crowd. They wanted to make use of the social sciences to perform the kind of social transformations they were interested in. For example, the U.S. Department of Agriculture under Henry Wallace had a Division of Farm Economics and something in social something – I forget the exact title of it – with M.L. Wilson as the chairman, and that was another one of these cases of applied social science, or rather interdisciplinary social science, and whole groups of anthropologists worked for that division. This was well before the war. This is the late ‘30s now, you see. In other words, you had interdisciplinary social science frequently with a strong anthropological coloring functioning in the late 1930s. It wasn’t called applied anthropology at that time. It was simply, again, interdisciplinary social science, you see, and Ed [Mole?] is an example of that. Ed [Mole?] went on into the war, and when Herb Passin and I were involved in food habits studies and, above all, when we were involved with . . . with public attitudes studies to wartime issues – that’s a later thing; that’s 1943 now, I think, ‘42 or ‘43 . . . ‘46 . . . ‘45--Ed [Mole?] was one of our colleagues, and he had graduated . . . he had come up through the ranks from the, again, Division of Farm Economics under M.L. Wilson, but he had a degree in anthropology, oh, hell, probably from Pennsylvania. I’m not sure just where, you see. Now, Washington at this time was full of people like this, you see; like us, you know. We’d had . . . we’d been in . . . we’d been in universities, we’d been in

departments, departments of sociology, departments of anthropology, departments of economics, departments of social work and so on, but we had all been doing pretty much the same thing because it was implemented by the war and by the New Deal, you see.

That is the New Deal was a very important influence on promoting this kind of research and scholarship, you see, in those days. And . . . and then you had our flyers like Hutch, Hutchens and Adler, you see, in Chicago who are promoting some of the same ideas. At . . . at Harvard you had another . . . another development. You see, you had [Talke]

Parsons who was introducing systematic sociology and incorporating anthropology in that through the person of Lloyd Warner. Lloyd Wa-. . . I . . . pardon me, Clyde Kluckhohn, and Clyde Kluckhohn joined the ranks, eventually, of applied anthropology in somewhat later years. That is, again, he was not part of that business school crowd at all. He was a Peabody Museum man, but he was working with the Navajo and he was advising the U.S. Department of Indian Affairs what should be done about the Navajo, see. In other words, he was doing, again, work in the sphere of human affairs, and not only that, but he immediately discovered that it had to be interdisciplinary. I mean you can't advise on how . . . on . . . on how to improve Navajo shepherding if you don't know anything about sheep. So you've got to get involved with agriculture, you see.

CANFIELD: Right. Right.

BENNETT: That's what happened to a lot of these people, you see. As soon as they started transforming their concepts and objectives for research from theoret-. . . purely theoretical or intra-disciplinary concerns to concerns about the real world, they then found it necessary to engage in much more disciplinary activity, you see.

CANFIELD: Broad disciplinary [inaudible].

BENNETT: Now, I . . . I can tell you this much. Come the 1950s or ‚60s, one of the reasons I gradually reduced my interest in applied anthropology was because it got less interdisciplinary.

CANFIELD: Umhmm. Okay.

BENNETT: In other words, I found that they were . . . that there was sort of a . . . a . . . a . . . a . . . a tendency toward ign-. . . ignorance developing in the field that they were . . . they were inventing principals. They were not formally involving themselves in tr-. . . the kind of training that was needed really to . . . to do practical work, although in some cases they did and in some cases . . . well, as a matter of fact, during that – I’m talking now about the period of the ‚60s when a number of departments like South Florida, for example, got involved with applied anthropology and they tried to make applied anthropology the . . . the . . . the burden of the whole program.

CANFIELD: Umhmm.

BENNETT: My criticism of those people was very simple: they didn’t know what the hell they were doing. In other words, there wasn’t enough training going on . . . there wasn’t enough formal training going on to justify that kind of activity. You couldn’t give a person a Ph.D. in Applied Anthropology unless he had an equivalent Ph.D. and perhaps three or four master’s as well in all sorts of . . . of . . . of real . . . real world fields, you know, like agriculture or mechanics or whatever, you see.

CANFIELD: Right. Right.

BENNETT: I mean the trouble . . . so that as soon as applied anthropology . . . well, now, this is another story, you see. That is, what happened to applied anthropology after the war? During the war, applied anthropology was simply . . . well, it started, as I’ve

said, in a sense at the Harvard Business School, but then it became an interdisciplinary outlook field and that meant that all kinds of people in Washington began joining forces and so on, you see. Now, after the war, I think the problem was, in the 1950s, you had the kind of inversion of anthropological ideas, a kind of involution as a . . . as a colleague . . . colleague of ours right here once called it, Eddy Robbins.

CANFIELD: Right.

BENNETT: He called it involution, an involutory period of anthropological theory. In other words, they lost interest in the real world and they began fooling around with culture, you see.

[End of Tape #1, Side #1]

[Begin Tape #1, Side #2]

CANFIELD: Okay. John, [inaudible] have you . . . if you have anything more to say about that go ahead with it, but I have a new question.

BENNETT: All right, shoot the question.

CANFIELD: All right. Tell me about the Japan experience. How did you get into the Japanese . . .

BENNETT: Well, as far as I am concerned, that was just a continuation of what I've been talking about, you see.

CANFIELD: Okay.

BENNETT: I mean, remember I said Herb Passin and I went to Washington to get jobs. Okay, we got several jobs. The first job we got was public opinion analysis. Well, we had a . . . that . . . we got a job with Rensis Likert, the most distinguished social . . . social psychologist working for the Department of Agriculture who had something called the Division of World Surveys. No, that wasn't quite it, but something like that.

CANFIELD: Umhmm.

BENNETT: And that was started by Henry Wallace and M.L. Wilson as a way of finding out how farmers were reacting to New Deal agricultural programs, you see. So . . . and Rensis Likert was . . . had been trained at the University of Michigan as the number one public attitude . . . public opinion analyst in the country, you see, . . .

CANFIELD: Hmm, okay.

BENNETT: . . . and he had been deputized, then, to take his . . . create . . . recreate his whole division as a wartime operation. That is, to poll the American people on their attitudes toward the war and American possible involvement in the war. And the first job we got was with him. He . . . he welcomed Chicago people because he knew that Chicago people had been through broad training, you see, and they were not set in their ways. They were flexible. They could participate as social . . . general social scientists or whatever they wanted to be, you see. So we got . . . Herb Passin and I got jobs right away, and for a whole year, really a year and a half, Passin and I were running all over the country doing poll . . . doing attitude surveys, you see. And . . .

CANFIELD: Hmm. Was it mainly rural . . . these are rural . . .

BENNETT: Yeah. Rur- . . . ma- . . . well, many of them were rural but many were urban.

CANFIELD: I see.

BENNETT: One of the most dramatic sample points – these were called sample points – some were rural, some were urban. One was on the south . . . whole south end of Chicago, which is full of neo-Nazis [chuckle–Canfield], and Passin and I got arrested down there. I mean one of the neo-Nazis that we were interviewing, or I was interviewing, I think, called the police on me, you see, and the police took me in. But we had passes, we had IDs, so nothing ever happened about that, but there . . . there were certain risks you were running and especially in the urban areas, you see.

CANFIELD: And when was this? What year was this, '38, '36, '39?

BENNETT: Must've been . . . well, it's probably late . . . late '42 or early '43, somewhere in that period.

CANFIELD: So even then neo-Nazis were . . . were . . .

BENNETT: Well, that was just one survey.

CANFIELD: Okay.

BENNETT: See, there were surveys of all different topics. As a matter of fact, there . . . one survey, the Food Habits Committee got . . . got Likert to do a survey of food preferences. That was probably one of the initial . . . initial pieces of food habits research, but the . . . the real food habits research was taken over by Margaret Mead, and she . . . she gave a lot of money to Lloyd Warner and said, “Warner, get your boys going on a food habit study in rural areas of Illinois.” And that's how Passin and I got to study . . . got involved in that, . . .

CANFIELD: I see.

BENNETT: . . . the food habit study, you see. But there was a little more to the story than that because we were also that summer working for Fay Cooper-Cole as

archeologists. I mean he insisted on all the graduate students, no matter what their proclivities and outlook, to have at least one summer of archeological research down at Kincaid not far from here.

CANFIELD: Hmm.

BENNETT: And so [inaudible]. And we were doing that and we got to talking to the WPA board as we were doing the test [inaudible] that we were [inaudible] and we realized these people were marvelous. They were all dried-out farmers . . . I mean, . . .

CANFIELD: Right.

BENNETT: . . . broke farmers from the . . . from the Depression days, you see. So when Margaret Mead and Lloyd Warner approached us for this food habits study, we said . . . we told them we knew exactly the place to do this, in southern Illinois. So that's what happened . . .

CANFIELD: I see.

BENNETT: . . . and that's why we did the study there, you see.

CANFIELD: I see.

BENNETT: In other words, this . . . this . . . this represents a . . . another facet of that era. Graduate students in that era were frequently treated as senior people, you see, . . .

CANFIELD: Huh.

BENNETT: . . . and were listened to. They were . . . in . . . in other words, they weren't just, you know, slaves to the Ph.D. program, they were . . . they were individuals, see. They were supposed to have their own ideas about things. They were supposed to be able to construct their own curriculum, you see. It was a much freer period then, much less academicized, the academicization, which I think is a curse of . . . of much American

social science. Very much of American university life today is really a product pretty much of the 1950s, the postwar kind of a thing, you see. And I think that . . . again, to go back again, I think . . . again, the 1950s were important because that was the era [or] the period in which applied anthropology began to, as they say, “come of age,” quote unquote. In my view it was the ruination of applied anthropology, you see. In other words, they became institutionalized. They became academicized. That was a mistake. They should’ve retained that freedom that they had in the wartime period, you see; freedom to do what they wanted to do, freedom to invent ideas and impress senior people with these ideas that needed doing. Well, of course, you don’t always have wars, you know. I’m . . . so much of what I say I freely acknowledge is the result of the war, you see. But anyway, go on. Another question.

CANFIELD: But how did you get from . . . it seems to me from the way this is going, you . . . you went from your Washington assignment then to the Southern Illinois Food . . .

BENNETT: No, it was a little . . . I . . . I’m trying to remember the exact sequence of those things. See, the period was very compressed, . . .

CANFIELD: Uh-huh.

BENNETT: . . . very compressed in time, you know. In 1939 the war started. Some of these initiatives in food habits had actually begun before the war . . .

CANFIELD: Because . . .

BENNETT: . . . because ev-. . . because everybody knew we would get involved. Roosevelt’s attitude was . . . was catching. Everybody knew that we would be involved in this war sooner or later, so a lot of these initiatives, like Likert’s outfit and some of the

polling on . . . on attitudes of the American people toward the war, some of the food habits stuff hap- . . . happened even before 1939. Then comes 1939, and then graduate students then made [inaudible] Washington, you see, because now they felt we're in it [or] we're going to be in it. Well, I'm . . . I . . . again, we're going to be in it [or] the war has actually started. The next . . . the next crisis point was Pearl Harbor, . . .

CANFIELD: Right. Okay. Right.

BENNETT: . . . see, and [after] Pearl Harbor we were in it.

CANFIELD: Right.

BENNETT: Then . . . so at each one of these points, before the war, when the war started, and then at Pearl . . . after Pearl Harbor, new initiatives for social sciences were created, you see. Now, it was during this period from . . . in other words from 1939 to . . . all through the . . . all through the 1940s, really, I was involved in various kinds of wartime-related research. In some cases it would last for six months. At one point, for example, for one whole year somewhere in there, I was . . . I was inventing ways of . . . for the War . . . War Production Board, I was inventing ways of . . . of persuading Americans to collect wastepaper and waste metals for wartime purposes. I . . . I was involved with the . . . I was writing the ads for that [chuckle–Canfield] and advising on the social psychology of . . . of [inaudible] stuff, you see.

CANFIELD: Right. Right.

BENNETT: It . . . everything was going then.

CANFIELD: Right. I remember those ads. I saw those ads. Yeah.

BENNETT: Yeah.

CANFIELD: I was a kid, but I remember . . .

BENNETT: You remember that as a child, yeah. Well, you . . . the . . . the ads you saw, I wrote!

CANFIELD: Okay. Right. That's nice, yeah.

BENNETT: But . . .

CANFIELD: Okay. How did you get to Japan? We're back to this Japan stuff. How did you get from southern Illinois into the . . .

BENNETT: All right. You're still interested in this Japan [inaudible].

CANFIELD: Well, I mean that's . . .

BENNETT: Okay. All right.

CANFIELD: . . . [inaudible] Japan was still part of . . . a sense of . . .

BENNETT: Well, see, to get . . . is to some extent involved with . . . with my friendship with Herbert Passin, you see. We had worked together in food habits studies in southern Illinois, and we had worked together in the Office of War Information, and we had worked together in Likert's outfit, which was really part of the Office of War Information but somewhat separate. And then he decided . . . he was getting drafted and he decided to take an option that was off-. . . offered, an alternative that was offered, to take a job with the War Relocation Authority. The War Rel-. . . you know what the . . .

CANFIELD: No. No.

BENNETT: The War Relocation Authority was a Washington bureau that was set up to take the Japanese Americans who had been interned in these damn camps out there . . .

CANFIELD: Right.

BENNETT: . . . into other parts of the U.S. and find them jobs. And Passin . . . Passin took that job, which gave him immunity for the draft for a while. But then the draft board

began to catch up with him anyway, so then he took another alternative. He said, “All right, I’ll be drafted,” providing . . . at that time there was an offer for people, especially scholars, people with university backgrounds especially anthropological backgrounds to study Japanese. So he went to Boulder to the navy school and he spent about a year and a half at the navy school learning conversational Japanese and a . . . a smattering of reading of . . . of literary Japanese. And at the end of that time . . . now, we’re getting up to the end of the war, you see,

CANFIELD: Right. Right.

BENNETT: . . . so that he actually never saw combat. He was in uniform because he had to be in uniform because he had . . . he had to go through the draft even though he never went through basic training, you see. But at any rate, he was . . . then the war ended, the [atomic] bomb[s] [were] dropped and Japan surrendered. So Passin got to Japan, and now we’re talking 1946, okay?

CANFIELD: Right.

BENNETT: See how compressed all this stuff was?

CANFIELD: Right. Right.

BENNETT: In the period of six years, really, . . .

CANFIELD: Right.

BENNETT: . . . [in] six years I had five different jobs, all war related, all social science related, you see. Whether you call them applied anthropology or not, who cares? I mean it’s ridiculous, you know.

CANFIELD: Right. Right.

BENNETT: I used every . . . every tool the social sciences ever gave me, you see, . . .

CANFIELD: Right. Right.

BENNETT: . . . to . . . to do what I was assigned to do, okay?

CANFIELD: Right.

BENNETT: So then Passin was . . . got into the . . . into the occupation of Japan, and for about two years he did not much of anything. He was just from pillar to post, you know, temporary job to temporary job. Finally . . .

CANFIELD: Meaning in the States or in Japan?

BENNETT: This was in Japan. Passin was in Japan. I was in the States. What . . . what was I doing in the States? I finally got my Ph.D.

CANFIELD: Okay.

BENNETT: I decided to stay and get my Ph.D., write my thesis, which I did, and as soon as I wrote my thesis Ohio State University said, "Bennett, come down here. We want to give you a job because we . . . we have no anthropologist. They've all been drafted."

CANFIELD: Right.

BENNETT: So I went down to Ohio State. At that time I was not drafted because we had two kids.

CANFIELD: John, what did you write your dissertation on? Was this the food habits study?

BENNETT: No, the dissertation was on . . . on the study in southern Illinois.

CANFIELD: Okay.

BENNETT: It was not, however, on food habits. It was on the economic and social structure of the community, influenced by both Redfield and Warner.

CANFIELD: I see.

BENNETT: Again, the . . . the use . . . the combination, you know, light and dark so to speak, you see.

CANFIELD: Right.

BENNETT: I mean humanism versus social science again, which was, again, the . . . I think the . . . the . . . the great bo- . . . the great benefit of Chicago at that time. There were no favorites being played, see.

CANFIELD: Right. Okay.

BENNETT: Okay. So, all right, how did I get to Japan?

CANFIELD: Right.

BENNETT: Well, because . . . then they started this organization, Public Opinion and Sociological Research, and the work that we did there . . . at least the photographs that I took of the subject of the number one monograph in the other office right now, you see, . . .

CANFIELD: Okay.

BENNETT: . . . and finally got around to getting those photographs into the record, you know. But . . . so I went over . . . I got there in '47, . . .

CANFIELD: Okay.

BENNETT: . . . '47 . . . late '47. Late '47, I think it was, or early '48. I think it was . . . may- . . . it could've been . . . could've been November or December, '47, or very early '48 [when] I got there, and Passin was influential in . . . in bringing me there. He absolutely insisted on bringing me there because I had been his colleague in all this similar kind of research in the U.S., see?

CANFIELD: And he was doing survey research then, or was that . . . was the . . . what was happening in Japan?

BENNETT: Well, PONSUR, the division . . . Public Opinion and Sociological Research Division was a contract research division. That is, it was headquartered in CINE, Civil Information and Education, one of the staff sections under [General Douglas] MacArthur, one of the SCAP sections in there, and it . . . because it . . . it . . . anything that smacked of social science or religion or journalism, the social phenomena kind of thing, were headquartered in CINE, you see, whereas the work of economics was headquartered in . . . in a different [inaudible] . . . well, okay, never mind. Let's not talk about the structure of the Japan occupation. So our mission was to accept requests, contracts in other words, who had to be financed by that . . . that staff section on any subject whatsoever that would involve social science, whether it was public opinion or whether it was economics or cooperatives or you name it. And we were capable . . . with the Japanese staff and an American social science interdisciplinary staff, completely interdisciplinary, we were capable of meeting those requests, and we would then organize the project and use all the facilities of the occupation like jeeps to carry interviewers around if there was an interview study and so on, you see.

CANFIELD: So then . . . then someone else assigned you, then, these . . . these projects, is that right?

BENNETT: Well, we could initiate our own projects. It was a double barrel proposition. We frequently . . . I . . . I think the standard way of doing it, we saw a problem but we knew a staff section . . . let's say they were going to do something about fishing cooperatives. That is, a Natural Resources Division of SCAP was supposed to

reorganize the ownership structure of the fisheries, the coastal fishery communities. We would see . . . we knew they were doing that because we had friend in there who were assigned to that, and then we would say, “Look, we . . . you need some research on the nature of cooperatives.” And then we . . . and then they would say, “By God, you’re right. Why don’t you do a study for us?”

CANFIELD: Okay. Where did the money come from?

BENNETT: The money came from SCAP.

CANFIELD: Okay.

BENNETT: It was part of the occupa-. . . well, the money came from the U.S. taxpayer . . .

CANFIELD: Right.

BENNETT: . . . where . . . and also some international funds were involved, see, because there were other . . . I mean this was the Allied occupation of Japan. The Americans were kings of the hill there, but there was still a lot of other countries involved, Britain, Ca-. . . all the British Commonwealth countries were involved. France was involved. Russia . . . [the] Soviet Union was involved.

CANFIELD: Right. Right.

BENNETT: And they all had to contribute this to the occupation, see, but MacArthur was the emperor, you see.

CANFIELD: [Chuckle] Right. John, what . . . what were you actually doing in Japan? Can you explain . . . I’ve heard you talk about some of the major projects then, if you . . . if you [inaudible] oversaw, explain what those were. [Inaudible] . . .

BENNETT: You mean what I did personally?

CANFIELD: Well, yeah. What were you directly involved in, okay?

BENNETT: Well, I was directly involved, [in the] first place, in managing the organization and . . . in other words, I was the chief.

CANFIELD: Umhmm.

BENNETT: John [Pelzell] of Harvard had been a chief. He was the first chief. He didn't stay any longer. He went back to Harvard to finish his Ph.D. He was a graduate student in Harvard in the Yenching Institute there, . . .

CANFIELD: Huh.

BENNETT: . . . a Far Eastern program at Harvard. He was sort of an anthropologist, but he was not one of these interdisciplinary types. He was a Peabody Museum Yenching type of anthropologist.

CANFIELD: Okay. Right.

BENNETT: But he's been . . . he's been in the Marine Corps, you see. Well, the chief . . . the chief of CINE was a lieutenant colonel of Marines, Don [Nugent?], who had been a high school principal in California. [Chuckle]

CANFIELD: Really?

BENNETT: But he was a Marine colonel, you see, . . .

CANFIELD: Umhmm.

BENNETT: . . . so he made sure that John [Pelzell], who was a Marine captain, . . .

CANFIELD: Right.

BENNETT: . . . got a job, see, in CINE, . . .

CANFIELD: Right.

BENNETT: . . . and the job was this sort of social science thing, you see. But John went back and there wasn't much going on for about two years until, finally, the land reform study started. There was a big trust . . . thrust in the occupation was the Japanese land reform. And they brought in a guy by the name of Raper, Arthur Raper. That name you should know. Arthur Raper was in the 1940s and ,50s "Mr. Land Reform." He not only wrote and organized the Japanese land reform, he did the same damn thing in Korea, and he did the same damn thing in at least one African country. He was the great . . . the great thinker of . . . on . . . on the . . . and organizer of land reforms in the postwar period, you see. And so Arthur Raper needed a staff . . . he needed a staff to do a study of the . . . of seven villages in Japan that had experienced a land reform, and the idea was to find out what the land reform had done to them. This was only two years after the land reform had been instituted, but he figured that was enough time to at least get an idea about the results and consequences and it was. So Arthur Raper recruited a bunch of people, and these people later . . . Americans like David [Sells] and Tommy [Tsujiama], [and] there were a couple of others; I think Passin was involved in it. No, he came in later at . . . when the write-up time [came], but that became the chore of the PONSER re- . . . American research staff, you see, by that time and the Raper study was published in a book. Later it . . . the University of Wisconsin Press actually reproduced it in a . . . in a hardcover version some years ago. I still have a copy of the original SCAP publication at home, if you ever want to see it.

CANFIELD: Oh, yeah. Yeah, I would like to see it.

BENNETT: I think it's maybe even be down . . . down at the office.

CANFIELD: Okay, I'd like to see that.

BENNETT: The . . . the title of it is, *Japanese Village in Transition*, see, by Arthur Raper, and then a list of the PONSR people who did all the fieldwork, see.

CANFIELD: I see.

BENNETT: David [Sells], Tommy [Tsujiama] and so on. Cynthia . . . Cynthias Ike, the names [are] all there.

CANFIELD: Really?

BENNETT: And I think the actual publication . . . Passin had much more to do with the actual organizing of the publication. He didn't have much to do with the fieldwork, but he . . . I think he was probably the principal figure for getting the data together, you see. At any rate, that was . . . that . . . that or . . . that particular project started . . . that confirmed PONSR as an authentic and useful division. Up to that time it was just kind of a public opinion unit in the depart- . . . in the education division of the CINE, you see.

CANFIELD: Huh.

BENNETT: Kind of a complicated story, typical bureaucracy stuff, very much like Washington, you know. It was a very similar kind of situation.

CANFIELD: Now, are . . . did you . . . you were in the fieldwork project? You were in the . . .

BENNETT: No, I had a lot of fieldwork . . . I . . . yes, of course. I was in the field fifty percent of the time, and I had to fight like hell to win that because the chief of CINE insisted that I should be on duty all the time. I was chief officer of the division and he was thinking in military terms. He wanted his men there ready, you know, for anything, you see, . . .

CANFIELD: Right.

BENNETT: . . . and I had to fight with him to . . . to say that . . . well, the chief of PONSER is not just a colonel or a captain. I may have had a colonel rank for salary purposes but I said, “I ain’t no colonel, I’m a social scientist, and I’m going to go out in the field, goddammit!” And he finally exceeded to that against his will, you see, and I . . . Passin was always the deputy chief when I was gone, you see.

CANFIELD: Yeah, I see.

BENNETT: But sometimes Passin and I were together in the field, especially when we went to exotic places like the Inland Sea in Japan where Americans had never been before because we were studying fisheries at that time, you see.

CANFIELD: Of course.

BENNETT: Now, there were a whole line of things . . . I stu-. . . I did studies of forestry, studies of fisheries, studies of cooperatives, studies of . . . of political autonomy in local communities, studies of prostitution, all kinds of stuff, and these were all generated either by us or by us plus another staff section or division in the occupation.

CANFIELD: What did you do with those studies? As you wrote something, what happened to your writings, you know, your reports? Did they just . . . did they ever get put together in . . . in a volume or . . .

BENNETT: Well, it’s never been . . . the actual research . . . let me try to . . . I mean there have been a lot of publications, and the Japan manuscript down the hall here has a bibliography. In other words, a lot of articles came out about these researches, some generic articles. There was at least one article in *Human Organization* in the 1950s which I wrote. There was a successor study in the 1950s sponsored by the . . . sponsored

by the Social Science Research Council on Japanese educated in America. I ran that study.

CANFIELD: Really? Okay.

BENNETT: I ran it at Ohio State. That was not . . . Passin did some interviewing in Japan for us on that one and his name is on the . . . was one of the three authors of the book. The book is in the other room. I'll show it to you. Now, that was a successor of PONSER in a sense, you see, but it was at Ohio State.

CANFIELD: Umhmm.

BENNETT: And it started, actually, . . . that plus . . . plus the book. Iwao Ishino, you see, was with the . . . I got . . . I hired him in the second year of my term as chief. I called up Clyde Kluckhohn. Clyde Kluckhohn had been counseling in Japan in the CINE. As a matter of fact, I think it was his advice that probably was influential in setting up PONSER at one point. He was one of their what they called "90-day wonders" and [was] brought in, you know, . . .

CANFIELD: Right. Sure.

BENNETT: . . . and wined and dined by MacArthur and all that jazz, you know.

CANFIELD: Right.

BENNETT: But the point is that I called Clyde [and] I said, "If you have somebody we need . . . we need a Jap-. . . we're losing our Jap-. . . one of our Japanese Americans." That was Tommy [Tsujiama]. He was going back to California to get a degree and he said, "I got just the man for you. Iwao Ishino." He was a . . . he was Alex Leighton's assistance . . . assistant in the famous study *The Governing of Men*. *The Governing of*

Men was a study of Poston. Poston was the one camp . . . the one relocation camp where they had riots [or] . . .

CANFIELD: Okay.

BENNETT: . . . they had protests, and Iwao Ishino, who'd had a couple of graduate courses in anthropology at Harvard – in that interdisciplinary era he had a couple of courses, I think, under Talcott Parsons, too – he was in that interdisciplinary kind of . . . kind of . . . kind of atmosphere, you see. So Clyde said, "I have just the man." So I said, "Send him out." I cut the . . . had the . . . had the travel orders cut within one hour and sent . . . relayed by telegraph – we didn't have computers in those days – to Iwao, and Iwao was out with his family within one month. And now Iwao . . . I brought the . . . brought Iwao to Ohio State University, you see.

[End of Tape #1, Side #2]

[Begin Tape #2, Side #1]

BENNETT: I set up something called Research in Japanese Social Relations, RJSR, and we published a series of mimeographed monographs. Some of those monographs were the result of that work for PONSER in the occupation, and eventually Iwao and I authored this book *Paternalism in the Japanese Economy*. That was simply a reprinting of some of the stuff that we did in Japan, you see.

CANFIELD: I see.

BENNETT: Okay? So that there was in Ohio State this continuation of the PONSR activity to produce the book, for example, on the Japanese students, which really was not done by PONSR, but it was done in . . . in the RJSR framework, you see. We also bought Herb Passin in for a couple of months.

CANFIELD: RJSR, it means . . .

BENNETT: Research in Japanese Social Relations, . . .

CANFIELD: Okay.

BENNETT: . . . see, and there was a whole series of mimeographed studies that came out for the Ohio State Research Foundation. There are still in the Ohio State Library, one or two maybe in this library, but the . . . the . . . the only hardcover stuff that came out on that was that *Paternalism in the Japanese Economy*, and then, even though it was sponsored by another organization in the postwar period, the study of Japanese students in America, you see. *In Search of Identity* we called that report.

CANFIELD: That's right. I remember that.

BENNETT: Yeah. It's in the other room.

CANFIELD: Yeah, I have a copy of that.

BENNETT: But the point is that the Ohio State then continued the interdisciplinary atmosphere, kind of a . . . a mixture of Jap-. . . Japanese studies and social science and applied problems of one kind or another.

CANFIELD: John, I don't want to pass over . . . it seems to me there's something else you've not said about the Japan experience. You ta-. . . I've heard you talk about surveys that you did where . . . where . . . where there was a statistic combination of things. What

was that project where you . . . you were collecting data and you had these young ladies calculating [inaudible]?

BENNETT: [Chuckle] You mean . . . hah! Well, you see, we didn't have . . . at that time we didn't even have calculators.

CANFIELD: Right.

BENNETT: I think toward the end of the division's life in Tokyo, I did . . . we ma-. . . managed to get some . . . some ordinary calculators, you know, . . .

CANFIELD: Right.

BENNETT: . . . like the kind they had here when you first came here, these damn things that are . . .

CANFIELD: So that was probably [inaudible] . . .

BENNETT: Yeah. You punch buttons, you know, and so on. But at first all we had was young Japanese girls with abaci, you see.

CANFIELD: That's right. [Inaudible].

BENNETT: And the point was, they . . . they would always beat the . . . the machine operators, you see, even when the machine oper-. . . machines came in we had people working with them. The little girls with the abaci would always be faster. [Chuckle—Canfield] You know how the . . . how a abacus works?

CANFIELD: Yeah, more or less. Yeah. Umhmm. Yeah.

BENNETT: See, you can't beat it for sure speed if you're good at it because you can . . . you know, you can . . . you . . . you can say . . . you can calculate a thousand digits with . . . with one . . . one single button, you know, you got it. You already added a thousand, you see. You can't beat it.

CANFIELD: Yeah. Yeah. That's right.

BENNETT: And no . . . I mean very few people understood that, . . .

CANFIELD: Yeah.

BENNETT: . . . that it was really a . . . a form of instant calculation in the hands of a very skilled operator. If the operators weren't skilled, they made a hell of a lot of mistakes.

CANFIELD: Yeah. That's right.

BENNETT: But the reason for all this was that we were doing public . . . public attitude calculations, you see. You know, . . .

CANFIELD: What . . . what was . . .

BENNETT: . . . [with] public attitude surveys, what you do is cross tabulate attitudes with reference to specific social and economic factors like income.

CANFIELD: So . . .

BENNETT: Let's see how many people at . . . with an income of between \$5,000 and \$10,000 believed that MacArthur was a jerk? [Chuckle]

CANFIELD: Right. Okay.

BENNETT: See? Okay.

CANFIELD: So you got people going into the field surveying?

BENNETT: Yeah, right. Now the business of going into the field that . . . they . . . I mean we did various things in the field. We did formal interviewing. We did ethnographic studies in the field. We did public attitudes studies in the field on the basis of . . . of kind of questionnaires. We did all kinds of things. That was the whole point, you see.

CANFIELD: And . . . and you . . .

BENNETT: We did anything to get the information in. We were interdisciplinary people, you see.

CANFIELD: Yeah.

BENNETT: Now, I think . . . so I think if you really want . . . you want to come down to this business of definition. My definition of applied research, first of all, is eclectic. There's no such thing as applied anthropology. It's applied whatever, you know. It's applied everything, you see?

CANFIELD: Okay.

BENNETT: You use whatever principal, whatever idea, whatever you need to have in order to get the information you think is important. That means secondly, you've got to have a clear-cut idea of what's wrong or what needs improvement or what needs to be fixed or what people want and something . . . you've got to have a definition of the situation, you see?

CANFIELD: Umhmm. That's right.

BENNETT: You cannot do applied work of any kind without a definition of the situation that you're trying to improve on, you know, or correct or something, you see.

[End of July 18th Interview Session]

[Begin July 22nd Interview Session]

CANFIELD: Okay. This is Robert Canfield and I'm talking to John W. Bennett on July 22nd. This is our second session together, and we will pick up from here on what we . . .

we were doing last ni- . . . last time. John, are there things you want to pull together at the beginning?

BENNETT: Yes, Bob. It occurs to me that that was a very important point made implicitly in the first session but not articulated, and that had to do with the fact that applied anthropology, whatever it is, is certainly intimately responsive . . . responsive to world conditions. In other words, applied anthropology is not done on the basis of purely scholarly motifs or theoretical propositions. In other words, it's indep- . . . it's . . . it's . . . it . . . it appears when an anthropologist responds to some problem in the outer world, and the one that I mentioned on the basis of my own biogra- . . . biographical data was World War Two; that is, the various topics . . . the various activities that I . . . I got involved in in World War Two and just before the war, all had relationships to the oncoming war or the . . . the existing war. And many of these were fairly important topics [such as] public opinion attitudes of the American population toward the war, and then in Japan important research concerning occupation period reforms, either reforms that had been done or reforms that were proposed, and which . . . in which we were asked to do the preliminary organizing research. So . . . and all of these . . . all of these things were deemed, at least by the people of the time, important.

CANFIELD: John, . . .

BENNETT: We could've mentioned . . . we could've added one more thing to this: the British social anthropology . . . applied anthropology of the 1920s and „30s. I didn't mention that because we got off pretty much on a biographical kick the other day, but the fact is that, again, you had a world con- . . . condition in the outer world that the anthropologist studying African tribes were responding to, mainly the fact that there were

problems with the British colonial administration. And these tribes and the anthropologists were asked, or volunteered, to do the research that would help clear these matters up. I'm thinking particularly . . . particularly of the little book by Brown and Hut is to describe the variety of these fairly important tasks that anthropologists assisted with. And then, of course, if we project this into the present . . . into the recent present, into the 1950s . . . in other words, we've come down from the ,20s, ,30s, ,40s, the wartime. Now if we get into the ,50s and ,60s, we have a . . . a different set of conditions in the world, namely economic development . . . economic and social development. And so that applied anthropology's identity changes depending on the conditions in the outer world, okay?

CANFIELD: John, I . . . I want to ask you a little more of that. You did do some opinion surveys in Japan, didn't you?

BENNETT: Yes, a lot of them.

CANFIELD: Okay. What . . . could you say more about what they were?

BENNETT: Yes, of course. [Cough] Just about every kind of topic you could think of was involved, and . . . for example, at one point we were asked to do a national survey on the nationals . . . on one of our national samples of Japanese attitudes towards prostitution. And the reason for that was very simple. The . . . a lot of the dependents . . . ladies in Washington Heights, which was an American constructed residential suburb, really, in the heart of Tokyo for, particularly, the wives and families of . . . of non-commissioned officers, they were advocating . . . many of them were . . . were Catholic and they were advocating that the occupation do something about this blatant prostitution in Japan. Japan has always had a rather large and flourishing and quite orderly feminine

professional world involving sex, as you'll probably know, I mean not only in several grades and levels of prostitution but also the more ornate and . . . and . . . and literary side of the geisha. And . . . in other words, it's been accepted in Japan as . . . as perfectly normal that men have these instincts. Well, the occupation was reluctant to pressure Japan . . . Japanese government at that time in . . . in order to draft legislation which would outlaw prostitution – that's what the ladies wanted – and we were asked to do an attitude survey on that, on the basis of the whole country, and we did and we published that. I mean it was published in the occupation as a . . . as a regular PONSER report, and we found some very important things. We found that . . . that there was a difference between men and women's attitudes towards prostitution. Women were much less favorable. Men were not only . . . were not exactly favorable, they simply accepted it as a matter of course and so on. In other words, we found . . . what we did was found that most of the cultural attitudes that had been described in various works previously toward prostitution and other feminine activities of a professional nature for Japan were really quite accurate. We . . . and the result was that the occupation refused to pressure the government. They felt that the government had it pretty well under control and the Japanese people were not that disturbed about it. They did, however, . . . they did, however, increase . . . on the basis on some of the recommendations that we made and other people, increase the . . . increase the medical attention given prostitutes and so on. But that was a rather unusual survey. Most of the others were involved with political and economic affairs. We did attitude surveys toward the improvement of fishery rights, the improvement of . . . of personnel relations in the . . . in the labor sector of the forest industry and wood products industry. We did public attitude surveys toward the

occupation changes in . . . in local autonomy, the ability of Japanese communities to govern their own affairs without interference. We did surveys on the . . . on the change in the police system, the elimination of the . . . of the secret police, for example, the Kampei Tai, and [cough] . . . and so on. That is . . . see, we had . . . the public opinion activities of PONSUR were branched out. That is, the . . . most of our surveys were actually run by a . . . a high quality attitude survey bureau lodged in the prime minister's office. They were our major arm. That is, the survey questionnaires and so on were constructed by that group and us in the office as a joint, sort of, Japanese government and PONSUR activity, you see. Not all the surveys, but cer-. . . any . . . any of those which involved governmental or political reforms tended to be a joined product. But we also did some things entirely on our own, and in many . . . I would say fifty percent of the surveys, public attitude surveys, we did were incepted by us. In other words, we saw a need for these . . . for some . . . some public opinion data. The other fifty percent we were asked to perform by different SCAP sections and Japanese government sections, you see, but we had . . . in other words . . . all right, our . . . our identity was somewhat mixed. We were a SCAP section and entirely within the occupation, but on the other hand we were also working hand-in-glove with the Japanese government, and our role was not that of a commanding . . . a military command relationship. It was entirely cooperative. And our staff con-. . . contained about twenty or thirty Japanese social scientists as well as about six or seven Americans, and we . . . we really . . . in some respects we saved the lives of some of these Japanese who . . . there was . . . I mean their . . . their department . . . psy-. . . psychology department, sociology department and so on, many of the Japanese universities were simply closed down because there wasn't enough money to operate

them in the postwar . . . early postwar period, and we hired these people and used them very effectively and . . . both at the level of sociological community work and investigations. For example, before we did public attitude studies on forestry rights and ownership and so on, we did community studies, studies of how the communities are organized. Another dual kind of study we did, we set up all the . . . the terms both on the basis of attitude surveys and also community sociological studies for the first cen-. . . census, the first census in Japan after the war, . . .

CANFIELD: Hmm.

BENNETT: . . . the 19-. . . the 19-. . . what was it? Nineteen-. . . well, the census was run in 1950, and we were doing . . . we did all the basic research on the . . . the family structure problems in designing the schedules in 1949. And so it goes. I could talk for two hours on all these projects, . . .

CANFIELD: Sure.

BENNETT: . . . see. But the point is, was it applied anthropology? Who knows? I . . . we didn't think of it as that, of course, you see, and I think that is another important point here. If you respond constructively to real problems in the outside world, I don't think it matters what you call it: applied anthropology, applied psychology, applied sociology or whatever. And I certainly . . . again, in my attitude towards it, I was equally ambiguous and I . . . that's the wrong word. It's not ambiguous. I just didn't care, you know. I was doing what I could do, and since I'd had rather eclectic broad-level training in various social sciences, I had the skills to do it with.

CANFIELD: John, one more question about your Japanese experience. How about language? Did . . . did . . . did the Japanese learn English pretty well so that you could relate . . .

BENNETT: Well, you . . . you must understand that English has been, since the 1920s, really, the second language in Japan.

CANFIELD: Okay.

BENNETT: The scholars all know it, you see.

CANFIELD: So we . . .

BENNETT: Now, however, we had to have a lot of translations. That is, translations of English documents into Japanese and Japanese documents into English. Take, for example, a questionnaire that we were running on a national or even a local sample of some kind. You had to . . . you . . . we would compose it initially in English. As a mixed group of Japanese and . . . and American scholars we would get together and try to compose it in English, and then after we got it in English just the way we wanted it, then we would have other sessions to translate it into Japanese so it could be run in Japanese on a national or local sample, you see. And sometimes the local samples were fussy because we had to . . . we might have to indulge in local idioms of one kind or another.

CANFIELD: Umhmm.

BENNETT: There's a certain amount of variation of Japanese language from one district in the country to another, just as there is in any . . . in any country including ours, but . . . including the U.S., but actually . . . but somewhat less than it is in . . . certainly less than it is in the U.S., but . . . so you did have to take that into account.

CANFIELD: [Inaudible].

BENNETT: But there was a lot of translation work going on in our office. I would say out of those twenty or thirty Japanese scholars that we employed, I would say at least . . . at least nine . . . between five and nine were engaged mainly in translation . . . written translation of various things.

CANFIELD: All right, John. How did you wind up your . . . your tour in Japan? What . . . what took . . . when did you leave actually, and what were the circumstances?

BENNETT: Well, I left in early 1950 and it was very simple. By this time the Korean War was well underway.

CANFIELD: Hmm.

BENNETT: And, as a matter of fact, we . . . I left in early '50 . . . yeah, I think it was around February of '50, February or March, I don't remember the exact date. I'd have to look it up. But by this time . . . when was MacArthur fired by [President Harry S] Truman, do you remember that? Was that in late '50, I think?

CANFIELD: Well, it could be. I . . . I . . .

BENNETT: I think it was in late 1950, and Ridgway . . . General [Matthew B.] Ridgway took his place.

CANFIELD: All right.

BENNETT: And I think that . . . that . . . see, I think the . . . the close-up . . . the closing days of the occupation started in early 1950, in . . . in about January, and the announcement was made by the State Department, I think, that the . . . see, but by this time MacArthur was in Korea running the Korean War.

CANFIELD: Yes, he is.

BENNETT: And Ridgway had come in to take his place at SCAP. So he's Supreme Commander of Allied Powers, you see.

CANFIELD: Oh, that's true.

BENNETT: See, and I think Ridgway began negotiations with the State Department saying that we . . . that in . . . in my opinion and the opinion of most of the specialists from SCAP, we've done our job here, we're finished, you know.

CANFIELD: We're actually . . . we're actually only there, what, four years or five years? Four . . . four years.

BENNETT: A little o-. . . well, there are two . . . really, two occupations, really. The first occupation was strictly military. That lasted about six months. That is this . . . the 1st Cavalry and the Eighth Army moved in. They had been with MacArthur in the island-hopping exercise, . . .

CANFIELD: Right.

BENNETT: . . . you know, in the Pacific, you see, and their job was simply to occupy Japan in the literal sense of a military occupation. During that period all the specialists began being brought in . . . in one after the other, and this was an operation that started, actually, two or three years earlier in Honolulu at the University of Hawaii. That is, various specialists were tagged for an eventual role in the Japan occupation, but it took quite a while for that to really materialize. In other words, it had to be organized. So what I'm saying [is], the first occupation was strictly a military occupation to pacify the islands. They did . . . it turned out they didn't need that pacification, but that was the initial objective. There are lots of famous stories about this, how the Marines landed on the beaches in Kamakura with guns drawn, you know, expecting to be blasted out of the

water, and being met by a . . . a prostitution entrepreneur with a truck full of whores, you see.

CANFIELD: [Chuckle] I see.

BENNETT: There are some marvelous stories of this type, you see, because the Japanese welcomed their . . . the people, you see, and . . . because the Japanese were thoroughly fed up with the war, you see, . . .

CANFIELD: Yeah. Yeah.

BENNETT: . . . and they had surrendered. The . . . the . . . the emperor had made a tape recording in a little studio next to my office, as a matter of fact. I mean I wasn't there then, . . .

CANFIELD: Right. Right.

BENNETT: . . . but it just happened to be that room. [It] was a little map room for [the] imperial broadcast, and he had made this . . . this tape, and they had to smuggle him in because a couple of young army officers refused to . . . to acknowledge the surrender. They . . . and they wanted to capture the emperor and kill him or something, you see.

CANFIELD: [Inaudible].

BENNETT: And . . . but they smuggled him in the back door of Radio Tokyo and got him into this little map room and locked the door, and then he gave . . . he gave [inaudible] tape recordings of the speech, you see. [Chuckle–Canfield] And that, of course, was not a tape recording, that was a wire recording. There weren't such things as tape record-. . . recorders at that time, just wire, you see.

CANFIELD: That's really . . . can you . . . can you again remind me of when you came? When did you go to Japan? What year was it?

BENNETT: Well, I started out . . . I started out in '46.

CANFIELD: Forty-eight . . . '46.

BENNETT: But I couldn't get a ship over. See, they didn't fly people over in those days.

CANFIELD: Right.

BENNETT: Only big shots. I mean generals . . .

CANFIELD: Right.

BENNETT: . . . and so on. So I . . . I vegetated in . . . in . . . in Seattle for almost . . . for a whole month waiting for some transportation. There were other problems at this . . . [chair squeaks] . . .

CANFIELD: It's all right.

BENNETT: What's going on?

CANFIELD: That's your chair. Your chair squeaks sometimes.

BENNETT: Okay. [Chair squeaks]

CANFIELD: It's all right.

BENNETT: What is that noise?

CANFIELD: Your chair squeaks when you move.

BENNETT: Was that [inaudible], was it? Maybe I'm wrong.

CANFIELD: Sometimes, yes.

BENNETT: Anyway, how are we doing?

CANFIELD: We're okay.

BENNETT: The point was that I just had to wait there, and Kathy came out to stay . . . spend some time with me for about two or three weeks. We spent most of our time up at

the University of Washington, the [inaudible] apartment. There were a bunch of people up there we knew. That's . . . as a matter of fact, Walter Taylor, the archeologist, was teaching there that semester.

CANFIELD: Hmm.

BENNETT: And he and I hung around a lot. As a matter of fact, there's a chapter in the book on Walter Taylor. There's a footnote about that episode . . .

CANFIELD: Yeah.

BENNETT: . . . how Taylor and I had . . . had this almost a whole month of arguments over Chinese food, you see, . . .

CANFIELD: Oh, really? Yeah.

BENNETT: . . . and his . . . his ideas about archeological theory and so on. I finally got a ship in early . . . early '47.

CANFIELD: Okay.

BENNETT: No, it's all reversed. It was late . . . it was late '47 that these events took place. I finally got a ship in early '48 . . . in January, '48, and got to Japan in ten days. I left in February or March 1950. So I was there for about two and a half . . . we were there almost three years.

CANFIELD: Oh, I see.

BENNETT: The family came over in, oh, around . . . I think it was in January, '49, or something.

CANFIELD: I see.

BENNETT: Oh, no, [it] was December of '48 [when] I think the family came over . . . the dependents came over, and so we were there about two and . . . almost two and a half years really and . . .

CANFIELD: Now, when you came back, you came back to Ohio State, . . .

BENNETT: Yeah.

CANFIELD: . . . is that right? You had already been hired by Ohio State?

BENNETT: Well, that was the whole point, you see. One . . . in other words, I had been hired by Ohio State as a replacement for a drafted anthropologist, and I then fell under the rules of wartime employment draft . . . in other words, when I went to Japan I was considered to be going into the army, I was considered to be drafted, and I had a simulated rank and so on, you see, so that Ohio State said, "Well, you're another . . . another wartime veteran." So as a wartime veteran I was then able to come back to Ohio State.

CANFIELD: Oh, I see.

BENNETT: They wanted me back anyway because I'd set up for the first time a real anthropological program. It was one of these sociology departments that was mainly concerned with family counseling and rural [inaudible] and that sort of thing, you see, and they wanted an anthropologist program, and I had set that up in the brief period I was there. I was there for only . . . I was there only about a semester and a half before I went to Japan, see.

CANFIELD: Well, John, when you . . . when you . . . well, let's think about this period in the 1950s and '60s, what was happening to anthropology, especially these . . . these people that are associated with applied anthropology?

BENNETT: Well, the point . . . see, the other day when we were talking, I . . . I laid considerable emphasis on this whole business of responding to wartime questions and comments, and I also at the same time emphasized the interdisciplinary atmosphere of the social sciences in that period. That is, I think the two were related but not necessarily. I think that . . . that in the 1930s, as a result of the New Deal social science, anthropology included, was heading in this interdisciplinary direction, a sort of global social science approach. And that . . . that particular approach then served well in the wartime employments here, you see. It served me well because I had this rather eclectic training in various fields of social science and behavioral science, you see. Social psychology and social statistics and sociology, social theory and all that, and anthropology and so on, you see, so that it meant . . . in other words, that . . . that you had a fusion in that period of two things . . .

[End of Tape #2, Side #1]

[Begin Tape #2, Side #2]

BENNETT: . . . during the war and before the war on applied anthropology. If you want to go back again, you could even date it back into England and their applied anthropology or applied colonial organization. They didn't call it applied anthropology .

..

CANFIELD: No [inaudible].

BENNETT: . . . in Africa, you see. Now I think something else happened, something very important happened. A lot of the people . . . a number of the anthropologists who had been in the war in uniform or out of uniform came back in the 1950s and decided that anthropology had to be rescued from interdisciplinary activities because they were scared that sociology would absorb the discipline. In other words, once colonialism . . . the second reason was this, that colonialism was in shambles. All over . . . in the 1950s all over the world all the old colonial empires were falling apart and new nations were coming into being, . . .

CANFIELD: Umhmm.

BENNETT: . . . and that meant a lot of anthropologists felt that something had to be done right away because the . . . the tribal . . . the tribal societies were going to be absorbed into the new nations as citizenry, you see, which would mean development, of course, economic development and so on, you see. That was well . . . that was known so that it was . . . there was a kind of panic attitude that I felt that some of the early meetings in the early 1950s of the . . . of the American Anthropology Association, people would get up and say we've got to get money to do this and so. Well, the government, you see, had money, and the federal government in the 1950s announced a tremendous expanded plan for funding anthropological work and what was called Foreign Area Research.

CANFIELD: That's right. That's right.

BENNETT: This university eventually benefited from that as you know, even as . . . well, I mean it lasted for just a decade, and then in the early 1960s when things happened here, that was part of the reason they happened here was when Eliot got a ten million dollar grant for Washington University for Foreign Area Studies, if you remember.

CANFIELD: Yeah. I wasn't here, but that . . .

BENNETT: [Chuckle] You weren't here then, but that's what set up what . . . what was developing then through the '60s and '70s in Foreign Area Research. I don't know how much of that is left. Not a hell of a lot, I imagine. But at any rate . . . so in the 1950s you had what several people including one of our colleagues, Ed Robbins, called "anthropological involution." And what he meant by that was that anthropologists were going back to Kroeber, see. That is, culture . . . cultural history, cultural description, ethnology. They had . . . they felt they had to do this so that anthropological theory, which had been heading in an interdisciplinary direction before and during the war, then made up an ab-. . . abrupt about face back into traditional anthropology emphasizing culture and ethnology, you see. I think another thing was that . . . important was that . . . simply that human paleontology, digging up fossil men, wasn't . . . had to get going after the war. The war closed down all the sites everywhere, you see, . . .

CANFIELD: [Inaudible].

BENNETT: . . . so there wasn't much of that anthropology. So after the war, the only anthropology was cultural or ethnology, whatever you want to call it, you see.

CANFIELD: Umhmm. Umhmm.

BENNETT: And so that I think anthropology lost its contacts with all the other social sciences during this period, and I think it's a matter to be regretted. I've always regretted it.

CANFIELD: How about . . . there . . . there are some . . . some of these famous figures in the discipline that you knew and dealt with in that [such as] Margaret Mead [and] Ruth

Benedict. You talked about Lloyd Warner. These, you know, s-. . . s-. . . new things were happening, but some of the old guard were also still . . .

BENNETT: Oh, yeah. Sure.

CANFIELD: . . . prominent figures, and Kroeber was . . . he must have died about that time. I don't know when Kroeber passed away, but it must've been in the ,50s or early ,60s. But Margaret Mead was . . .

BENNETT: If I remember . . . pardon me.

CANFIELD: Sure.

BENNETT: If I remember right, Van Willigen made a list . . . had a list there and the questionnaire about what he considered to be applied anthropologists. Could you read that list again?

CANFIELD: Well, you . . . you got [inaudible] Margaret Mead, Ruth Benedict, . . .

BENNETT: Right.

CANFIELD: . . . Allison Davis – I don't know that name – Lloyd Warner, John Provinse, Rhoda Metro. He mentions the . . . the studies of food. This is the . . . these food habits relation, Rhoda Metraux, Hortense [Powdermaker], Herbert Passin, and John Bennett. These are the names . . .

BENNETT: Well, it's . . .

CANFIELD: . . . but he doesn't mention [inaudible].

BENNETT: . . . it's really a miscellaneous background, you see.

CANFIELD: Yeah. Yeah.

BENNETT: Now, some of those people . . . he could have . . . he should have mentioned Eliot Chapple.

CANFIELD: Okay.

BENNETT: See, put that name in there. He should have mentioned Freddy Richardson. That name should be in there. Chapple, Richardson, Warner, that group, and some others were all part of that Harvard interdisciplinary movement that started originally in the Harvard Business School was . . . as the patron of these people, you see, and they had . . . I mean, let's see. Richardson . . . Richardson and Chapple stayed at Harvard, I think, and wrote books. Chapple wrote a famous *Introduction to Anthropology* after the war which was a real [inaudible] thing, and it is a perfect expression of that interdisciplinary . . . that wartime Harvard interdisciplinary attitude. It had all kinds of stuff in it that normally was not considered part of anthropology. It . . . if you look at the book, it's . . . it's . . . it's a fabulous book in many ways, but it didn't speak to these involutionary anthropologists in the 1950s who were trying to revive the . . . the traditional past of the field, you see.

CANFIELD: I see.

BENNETT: They couldn't make head or tail out of it, you see. And Warner, of course, during the '50s was publishing the . . . the volumes of Yankee City. He'd done the research earlier, but he was . . . well, I think he was . . . he had two or three additional projects in the '50s, too, but eventually four volumes came out on that. Was it applied anthropology? No, I don't think so. I think it was interdisciplinary anthropology, you see.

CANFIELD: [Inaudible].

BENNETT: It was a different thing. Now, Margaret Mead, as I mentioned last time, grabbed on to any avant-garde field, [or] anything she regarded as avant-garde. She was going to dominate it, she was going to advertise it, [and] she was going to promote it.

Margaret Mead was essentially a publicist. I don't think she was much of a researcher. I think she . . . her imagination usually got away with her on her . . . much of her research, but she was a damn good publicist of new ideas and a publicist of anthropology. But her anthropology was very applied, in a sense. She felt that anthropology should be out in the world doing good things and advertising good ideas and good scientific procedures to the general public. She was always advocating things like mother's milk, that sort of thing, you see. She was always out in the front of some . . . of some particular kind . . . some particular avant-garde field, and then she would iden-. . . she was very clever in identifying it with anthropology, you see, so that she was . . . and then she got very deeply involved in general science and eventually became a president of the American Association for the Advancement of Sciences, as you know. I think the last ten years of her career were really not so much in anthropology. They were in general science and . . . and the pu-. . . public relations of science, as well as the public relations of anthropology. Now, there was a couple of other names there. You mentioned Allison Davis.

CANFIELD: Okay.

BENNETT: I don't consider Allison Davis . . . yeah, well, Allison Davis was a buddy of Lloyd Warner. He and Lloyd Warner produced a series of books on race relations.

CANFIELD: Hmm.

BENNETT: Now, Allison Davis wrote a famous book called *Deep South* by Allison Davis. It's still in print, by the way. It was the first major social and cultural study of black society, black social . . . social organization, communities in the South. Davis and Warner . . . Warner was involved in that, too; Davis and Warner. And that . . . there was a whole group at the University of Chicago interested in . . . in studying black society.

That is, America Negro culture in society, you see. There was a center in Chicago for that around Warner and Allis-. . . Allison Davis was really a kind of . . . he started out as a graduate student but he became . . . eventually he became a professor, and I think his . . . he . . . he didn't have a professorship in the university, he had one in the college as an undergraduate instructor. But he was . . . he was a colleague of Warner, in other words, from the standpoint of research. Any other name there?

CANFIELD: Okay. This is all the names that are here, but there's one more name, John. We have to talk about Sol Tax . . .

BENNETT: Okay.

CANFIELD: . . . that is . . .

BENNETT: Now, that is important. See, I think this is what we're closing on now.

CANFIELD: Yeah. Okay.

BENNETT: See, Sol Tax had been involved in applied anthropology for a long time. He served in . . . as an officer of the organization at least one term. I don't know whether he was president of the society, probably was, but he was always in favor of applied anthropology in some fashion or another. The problem was that Sol didn't like the applied anthropologists and the reason was very simple. It was, again, this heritage from the business school and other things that suggested to him that applied anthropology was really reactionary, politically and economically reactionary. They weren't really taking up the cudgels for liberation or freedom or prosperity or whatever for these oppressed millions in other parts of the world. Now, Tax was a kind of radical. He was a sort of a closet radical in a way because he never came out in the open and said, "Look, I'm a socialist," or "Look, I'm a communist." He never said those things. He never made a

political speech to my knowledge, but he made academic speeches which were essentially political. He felt . . . now, he invented . . . he got money to do a study of the Fox Indians in Iowa just across the Mississippi River from Wisconsin, a big Fox Reservation over there. And Tax and some . . . some of the . . . some of the radically oriented graduate students at the University of Chicago decided to do something about the Fox Reservation. It was in bad shape and the Fox were in bad shape, and the Fox were doing the wrong things in the reservation. They were not exploiting American government policies in their own interest, for example, and so on. So Tax set up the . . . what he called Action Anthropology Program in the Fox Reservation, and action anthropology meant exactly what it said. You do action, you do things for people, see, not applied anthro-. . . see, applied anthropology . . . the thing that Tax disagreed with was applied anthropology was an academic concept. You took a . . . a principal in anthropology and applied it to some real situation, but that . . . that was ambiguous, you know. It didn't say what you were really doing. Were . . . were you helping people or making things worse for them? See, Tax personally felt that a lot of the applied anthropology programs around the country or around the world had actually done damage to these societies because the . . . the people involved, the anthropologists had no political training or knowledge, you see. So he was going to change all of this, so he set up an Action Anthropology Program which involved getting . . . involving the Tax . . . the . . . the Fox people in conferences with Tax and his students, and trying to get . . . the students and Tax trying to get the Fox leaders and other people, other ordinary Fox people, to articulate their needs, and then get them to verbalize what they should do about those needs, you see. This was not applied anthropology. This was radical action, you

see. Now Tax, in the course of the . . . several years . . . I think it was six or seven years that the Action . . . Action Anthropology Program lasted. It didn't last a hell of a long time. The students fanned out and so on. They wrote a lot of stuff and there's a famous – it's quoted here in my book, cited in my book – there's a famous documentary collection at the University of Chicago Libraries on hundreds of documents that were generated in the Action Anthropology Project. It's never been published. No, parts of it have been published as a matter of fact. Some of the former action anthropologist students of Tax published, but I can't remember their names now. Well, Susan Tax, his daughter, was involved in this; that much I know. But there was another guy. It doesn't matter. I have in my book . . . in that chapter here I have a small bibliography of the stuff, but . . . and I'm sure other people do, too. It's not unknown. The important thing is that Tax really was trying to radicalize . . . in a way he was trying to radicalize applied anthropology. And I think that . . . if I had been . . . at that time I was no longer really . . . I was doing research. I was doing . . . well, I had . . . I was in Japan when he started the project really, and I think that . . . I felt that . . . I felt that the kind of research that I had been doing, what . . . what you would call, I think, applied . . . applied contract research, you see, . . .

CANFIELD: I see.

BENNETT: . . . was probably equally important. We were doing it in the context of a very powerful organization, the Japan . . . the Japan occupation, you see. And what I . . . when I saw . . . when I came back I was engaged, really, in building anthropology at Ohio State for one thing, and also I started work on agriculture in those days. Now I felt that working . . . for an anthropologist working in the agricultural field, I was making a

contribution to farmers and farming by helping improve their ways of doing things and their access to public policies which benefited them, you see.

CANFIELD: And . . . but that was a . . . you know, now you're tal-. . . talking about something I don't have a clear sense of, but I remember you . . . you've written some stuff on far-. . .

BENNETT: Well, see, I went off on these junkets for the Agricultural Development Council . . .

CANFIELD: Okay.

BENNETT: . . . in the 1960s, two world tours as a matter of fact, studying cooperatives and other things, you see. See, my definition, in other words, of applied anthropology was to facilitate access to researchers. I felt that this was the major issue, see. But there was another issue growing during the '60s and '70s, and that was environment, see, and I was very much preoccupied with environment. I felt that you had to protect the physical resources as well as try to get economic resources. And that was a very complicated switch kind of deal, you know. I mean if you . . . you improve access to physical resources, you may be endangering the environment. So I began to try to find a balance between these things. Were applied anthropologists doing something? Well, yeah, some of them were engaged in this but not very vigorously, in my opinion. I think it was during . . . I can tell you this much. During the late '60s and '70s I kind of lost interest in what was called applied anthropology. And the reason was, again, as I implied earlier today, development. Development work was being carried on pre-. . . predominantly by government organizations like U.S.A.I.D and by economists, and they were hiring anthropologists but to do trivial research, like doing little . . . little . . . little

studies of how people would like a better well in their community, see, and that sort of thing. In other words, a lot of the work was not really Tax-type action stuff or . . . nor was it really resource access and resource protection activity, which involved public policies as well as trying to change local habits, you know.

CANFIELD: Right.

BENNETT: It seemed to me to have lost . . . applied anthropology seemed to me to have lost its mission in the . . . in the late . . . in the late „60s and on through. It began in the late „50s, really, and went through the „60s and into the „70s and „80s. I think there was another factor, too. By the 1980s I think that general anthropology took over a lot of applied anthropology right here in this . . . in this department.

CANFIELD: Right.

BENNETT: I mean the kinds of projects that Bowen sends his people on are essentially what ten years ago would've been called applied anthropology. Now you call it cultural anthropology, you see. So that I think that applied anthropology not only lost its mission and became a kind of a routine search for trivial improvements here and there, but I think it . . . it got . . . a lot of it got taken over by general anthropology, you see. So what . . . in other words, where is applied anthropology today? I really don't know. I don't know whether there is any such thing anymore, you see. I mean Van Willigen and some of his people undoubtedly believe in it. More power to them, but I would personally recommend that they reinvestigate action anthropology.

CANFIELD: Hmm.

BENNETT: But, of course, you know, the . . . the big ob-. . . the big objection to Tax's stuff was political on the part of anthropologists. They didn't want to get involved in this

kind of stuff because it was dangerous, politically dangerous. Their administrations and their universities wouldn't hear of anything like that, you see, and meddling with people's life may . . . making people . . . making people start a little revolution, that kind of thing. And see, there were a number of other projects related to action anthropology. For example, one is the . . . the . . . the Cornell project in Peru. There was . . . that was a real disaster, you know, because they . . . they . . . the people in the project, in the Cornell project, were sort of action anthropologists without saying so, and they stimulated the local farmers in this Peruvian valley to protest politically, you see, and things began getting out of hand. If you remember the story, it was all . . . pieces of it all appeared in . . . in *Human Organization*. *Human Organization* is a journal, by the way. I think it's been a useful journal. It's kept up with the field, good or bad, you know. And it has also published documents about these political and paradoxical aspects of applied anthropology, you see.

CANFIELD: Umhmm.

BENNETT: Its diminishing importance, underdevelopment studies, the controversies over Tax's action anthropology is [in] *Human Organization*, I think. And then, also, the interdisciplinary stuff came out from time to time. Action . . . I think that *Human Organization* is a good journal, an interesting journal, and everybody ought . . . every anthropologist ought to look at it from time to time, even though I don't think it . . . I don't think it represents the field anymore because nobody knows what the field is, you see.

CANFIELD: Okay, John, is there anything you want to . . . you feel like we need to . . . to . . . to . . . to do to wind this up? Where . . . where would you . . . where would you put . . . anything you want to . . .

BENNETT: Well, I would say that I think the social sciences in general should have more of a social conscience, yes. I think the thing that is lost in applied anthropology . . . it is . . . it was never very prominent in anthropology, you see. As . . . as I said before, so much of early applied anthropology was really interdisciplinary research so it didn't have political implications. Not only that, but if . . . if the Harvard Business School was [chair squeaks] . . .

CANFIELD: Yeah, that's your chair.

BENNETT: . . . was part of the . . . part of the . . . part of the origins, you see, that . . . they were certainly not radical by any means.

CANFIELD: [Inaudible].

BENNETT: And as a matter of fact, if . . . if anything, they were reactionary. They felt that the best way to improve business organization was to apply social science to it, . . .

CANFIELD: Umhmm.

BENNETT: . . . which meant really co-opting the em-. . . em-. . . employees, see.

CANFIELD: Yeah. Yeah.

BENNETT: That's what people like Tax were objecting to. They and . . . and Eliot Chapple was a spokesman for this kind of stuff, see, so that actually, I think, applied anthropology sounded good but it never really amounted to very much. And from a standpoint of really helping people, . . . so . . . and then, of course, in the . . . in the later years the big government aid agencies began taking over and they were calling the tune

preparing, in other words, former tribal societies to really participate in the international capitalist market, see. So that applied anthropology has never had really very much to do with changing the way people live except in a purely material sense. And I think that anthropologists ought to reinvestigate developments like Tax's action anthropology and . . . difficult as it may be, but I think there's still something there. There's a missing piece in the whole business of the relationship of anthropology to the real . . . the real world, see.

CANFIELD: John, I want to . . . I want to bring one thing back to . . . to this [and] that is, you and Tax both come from Milwaukee, and you were saying something to me about Ta- . . . Tax's own background.

BENNETT: Oh, well, see, that's simply the origin of Tax . . . Tax's radicalism. See, Milwaukee was an unusual environment. It's . . . parts of it [are] here in my book.

CANFIELD: Okay.

BENNETT: But Milwaukee was an unusual environment. It was a known German socialist enclave with German socialism dating from the 1848 period. It was not socialism as we thought of it in later years. It was a very modest kind of radicalism in which you did things for people, in which you introduced social programs, you directed budgetary funds into useful social affairs rather than just building big city halls and that sort of thing, you see. It was much more than that, and Milwaukee had so- . . . "socialist," quote unquote, mayors for many years. And Tax's family . . . Tax's father had been one of these . . . these German socialists, and Tax . . . Tax grew up in a family in which public service and social service was considered to be a [inaudible]. That's what you did.

CANFIELD: Right.

BENNETT: If you become a scholar, you implement that. And his first project as a teenager in Milwaukee was to organize the union of all the . . . the paperboys that deliver . . . paper delivery boys and he was successful. He really did organize it. He got the damn papers, the Sent-. . . the *Milwaukee Sentinel* and the *Milwaukee Journal* to recognize these . . . these paperboys and to accept . . . accept negotiations over wages. They were . . . they weren't being paid anything, practically, you see. It was considered to be a . . . a . . . the social benefit of running around the neighborhood and helping out newspaper companies was considered to be enough in its own right. But he changed that. And old Sol really changed that, and so he started out as a teenager with the union organization program, see. Now, he could have . . . he could have continued that the rest of his life, union organization. He could have gone in the union organization, could have been a big shot probably, but he got involved in . . . in . . . in academia. I don't know how he got involved in academia. Why he selected an academic career, I really don't know. What I do remember is that when I was a graduate student in anthropology at Chicago, there was Sol, and I'd had a brief contact with him in Milwaukee much, much earlier, but a very brief contact. It's described in *Classic Anthropology*, their book. But the point was that . . . that at Chicago he was Redfield's handyman. I think that's the way to put it. He'd been Redfield's graduate student in the field and had done studies in Yucatan. Part of his work was done . . . part of his di-. . . work really appears in Redfield's book, *The Folk Culture of Yucatan*. And Tax did a lot of the field work on that study. Then Tax went to Guatemala. See, Redfield got him to go to Guatemala in order to do comparable studies of folk societies and the transformation of folk societies, and Tax's thesis was on that. Tax's thesis was on . . .

CANFIELD: Was it *Penny Capitalism*?

BENNETT: . . . *Penny Capitalism*, which was about, you know, the way these tribal merchants would travel around Guatemala and . . . selling stuff and so on, you see. A very interesting . . . very interesting monograph, by the way. Fascinating. I wish he'd stayed by that. But the point was that he was under Redfield's control, his thumb, and I think that his inherent radicalism for at least a decade was just unexpressed. And he had a little desk at the end of the hall near Fay Cooper-Cole's office, and he was . . . was doing things there, you know, writing things, working up data, you see. He was the odd man out. Gradually as Redfield withdrew and went into the university administration, Tax emerged, and eventually Tax became chairman of the department.

CANFIELD: Yeah.

BENNETT: It took a long time for that. And then Tax got deeply involved in all these wild professional activities all over the place, you know, organizing international conferences, the Current Anthropology Affairs, see.

CANFIELD: That's right.

BENNETT: The . . . I mean he became "Mr. Anthropology" and, of course, there was always rivalry . . .

[End of interview]