

DEMOGRAPHIC DESTINIES

Interviews with Presidents and Secretary-Treasurers of the Population Association of America

PAA Oral History Project

Volume 1--Presidents
Number 2--From 1961 through 1976

Prepared by

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PAA Historian 1982 to 1994

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ABOUT THE PAA ORAL HISTORY PROJECT AND THESE INTERVIEWS

This series of interviews with past presidents and secretary-treasurers and a few others for the oral history project of the Population Association of America is the brainchild of Anders Lunde, without whom PAA would scarcely have a record of its 60year history. Dismayed by the dearth of usable PAA files he inherited as secretary-treasurer in 1965-68, Andy later determined to capture at least the reminiscences of some of PAA's longest-time members. When written pleas yielded few results, he set about doing taped interviews with past presidents and secretary-treasurers and conducted over a dozen (with help from Abbott Ferriss and Harry Rosenberg) between 1973 and 1979.

Andy also assembled core records of meetings, membership numbers and officers and Board members since PAA's founding in 1931. He established PAA's official archives and arranged--with the help of Tom Merrick and Conrad Taeuber--for their cataloguing and deposit in the Georgetown University library. *[Note: the archives were removed from Georgetown University in the late 1990s, and are now housed in a storage unit rented by the Population Association of America, accessible through the Executive Director of the PAA.]* With Con Taeuber, he organized the "PAA at Age 50" session at the 1981 50th anniversary meeting in Washington, which produced four valuable papers on early PAA history by Frank Notestein, Frank Lorimer, Clyde Kiser, and Andy himself (published in *Population index*, Fall 1981). And he launched "Vignettes of PAA History," of which 19 have appeared in *PAA Affairs* since 1981. *[Note: three more appeared in the PAA Affairs in the 1990s written by John Weeks.]*

Retired in Chapel Hill, NC (and now a renowned creator of and writer on whirligigs), Andy asked me to take over as PAA historian in March 1982. I was eager to resume the interview series, but had no time until my retirement in 1987 as editor of the *Population Bulletin* of the Population Reference Bureau. Since January 1988, with the PAA Board's blessing and full cooperation of the interviewees, I have added 41 interviews to Andy's list, including reinterviews with six on his list-Ansley Coale, Kingsley Davis, Ronald Freedman, Dudley Kirk, Henry Shryock, and Conrad Taeuber [supplemented in 1992-93 with interviews of Ron Rindfuss, Etienne van de Walle, and Barbara Foley Wilson].

Originally, my goal, as was Andy's, was to have the tapes and transcripts for the record, safely deposited in the archives and available, of course, to "scholars," and also, as reported to the Board in my "project proposal" of March 20, 1987, to print excerpts from the interview transcripts along with other materials on PAA's history in a "Selective History of PAA." However, I found the interviews fascinating as I worked along, knew other demographers would too, and realized that their full flavor and easy accessibility would be lost in this excerpting and archiving. So I decided to assemble this set of the full edited transcripts. (The tapes and transcripts will still go into the archives and excerpts from the transcripts will appear in several "history vignettes" in *PAA Affairs*, but I have now dropped the plan for a printed "Selective History." The "other materials" that were to be included were collected by former PAA Administrator Jen Suter in a special file available in the PAA office.)

The 49 transcripts presented here cover 36 of PAA's 53 presidents through 1990 [updated to 1993], 14 of the 19 secretaries or secretary-treasurers (four of whom were also president), and four others specially interviewed for the series--Deborah Freedman, Alice Goldstein, Anne Lee, and Lincoln Day. This includes all presidents and secretary-treasurers still living as of 1993, except Evelyn Kitagawa (alas, we missed on four tries at a meeting) and Calvin Schmid (now in a nursing home in Washington state—*note: Calvin died in 1995*). I was able to interview most people at their home base (which involved some interesting travels). Nathan Keyfitz, in Indonesia, and Kurt Mayer, in

Switzerland, kindly supplied "self-interviews," following questionnaires which I sent them. All my interviewees edited their transcripts. I did further light editing to tighten them up and added explanatory notes and book titles, etc. All my interviewees signed "release" letters, indicating their agreement to having the edited transcripts released into the public domain.

Going beyond Andy Lunde's original aim of focusing on PAA history, I asked these demographic luminaries about their own careers, recollections of others in the field, and views on key demographic issues over the years they have been involved and for the future, in addition to their reminiscences about PAA. The results provide some unique insights into the personalities, situations, and issues that have gone into the making of demography in the United States as well as of PAA. This will be valuable input to the full histories of U.S. demography and of PAA that should be written someday. Meanwhile, the transcripts make for great browsing. (I recommend a strong reading table to support their hefty volume.) As Jane Menken put it: "Demographers are such *interesting* people," and, I would add, marvelously interconnected, as confirmed over and over in these interviews.

The 46 photos of interviewees [[see the file: "PAA History Photos Pres & SecTreas 1947-1993.PDF"](#)] also included come from Princeton's Office of Population Research, the Population Reference Bureau (by Art Haupt, former editor of *Population Today*), Henry Shryock (photo of John Durand and Frank Lorimer at the 1942 PAA meeting in Atlantic City), George Myers (photo of Joseph Spengler), several from interviewees themselves, and the rest I took at the time of the interviews.

I am grateful to Andy Lunde for conceiving this project, to my interviewees for their ready cooperation, to Population Reference Bureau librarian Nazy Roudi and other PRB people for their expert and cheerful help in tracking down background material, to Conrad Taeuber, Henry Shryock, Suzanne Bianchi, Paul Glick, and Reynolds Farley for encouragement and special help, to the PAA board and current officers for their "enthusiastic" support and two subsidies toward my work on the oral history project, to Joe Brennan, Kathryn Murray, and Artmaster Printers for skilled help with the production of this transcript set, to (former) PAA Administrator Jen Suter for kindly agreeing to handle requests for the set, and especially to my husband, Herman, without whose understanding and financial support this project could never have been accomplished.

Jean van ter Tak ("VDT")

PAA Historian Washington, D.C. (May 1991, updated November 1991)

ABOUT "VDT": I am Canadian and got a B.A. in history from the University of Toronto in 1948 and an M.A. in demography from Georgetown in 1970. I have worked with the Oxford University Press in Toronto and London (where I met my Dutch husband, then at the London School of Economics), the Population Crisis Committee, the Netherlands Interuniversity Demographic Institute, Georgetown's Center for Population Research, the Transnational Family Research Institute (where I wrote and edited books and articles on demographic aspects of abortion and contraception), and 12 years at the Population Reference Bureau. My economist husband-long at the World Bank and now consulting part-time for the Bank-and I have three sons, three daughters-in-law, and two grandchildren. We have lived in London, Geneva, Bangkok, and since 1961 in Washington-with a sabbatical year, 1970-71, in the Netherlands. We retired early in order to travel energetically and in the past four years have hiked, camped, camel-treked, sailed, birdwatched, etc., on all seven continents.

THE PAA PRESIDENTS

	Years	President	Interview date, place, interviewer	Page
1	1931-35	Henry Pratt Fairchild	No interview	
2	1935-36	Louis I. Dublin	No interview	
3	1936-38	Warren S. Thompson	No interview	
4	1938-39	Alfred J. Lotka	No interview	
5	1939-40	Leon E. Truesdell	No interview	
6	1940-41	T. J. Woofter, Jr.	No interview	
7	1941-42	P. K. Whelpton	No interview	
8	1942-45	Lowell J. Reed	No interview	
9	1945-46	Frank H. Hankins	No interview	
10	1946-47	Frank Lorimer	No interview	
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11	1947-48	Frank Notestein	4/27/73, New Orleans, Lunde	
12	1948-49	Conrad Taeuber	12/5/73, Research Triangle Park, NC, Lunde	
13	1949-50	Frederick Osborn	No interview	
14	1950-51	Philip M. Hauser	11/12/88, Chicago, VDT	
15	1951-52	Rupert B. Vance	No interview	
16	1952-53	Clyde V. Kiser	4/26/73, New Orleans; 12/15/76, Chapel Hill, Harry Rosenberg (with Hamilton and Spengler)	
17	1953-54	Irene B. Taeuber	4/28/73, New Orleans, Lunde	
18	1954-55	Margaret J. Hagood	No interview	
19	1955-56	Henry Shryock, Jr.	4/27/73, New Orleans, Lunde; 4/8/88, Washington, DC, VDT	
20	1956-57	Joseph J. Spengler	12/15/76, Chapel Hill, Rosenberg (with Hamilton and Kiser)	
21	1957-58	Harold F. Dorn	No interview	
22	1958-59	Dorothy S. Thomas	No interview	
23	1959-60	Dudley Kirk	4/27/79, Philadelphia, Lunde, and 4/29/89, Stanford, VDT	
24	1960-61	C. Horace Hamilton	12/15/76, Chapel Hill, Rosenberg (with Kiser and Spengler)	
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25	1961-62	John D. Durand	8/11/79, Spruce Pine, NC, Abbott Ferriss	<u>6</u>
26	1962-63	Kingsley Davis	4/26/79, Philadelphia, Lunde and Ferriss; 5/1/89, Stanford, VDT	<u>13</u>
27	1963-64	Donald J. Bogue	3/30/89, Baltimore, VDT	<u>39</u>
28	1964-65	Ronald Freedman	4/56/79, Philadelphia, Lunde; 6/12/89, Ann Arbor, VDT	<u>68</u>
29	1965-66	Calvin F. Schmid	No interview	
30	1966-67	Paul C. Glick	5/9/89, Phoenix, VDT	<u>101</u>
31	1967-68	Ansley J. Coale	4/27/79, Philadelphia, Lunde; 5/11/88, Princeton, VDT	<u>135</u>
32	1968-69	Otis Dudley Duncan	5/3/89, Santa Barbara, VDT	<u>164</u>
33	1969-70	Everett S. Lee	6/28/79, Athens, GA, Ferriss	<u>200</u>
34	1970-71	Nathan Keyfitz	12/31/88, Jarkata, Indonesia (self-interview re VDT)	<u>210</u>
35	1971-72	Amos Hawley	4/6/8/88, Chapel Hill, VDT	<u>231</u>

36	1972-73	Norman B. Ryder	5/11/88, Princeton, VDT	250
37	1973-74	Arthur Campbell	2/16/88, Washington, DC, VDT	278
38	1974-75	Charles F. Westoff	5/10/88, Princeton, VDT	292
39	1975-76	Sidney Goldstein	12/14/89, Providence, RI, VDT	313

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40	1977	Evelyn Kitagawa	No interview
41	1978	Richard Easterlin	5/4/89, Los Angeles, VDT
42	1979	Charles B. Nam	4/22/88, New Orleans, VDT
43	1980	Jacob S. Siegel	6/21/88, Washington, DC, VDT
44	1981	Judith Blake	5/4/89, Los Angeles, VDT
45	1982	John F. Kantner	3/22/88, Bedford, PA, VDT
46	1983	George F. Stolnitz	1/20/88, Washington, DC, VDT
47	1984	Samuel H. Preston	6/14/88, Philadelphia, VDT
48	1985	Jane A. Menken	6/13/88, Philadelphia, VDT
59	1986	Paul Demeny	6/8/88, New York, VDT
50	1987	Ronald D. Lee	4/28/89, Berkeley, VDT
51	1988	Reynolds Farley	2/4/89, Washington, DC, VDT
52	1989	Harriett B. Presser	11/15/89, College Park, MD, VDT
53	1990	Larry L. Bumpass	3/21/91, Washington, DC, VDT
54	1991	Ronald R. Rindfuss	5/1/92, Denver, VDT
55	1992	Etienne van de Walle	2/17/93, Philadelphia, VDT
56	1993	Albert Hermalin	2/17/93, Philadelphia, VDT

JOHN D. DURAND

PAA President in 1961-62 (No. 25). Interview with Abbott Ferriss at Dr. Durand's home in Spruce Pine, North Carolina, August 11, 1979.

CAREER HIGHLIGHTS: John Durand was born in 1913 in Washington, D.C., and died in 1981 in Spruce Pine, North Carolina. He received his B.A. in economics in 1933 from Cornell, where he was research assistant to Walter Willcox, and his Ph.D. in economics in 1939 from Princeton, where he was the first Milbank Memorial Fund Fellow at the Office for Population Research in 1936-39. Before going to Princeton and again from about 1939 to 1947, he was in the Population Division of the Bureau of the Census (of which his uncle, E. Dana Durand, had been Director during the Theodore Roosevelt administration). In 1947, he joined the newly-established Population Division of the United Nations as its first Assistant Director and was its third Director (following Frank Notestein and P.K. Whelpton) from 1953 to 1965. Among other groundbreaking demographic activities at the UN, he directed the making of the landmark Determinants and Consequences of Population Trends (1953; revised edition, 1973), launched World Population Perspectives, conceived and raised initial outside funding for the UN regional centers for demographic research and training, and organized, in collaboration with the IUSSP, the first World Population Conference in Rome in 1954 and the second such conference in Belgrade in 1965. From 1965 to 1979, he was Professor of Economics and Sociology and Director of the Population Studies Center at the University of Pennsylvania. His publications, focused mainly on the labor force, included The Labor Force in the United States, 1890 to 1960 (1948) and The Labor Force in Economic Development: A Comparison of International Census Data (1975). At the time of his death, he was working on a history of world population, of which a first chapter, "Historical Estimates of World Population: An Evaluation," appeared in Population and Development Review in September 1977.

FERRISS: John, you were president of the Population Association of America in 1961-62. Can you recall any incidents during that term of office that particularly interested you?

DURAND: I think it was a very uneventful year. I can't remember any events or incidents whatsoever.

FERRISS: You were at the University of Pennsylvania at that time, weren't you?

DURAND: No, I was at the United Nations. My association with the UN began in 1947. Preparations were being made for the first session of the Population Commission, which was to be in February 1947. Frank Notestein was acting as in charge of the Population Division, the secretariat of the Population Commission. He was still at Princeton's Office of Population Research. He was really only a consultant. I was working at the Bureau of the Census and Frank was trying to draw up a little staff for this Population Division. So he came down to Washington in January 1947 and proposed to me to come up and join the staff. I had some hesitation, but I finally decided I'd be happy to do that. I'm very glad I did. We organized the first session of the Population Commission, which was at Lake Success. There were about a hundred people there.

So that's how I got started at the UN and I remained there for 19 years. I left the UN in 1965 to go to the University of Pennsylvania. The year of my presidency of the Population Association occurred during that interval.

As I said, it was a very uneventful year for the Association, as far as I can recall. I can only remember the annual meeting in Madison [May 4-5, 1962] and that was very nice.

FERRISS: Was it well attended?

DURAND: Yes, there were plenty of people and it was a beautiful meeting, or so it struck me. I remember very especially the hospitality of the Wisconsin group.

FERRISS: The tradition at that time was to meet at universities, was it not, rather than in a hotel?

DURAND: Well, there had been some meetings not at universities; in Washington [1935, 1939, 1960]--that wasn't particularly a university. It was more that it was the capital.

FERRISS: You became interested in demography when you were a student at what university?

DURAND: I was a student at the University of North Carolina at Chapel Hill, in 1931, if my memory is not tricking me. I was a sophomore. And we didn't have any money; I needed a job to continue my education. I had an uncle at Cornell, professor there, who was a friend of Professor Walter Willcox. Willcox had just retired as professor and was undertaking a historical study of the vital statistics of the United States. This was published, by the way [Studies in American Demography, 1940?], but I very rarely see a copy of it. I haven't got a copy myself and I don't find copies in the libraries. It seemed to me an interesting project. He was studying historically not only the development of statistics but mainly the trends of the vital rates of the United States, going back into the 19th century.

So I got a job, through my uncle's help, assisting Professor Willcox with this study. I moved to Cornell University and I worked part-time, 20 hours a week, earning--as I recall--25 cents an hour. My job, initially, was to punch the calculating machine and write down the figures I got.

That already I began to find kind of interesting. I remember I was grinding out the birth and death rates for the counties of North Carolina. At first the people were born; you just put in the figures and write down the answer, hour after hour; it could get a little soporific. But after a while, I began to think that these statistics were showing some interesting things and it made me wonder why. Up in these mountain counties the death rate was very low--way down, like 4, 5, 6--and when you got down to lower altitudes the death rate was higher. I was struck. I said, "They're supposed to be very healthy [in the mountains], but are they all that healthy?" So I asked Professor Willcox, "How come they're so low?" And he says, "Well, look," he showed me, "Under ideal conditions if the death rate were 4, the average length of life would be 250 years." So that was impossible. He led me along this way to the conclusion that the reason why the death rates were so low was obviously because they weren't counting all those deaths. This sort of discussion that he would lead me into attracted me. He'd give me a little leading question or part of an answer to develop my interest, which he succeeded very greatly in doing.

This was the famous Professor Willcox, an old, old man already at that time.

So I continued for the rest of my undergraduate career at Cornell, working part-time for other professors on assignments similar to that, and that led to a growing interest in the subject matter of this study. So that's how I became interested in demography.

FERRISS: You majored in sociology or economics?

DURAND: I majored in economics, but I didn't learn any economics there at all. I learned some other things. They tried to teach me economics, but whatever I learned in that line I immediately forgot. And the same with statistics; I think a little more statistics than economics, but very little indeed.

But I got the degree--sort of a C-average kind of a deal. Then I got a job there, working for a

while at the public utility, Associated Gas and Electric Company, keeping a record of some stock transfer deal that they were foisting off on their customers. I worked on that for about half a year. Then I succeeded sufficiently in the civil service exam to get taken on at the Bureau of the Census to work under Leon Truesdell. This was in 1933. I had various jobs I was given.

There are certain people that I want to mention that I owe a lot to. They encouraged me and whatever I've amounted to as a demographer, I owe to them. I've already mentioned Walter Willcox and now I mention Leon Truesdell. I really owe a great deal to him. I was one of his young assistants for half a dozen years. Well, now, it was only three years before Frank Notestein--there's the third man I have to mention with great gratitude.

Frank Notestein came to the Bureau of the Census and I was in a little office with Henry Shryock. Frank was looking for staff because he was trying to set up at that time another new thing, the Office of Population Research at Princeton University. It didn't exist yet, but they'd gotten some funding for it and he was trying to build it up and he had to have a helper. He was interested in interviewing Henry, proposing him as a candidate for his assistant, and Henry did take that job. I was sitting there and Frank noticed me and said, "We also got money for a fellowship for a graduate student in demography" and would I be interested in applying. [John Durand was the first Milbank Memorial Fund Fellow at OPR.] If I was, they would consider my application, along with others, for this fellowship. So I said, "Sure," and I applied and I got it. He took me! Well, my gratitude to Frank Notestein begins there.

So the next three years, I was at Princeton with Henry Shryock and Frank Notestein. I was supposed to be a graduate student in demography, but actually, they had no demography program. They had no sociology program but, of course, they had their economics, so I was a candidate for a degree in economics. I got my Ph.D. in 1939 in economics.

During those three years, I was supposed to be learning demography in addition. Well, I did learn some demography, just in a kind of very haphazard way by being around Frank Notestein. Frank is very talkative and he would discuss at the drop of a hat most anything having to do with population questions. Occasionally, he would mention something and say, "Go read about that." That was the sort of instruction in demography I received at the university; whatever I learned was pretty haphazard. No doubt this accounts for a good deal of my professional deficiencies. I was given no systematic academic instruction in demography. I picked up what I could.

Another way I learned there was by working on the Population Index. Part of my job in addition to studying was to assist in the correction of this, four issues a year. There was Irene Taeuber down in Washington in the Library of Congress, getting up the bibliographic items. She would put them on these little three-by-five slips of paper--not cards, slips of paper--with titles, whatever was to be that citation, and she'd send them up. We'd be confronted with this stack of slips of paper. They had to be sorted, made ready to type, then typed, verified and all this, put together, photographed, and made into the Index. Well, I think that's one way to learn about demography and the literature. At least I got acquainted with the names of people who were writing about that field. There weren't as many as there are now. So I knew most of the names of the authors after I'd been working in that way on the Index for three years. Maybe I learned more that way than any other.

FERRISS: You classified and arranged them?

DURAND: Yes, we classified them according to subject and author, and read them to see if the citation made sense, try to get it correct, check up on things where there was a question. There was a typist there who would type it. Then we would proofread the typing and set up the physical copy for reproduction.

FERRISS: You must have had some research responsibilities too, or you couldn't have tried to write a dissertation.

DURAND: I was always doing a little project, like graduate students do. I got some coaching from Frank--and from Henry. I also add Henry Shryock to my list of men that I owe gratitude to.

Let me complete that list of all the ones I want specially to mention. There's Frank, and I owe a second debt of gratitude to Frank, who brought me to the United Nations.

Then at the United Nations, there's Pat Whelpton. After a couple of years, Frank found he could no longer spend the time on that and he quit his part-time attachment to the UN [in 1949] and went back full-time to Princeton; and he left me. There I was, acting director of the Population Division that I was putting up. I struggled along the best I could and was always trying to find an appropriately senior, outstanding expert in demography to take over the directorship of that outfit. Well, they got Pat Whelpton. He agreed finally to come and he stayed there for several years [till 1953].

FERRISS: What year was this?

DURAND: Pat came about 1951 or 52. I came to the UN in 1947. Frank would have left about 1949. There was an interval and after about two years or so, Pat came. Pat stayed two or three years and then he faded away and left me.

FERRISS: Does that complete your list?

DURAND: No, I will go on to the university. I left the UN in 1965. I began to feel that it was time for me to leave the UN, particularly in view of the ways in which the work of the United Nations was evolving, directions in which I didn't feel I was very able to serve. It was becoming more and more related to action rather than research, big programs of action, not just studies. I'd felt more at home when the idea was to make demographic studies.

I don't think we did too badly in those that I was there. I think we put out some pretty good things. One especially that is well known is The Determinants and Consequences of Population Trends [1953]. And then I could mention the Mysore population study and a few others. I think they were substantial contributions.

But it began to change and I felt I was not really the man to try to continue along that line; I'd be better at the universities. So when the University of Pennsylvania people asked me if I'd like to come down, I decided to accept.

So now I'll add two more names and then I will have finished the list of people to whom I really feel I owe great debts of gratitude: Dorothy Swaine Thomas and Richard Easterlin at the University of Pennsylvania.

FERRISS: When did you first become associated with PAA, that is, what is your first recollection of the Association?

DURAND: In my early years at the Bureau of the Census, frankly, I don't believe I was aware of the existence of the PAA. When did the PAA start; 1933, was it?

FERRISS: Something like that.

DURAND: About the same time I was graduating from Cornell. As far as I can recall, I didn't know

that such a thing as the Population Association of America existed.

I think that the Association came into my consciousness when I was at Princeton, but it was very much in my consciousness there. You said that one of the questions you'd ask me was, "What were the most interesting meetings of the PAA?" Well, I'll put that back in 1937, 1938, 1939--somewhere along in there while I was at Princeton--when PAA met at Princeton. [The Princeton meeting of those years was May 6-8, 1938.] It held its meeting at the Princeton Inn, a little hotel. I was a kid, you know, I was a student. But there weren't that many kids because there weren't that many students. This was an association of greybeards--well, Frank Notestein and a couple of others were not greybeards, actually. But there were others who were greybearded, and there were very few kids. I think their attitude was, "Oh, here's this youngster, or pair of youngsters. We need young fellows." They really enjoyed it, and it was very nice for me to be treated as a kid. I suppose now there are so many kids, young people, graduate students around that it wouldn't be the same. All the principal demographers there were at the time were there. There were, I'd say, about 30 or 40 people at this meeting. And I could be acquainted with and talk to all of them. They were willing to talk to me--most of them. This was a great thing for a youngster.

You could say I was fortunate there to be kind of in on the ground floor. Not really ground floor, because the ground floor was constituted by those founding members, such as my mentors, Walter Willcox, Leon Truesdell, Frank Notestein, and Pat Whelpton, but pretty close to the ground--on the second story at the most. And the whole thing was still small, friendly. So I enjoyed that. I look back on that meeting as the meeting nonpareil in my life, and, of course, there were a lot of other meetings.

FERRISS: Do you recall who was president at that meeting?

DURAND: Nope.

FERRISS: Alfred Lotka was president in 1938-39; Warren Thompson in 1936-38.

DURAND: It would have to be Thompson [yes], or Lotka.

FERRISS: Did you read a paper at one of those early meetings?

DURAND: No, I sat and listened.

FERRISS: Do you recall any incidents in the discussion; any arguments that ensued?

DURAND: Not much. I can only think of two arguments at a Population Association meeting. One was at a meeting in New York City, around the mid-1950s. [Most likely the New York meeting of 1961.] What I recall has to do with Frank Lorimer and Kingsley Davis. Kingsley was talking about Ireland and he hadn't exactly put Ireland in the most favorable light. He said some things about the nature of the Irish, the character of that country, that seemed to impel Frank Lorimer to come to the defense of Ireland. He got up and said, "The Irish are not all that bad," and went on to point out their attractions--a certain amount of poetry, certain amount of music, and so on--the qualities of Ireland and the Irish.

That night, Frank came up to supper at our house, in Scarsdale, and sat in our kitchen. And he was full of remorse, because he felt that he had misbehaved in the meeting and allowed himself to get drawn into an argument that was not a substantive demographic argument and therefore was out of place at the meeting. He was very contrite. And I assured him, "Oh, Frank, you're a good fellow; your

heart is of gold. Because you felt that Ireland was being mis-viewed, it's all right that you should have said what you did; you didn't overdo it." So I remember this argument and that's one of the primary ones that I recall.

The other one has to do with--and I could almost include this man in that list I gave you of people I owe debts of gratitude to--Simon Kuznets. At one of the meetings and I can't remember which one, Simon presented early results of his study of the long waves, long cycles. He'd been studying the long cycles, economic indicators, and had observed that there were parallel long cycles, waves, variations, in international migration. He was looking at that from the demographic aspect, these long cycles. And nobody would pay much attention to Simon on this. The argument was about whether these cycles were real and Simon was defending his conclusion. He didn't get much of a hearing. I remember that and felt at the time that their attitude was, "Look, Professor Kuznets, of course, you're an honorable economist but you're among demographers now, so you need to get a little better acquainted with demography." It was many years before Simon was accepted by demographers.

FERRISS: The methods for identifying those cycles were not too developed at that point.

DURAND: That's true. Easterlin went on to do more research on that.

FERRISS: During your term of office as PAA president, you don't recall any issues or problems?

DURAND: I'm sure there was nothing very major.

FERRISS: Horace Hamilton was president the year before you.

DURAND: Horace's presidential address was not much of a presidential address ["Some Problems of Methods in Migration Research"]. I've listened to others. Richard Easterlin's address at Atlanta [1978], I thought was an outstanding one ["What will 1984 be like?"]. There are really many contributions in that presidential address. Frankly, I feel my presidential address was not an academic speech ["Demography's Three Hundredth Anniversary"--300 years since publication of John Graunt's book, Natural and Political Observations . . . Made on the Bills of Mortality]. I think it was not too badly received as such. People were amused, but it wasn't really a weighty thing.

FERRISS: That was presented at a dinner, was it not?

DURAND: Yes.

FERRISS: Horace Hamilton was president the year before you. Did he pass on any problems or any advice to you?

DURAND: I don't remember. All I remember was Horace introducing me at the annual meeting the following year.

FERRISS: Kingsley Davis took over the year after you, 1962-63. Did you transmit any . . .

DURAND: Legacy to Kingsley? I doubt if I had any fatherly advice to pass on to Kingsley. I can't remember.

I believe the job of president of PAA at that time was not so demanding as it has since become with the extension of the activities of the Association. I doubt that my particular year of presidency

was all that much more uneventful than another one. I don't think there was that much to be done. There must have been some files that had to be passed on to Kingsley that I might have received from Horace. I don't remember any of that. There weren't a whole bunch of committees like there are now.

What was your part of the annual meeting? That was the after-dinner speech like I gave; that was your part of the affair. And that was the entire year. Somebody gave a little news or announcement or maybe there would be a committee reporting at the business meeting, and that would be it. Not like it is now, when there's so much business that you can't crowd it all into a business meeting and the Board of Directors is harrassed by all these difficult decisions to be taken. It wasn't like that, I believe.

KINGSLEY DAVIS

PAA President in 1962-63 (No. 26). Interview with Jean van der Tak in Dr. Davis's office at the Hoover Institution, Stanford University, California, May 1, 1989, supplemented by corrections and additions to the original interview transcript and other materials supplied by Dr. Davis in May 1990.

CAREER HIGHLIGHTS: (Sections in quotes come from "An Attempt to Clarify Moves in Early Career," Kingsley Davis, May 1990.) Kingsley Davis was born and grew up in Texas. He received an A.B. in English in 1930 and an M.A. in philosophy in 1932 from the University of Texas. He then went to Harvard, where he received an M.A. in sociology in 1933 and the Ph.D. in sociology in 1936. He taught sociology at Smith College in 1934-36 and at Clark University in 1936-37. From 1937 to 1944, he was Chairman of the Department of Sociology at Pennsylvania State University, although he was on leave in 1940-41 and in 1942-44.

"I came to Harvard in 1932 with a fellowship in hand (the Bromfield Rogers Memorial Fellowship). By going to meetings and seminars, I quickly got acquainted in the new setting. In my second summer in Cambridge [Massachusetts], I was an assistant to Howard Becker, a young luminary in sociology who was regularly at Smith College. When Frank Hankins, chairman of the department at Smith, decided to take a year's sabbatical, Becker knew about me and asked if I would be interested in coming to Smith for a year. I agreed to come. The next year, Howard Becker took a leave and I substituted for him, giving me another year at Smith.

"When Becker was about to return to Smith, the Great Depression had reached its nadir. Nevertheless, I found a job at Clark University--a job I took because it was close to Cambridge and therefore would allow me to keep up my contacts at Harvard. However, before the end of the year [1936-37], I accepted a position as Head of the Division [Department?] of Sociology at Penn State. I got this job because the current incumbent, Willard Waller (well known at the time), was a good friend who believed I could do a good job at Penn State.

"I was in residence at Penn State for three years, 1937-40. I had, however, become interested in population studies and so I applied for a postdoctoral fellowship from the Social Science Research Council for further study in that field.

"During that year [1940-41], I was on leave from Penn State. [The year was spent at the University of Chicago, studying demography with Samuel Stouffer; in Puerto Rico, conducting a fertility survey; and at the Census Bureau in Washington.] The next year [1941-42], I returned to my regular job at Penn State.

"In the following year, as a result of coming to know Frank Notestein and others at Princeton, I was invited to come to Princeton for a year as a visiting research associate in the Office of Population Research, on leave from Penn State. This arrangement lasted for two years, at which time I was given an appointment to the Princeton faculty as an Associate Professor of

Public Affairs, while retaining my affiliation with OPR. After these two years on leave, in 1944 I resigned from my position at Penn State. [At Princeton, after 1944, he was Research Associate at OPR from 1944 to 1948, and from 1945 to 1948, Associate Professor of Anthropology and Sociology, a department which he started.]

"I accepted a position in the Graduate Faculty of Political Science at Columbia in 1948. [At Columbia, he was Associate Professor of Sociology, 1948-52; Professor, 1952-55; and also Associate Director, 1948-49, and Director, 1949-52, of the Bureau of Applied Social Research.] I went to [the University of California at Berkeley in 1955 as Professor of Sociology. Subsequently, in 1970, my title was changed to Ford Professor of Sociology and Comparative Studies. In 1977 this position was given to me emeritus. [Also at Berkeley, he chaired the Department of Sociology, 1961-63; founded and chaired International Population and Urban Research, 1956-77; and helped establish the Department of Demography.]

"In 1977 I went to the University of Southern California [Los Angeles] as Distinguished Professor of Sociology, a title I still hold. In 1980-81, I was a fellow, for the second time, at the Center for Advanced Study in the Behavioral Sciences [Stanford]. In 1981 I accepted a part-time appointment at the Hoover Institution [Stanford] as a Senior Research Fellow. I still hold this post as well as the position at USC."

Among Kingsley Davis's many other posts and awards relevant to demography, he was the second U.S. Representative to the United Nations Population Commission, 1954-61; he was president of the American Sociological Association in 1959 and has received ASA's Distinguished Career Award; he received the Irene Taeuber Award for Distinguished Research in Demography from PAA in 1979; and he was the first sociologist or demographer elected to the National Academy of Sciences, in 1966.

Throughout his career, starting in the mid-1930s, Kingsley Davis has produced a prodigious number of influential books, articles, book chapters, and conference papers on fertility, migration, urbanization, and practically all areas of demography. William Petersen has said that he was "a pioneer" in establishing the field of social demography (Petersen, "Kingsley Davis," International Encyclopedia of the Social Sciences, Biographical Supplement, Vol. 18, 1979, p. 139).

VDT: How did you first become interested in demography?

DAVIS: I got my Ph.D. in sociology at Harvard, studying mainly with Talcott Parsons. My dissertation was on comparative kinship systems. I did it under Lloyd Warner, who was at Harvard then. Just before I got my Ph.D., he left and went to Chicago, but I ended up writing the dissertation on the subject that he and I had agreed on. I published an article with Lloyd Warner in the American Anthropologist on my dissertation ["Structural Analysis of Kinship," American Anthropologist, April-June 1937]. So I was interested in comparative research, especially on social structure, before I got interested in population.

As you know, at the time there were interesting population policies, especially pronatalist policies, in Europe. My feeling was that these policies were naive when it came to the sociology of the family; they didn't know what they were doing. So I decided to publish an article and set them right. I wrote a critique, which was published in a British journal ["Reproductive Institutions and the Pressure for Population," Sociological Review, July 1937].

In doing that study, I read a lot of demography, especially on fertility, and I thought, "Well, this is an interesting field." It was concrete; one could get his hands on the phenomenon; it wasn't all verbal theory.

I continued to read in the field and to get acquainted with what was going on. When I went to Penn State as chairman of sociology, I could put in any course I wanted. I put in a course on population so I could learn the subject. And I did more reading and gradually became a demographer.

In 1940 I received a Social Science Research Council fellowship for postdoctoral study in the field so I could learn demographic techniques that I didn't know before. I spent that year partly at the University of Chicago with Samuel Stouffer. It was somewhat a mutual education, because Stouffer didn't really know much demography. He was more of a statistician; very interested in methodology.

Then I went to Puerto Rico because I wanted to get to know demography in a nonindustrial country. I spent that time interviewing people in the countryside [the region surrounding the mountain town of Lares]. I did a survey, but like other people I found that my cases became too few when I put the characteristics in.

VDT: When you wanted to analyze the data?

DAVIS: Yes. I was looking at fertility and why their fertility was so high, as it was at that time. But it was mostly practice; I was learning in the process.

VDT: Did you set that survey up on your own?

DAVIS: Yes. At first I had a very capable woman, a Puerto Rican nurse, helping me, but I got to where I could do the interviewing too. I had started learning Spanish some time before I went to Puerto Rico, knowing that I was going there.

I came back from Puerto Rico and went to the Census Bureau for four or five months to see how they were doing on the 1940 census.

VDT: That was the time that the "class of 1940," as they called them, came in--Paul Glick, Henry Shryock . . .

DAVIS: Right, they were all there.

VDT: So all that really plugged you into demography?

DAVIS: Yes.

VDT: Then you went back to Penn State for a year, and then what took you to Princeton?

DAVIS: The Office of Population Research. At a meeting of the PAA in Chapel Hill [1940], I got acquainted with Frank Notestein. He thought I looked like pretty good material. He had a contract with the State Department at the time to do special studies of particular areas. He and I talked and he seemed interested in my thoughts and ideas on Latin America. So he gave me a position at the Office of Population Research, mainly to do research on Latin America.

VDT: It was not thought at that time that you would do India?

DAVIS: No, although it wasn't long before India was included in my portfolio. [Davis's OPR research on India, culminating in The Population of India and Pakistan, 1951, was funded by OPR's contract with the Office of the Geographer of the Department of State, which began about 1945, after World War II ended and after OPR had produced four important books on the population of Europe for the League of Nations. The State Department contract was "to extend its (OPR's) work to Asia." Frank

Notestein, "Demography in the United States: A Partial Account of the Development of the Field," Population and Development Review, December 1982, pp. 664-665.]

VDT: How did you first become interested in India?

DAVIS: I was always teaching sociology and I was also working with this SSRC fellowship. I'd been interested in social stratification, a very important topic in American sociology. I did some work with Wilbert Moore on the theory of stratification. We did a much-debated article on that ["Some Principles of Stratification," American Sociological Review, April 1945].

When you're dealing with social stratification empirically, you can hardly ignore India, so I studied the caste system. And when it was time at the Office of Population Research to assign different areas to different people, I more or less staked a claim on India. I'm glad I did, because it proved to be a very interesting case, not only of stratification but of population as well.

VDT: So that's how you came to write the famous book, The Population of India and Pakistan [Princeton University Press, 1951]. Did you go to India?

DAVIS: Only after the book was published. I wanted to go see whether what I said was correct.

VDT: But how did you collect all the data; was it shipped over to you?

DAVIS: That's one of the interesting things about India. For its stage of development, it has better census data than any other country in the world. I wore out the census volumes borrowed from the Harvard library, especially the 1931 census; I practically wore it out.

VDT: But you also put in all the sociological insights and perspectives that nobody had done before.

DAVIS: Yes, it wasn't just all statistics.

VDT: Indeed, that was the wonderful thing about it. That book, as Ansley Coale among others has said, was a monumental book. It's still being reprinted; still a seller as a reference on the population of India. That book influenced the start of the Indian family planning program in 1952.

DAVIS: Yes. When I finally went to India, in 1951 or 52, I visited the Commission or Office on Economic Planning. They were planning a lot of things at that time, with a new government coming in after Independence [in 1947]. The Gandhi party had been concerned about population, believed in population policy. When I went into the office they took me into the seminar room, a big room with a big oval table, and I saw two or three copies of my book, dog-eared; they'd obviously been read. And we talked about population policy. They asked me, very seriously, if they started birth control in India--that was the term they used; "family planning" hadn't come in--would the population show the same immorality that Kinsey showed in American women?

VDT: They had seen the Kinsey report?

DAVIS: Yes, they're a literate people; they read a lot. So they were really concerned. I had to think fast. I told them that surely they did not think that the conduct of Indian women was based on ignorance.

VDT: Very good! In your book, you recommended that India should have a sustained and vigorous birth control program, along with emigration and rapid industrialization. You didn't really believe at that time that the government would take you up on it, but they did.

DAVIS: Right.

VDT: Not too vigorously at first.

DAVIS: Not vigorously. For years they worked on only one birth control means--rhythm.

VDT: Also in the book, you pointed out that India's population at the time of Independence in 1947 was 420 million and growing at 1.2 percent a year. If that rate continued, the population would double to 840 million in 2005, which you said would be a catastrophe. According to the Population Reference Bureau's World Population Data Sheet, India's population is estimated at 835 million this year, 1989, growing at 2.2 percent a year. Do you think that's a catastrophe?

DAVIS: Catastrophe all the way round.

VDT: They didn't listen hard enough to you.

DAVIS: No. I made the contrast with South Korea, Hong Kong, Singapore, Japan . . .

VDT: And China.

DAVIS: Yes, they've done a lot.

VDT: At Princeton, simultaneously with the India-Pakistan book, you also wrote Human Society [1949].

DAVIS: Yes. Well, I went to Princeton to do research at the Office of Population Research. Then they appointed me to the faculty at Princeton, in the Woodrow Wilson School of Public Affairs, made me an associate professor and asked me to do some teaching. I was to start the sociology and anthropology department.

VDT: I had the impression that Notestein really brought you on, as a sociologist, for that reason; that you could start the department.

DAVIS: It was a possibility. I got Wilbert Moore there, who had been at Penn State with me, Paul Hatt, and Marion Levy. [Also Melvin Tumin, Edward Devereaux, and Harry Bredemeier--all "men who soon made the department one of America's best." Petersen, op. cit., p. 140.]

VDT: All top names. About Human Society, John Weeks, one of your Berkeley students whom I happened to talk to recently, considers it your greatest book.

DAVIS: Really! I'll tell a bit of the background of that. There was one course in population at Princeton and Notestein taught that, so all my teaching was in other aspects of sociology. We had a pretty big introductory course in sociology. The students rated the courses--I guess they still do--and we got a mediocre rating. We decided we were going to get that course up in rating. Harry Bredemeier, an instructor, and I carefully prepared every lecture, sometimes dramatizing it and

pretending it was a radio broadcast play or whatnot. We had precepts at Princeton, breaking down the courses into discussion groups. The students became so enthusiastic that they wanted to have more precept hours than were scheduled. The course came out at the end with a very high rating.

We discovered that the best material was the material we wrote ourselves. Human Society was written to improve that course at Princeton. That was completely in addition to the research at the Office of Population Research.

VDT: You were doing the two simultaneously. Human Society inspired others and it has remained a textbook in the field.

Besides those we've mentioned, what other colleagues do you recall at Princeton?

DAVIS: Dudley Kirk was there and Clyde Kiser. Kiser lived in Princeton and worked simultaneously in Princeton and at the Milbank Memorial Fund in New York. He worked mainly on American demography, but I didn't follow his work too well, so I didn't know too much about it. I also remember Ansley Coale.

VDT: Also at the time you were at Princeton, in the mid-1940s, you helped frame the demographic transition theory, which, of course, several people were working on at that time. Were you one of the first to use that expression, the "demographic transition"?

DAVIS: I edited a volume of The Annals of the American Academy of Political and Social Science on world population [Vol. 237, January 1945]. I called it "World Population in Transition." And the title of my paper, which I put first in the volume because it covered the theory, was "The World Demographic Transition."

VDT: So probably you were one of the first to use that term--well, everybody knows that.

DAVIS: I was a young man who got acquainted quickly in the field. I went to all the meetings; published a lot.

VDT: You did. Well, you've been publishing a lot ever since.

DAVIS: Had a lot of fun. So they wanted to do an issue of the Annals on population and they thought of me, although I was not at the time a senior person.

VDT: No, you weren't. That was quite an accolade, to ask you to edit that volume.

DAVIS: I think Notestein was a little miffed. I had gotten well acquainted with the editor of the Annals, Thornton Sellin, a criminologist, so he thought of me to edit the issue on population. Later I edited another issue for him, this one on Latin America ["A Crowding Hemisphere: Population Change in the Americas," Annals, Vol. 316, March 1958].

VDT: In that first Annals article, you were espousing the demographic transition theory in its classic form--that socioeconomic change is necessary for fertility to decline, following mortality decline. However, I was interested in following your change in view in an article by Dennis Hodgson, "Demography as Social Science and Policy Science," in Population and Development Review, March 1983. He pointed out that by the mid-1950s, you were urging a direct family planning program for India, without waiting for socioeconomic change.

DAVIS: Yes.

VDT: You wrote: "The planned diffusion of fertility control in peasant populations prior to, and for the benefit of, the urban-industrial transition is a viable (and humane) policy for a country like India."

Then you shifted back, in 1967, in your provocative article in Science [November 10, 1967], "Population Policy: Will Current Programs Succeed?" You argued that family planning programs as they were then structured couldn't do the job alone because they stressed fertility desires and those desires were too high--four children and more--to bring fertility down to replacement level, and that there had to be socioeconomic change to change motivation before there could be a deliberate reduction in fertility. You went on to outline some policies to change motivation: control illegitimacy and encourage late marriage and low fertility within marriage.

As I understand it, you started off first with classic demographic transition: socioeconomic change has to be first. Then you were saying in the mid-1950s--probably wanting to push India into more direct family planning programs--that fertility could be changed without waiting for socioeconomic change. Then by the mid-1960s, you were back to saying there had to be socioeconomic change first. Do you still stand by that?

DAVIS: I think maybe there's some misconception.* [See Davis footnote of May 1990 on the Demographic Transition, below.] It was never my thesis that you could change reproductive patterns simply by offering devices. The very emphasis on the institutional side of things was to show that reproductive motivation is intertwined with the rest of the social system. You can't ignore these intertwinings.

Now, in policy you've got two things always. You've got what's necessary from a scientific point of view, which is cause and effect. And you've got what's not necessary but may be desired nevertheless. Economic development is too comprehensive to be thought of as an instrument for population control. All along, I have consistently argued that population policy should try to change those aspects of the institutional structure that support high fertility. The U.S. has never taken that suggestion seriously, but Singapore, Taiwan, and Korea have.

VDT: You mean things like encouraging later marriage?

DAVIS: Yes. To be sure, the age at marriage has been going up [in India], but it's a slow process. What has been happening is difficult to ascertain, because India doesn't have a vital statistics system and depends on censuses for data on marriage.

It's a false antithesis to say that population control depends on total development or that it can just be done by family planning. It doesn't make sense either way. Obviously, if one is going to bring fertility down, one must have the means to do so, but having the means will not do it alone. There's no opposition. I've never been against distributing contraceptives, and I've never talked about transforming fertility by simply furnishing them contraceptives. And I think the Indian case illustrates the truth of that.

VDT: That they've stressed just distributing the devices without trying to change the institutions?

DAVIS: Yes, they didn't do much. Fertility has dropped a little in India, but amazingly little since 1952. The fact is they have added hundreds of millions of people while carrying on what they considered to be a population policy. In those terms, the policy has to be judged an utter failure. To characterize it by the perspective of the world, the failure to control population is the greatest tragedy

that ever hit humanity.

VDT: You mean in general the countries that have failed to do so?

DAVIS: Yes.

VDT: Of which India is the largest. But think of China, the means they have used to bring down their fertility rate.

DAVIS: They have taken seriously the motivation side.

VDT: Motivation? Well, coercion, perhaps.

DAVIS: You can't have control unless you use incentives and disincentives. And keep records.

VDT: Keep records? In this country?

DAVIS: Yes. It isn't backward countries alone that have a hard time dealing with the population problem.

VDT: You really have said both approaches are necessary. An article of yours that I like very much was your review of the World Fertility Survey in Sociological Forum in 1987 ["The World's Most Expensive Survey," Sociological Forum, Fall 1987]. You criticized the WFS as a waste of money. But aside from that, you charged that family planners were still avoiding the issue of socioeconomic change, or as you said, changing the institutions that make for high fertility. You wrote that "the use of birth control is a necessary condition for fertility decline, but so is social modernization." So, you've really made that point well.

***Davis footnote on the Demographic Transition, May 1990.**

The essence of the Demographic Transition [theory] is that it is a dynamic model, a model of a process of change. This process has changed over the decades, as the name implies, but it has always accompanied the Industrial Revolution, and has never not accompanied that change.

I was not responsible for the term, but I believe I was instrumental in its becoming a standard part of the demographic vocabulary. A competing term was Vital Revolution, recommended by Norman Himes, but it never caught on, whereas "The Demographic Transition" did catch on.

At first the term was applied to the "classic" cases--the proven cases of European countries and European countries overseas--because these were the only countries that had completed the fertility change. Japan was the first non-European country to go through the transition, but Korea, Singapore, Hong Kong, and Taiwan have followed closely. "Progress" had been a term used long in advance of the Demographic Transition.

The demographic transition is not a scientific law, but an extremely useful empirical regularity. It is useful precisely because it relates several variables in a system and because it is not an equilibrium model. It can be expected that as latecomers enter the process, the exact pattern will be altered. I have emphasized the greater speed of the transition now compared with the past (see Kingsley Davis, "Population and Resources: Fact and Interpretation," forthcoming, August 1990, in a special supplement to Population and Development Review). This has meant a faster growth of population during the transition.

One problem is that people view the demographic transition in a mechanical way, as if it were a scientific law. If there were such a law, it could not tolerate an exception. The whole thing would have to be thrown out. An empirical generalization, however, can tolerate some exceptions and still be valuable in analysis.

One way of avoiding the mechanical approach is to look at it in motivational terms. This is what I tried to do in the paper on "Change and Response" ("The Theory of Change and Response in Modern Demographic History," PAA presidential address, Population Index, October 1963). What did it mean to the individual to be in a situation where families were larger than they had been in the past? Most of the people were farmers. What did it mean on the farm to have more children to support on limited land? How did people respond? My belief is that the new regime viewed the demographic change as catastrophic. All responses were utilized to some extent (this is where the "intermediate variables" come in). Celibacy, late marriage, abortion, infanticide, migration, birth control within marriage--all were used as escapes from large families. The value of children tended to be less than their cost. The birth rate, and especially the replacement rate, began to fall.

Thus the demographic transition was not something spontaneous, having nothing to do with motives. As I see it, and as I tried to stress when writing about it, it had to do with efforts to preserve or enhance social status in a newly emerging industrial society. It was the product of a very human process of change. It is not a predictive instrument. One cannot say that country A is two-thirds into the demographic transition and thus its population must be growing a certain percent each year.

VDT: Let's get back to your career. What took you to Columbia in 1948?

DAVIS: Mainly a chance to teach demography. Notestein was giving the only course at Princeton; there I was teaching something else beside population. By going to Columbia, I could teach the graduate course in population and would have more students--which I did; a lot more students. There was also more independence to do what I wanted to do. But the trouble was, I hated New York City.

VDT: New York City--even then?

DAVIS: Even then.

VDT: Some of your students at Columbia--I've heard or you've mentioned--were Andy Lunde, William Petersen, Art Campbell, Sam Baum, Gwendolyn Johnson. Can you remember others? You've said that when you left Columbia [in 1955] you gave Lazarsfeld a list of 14 or 15 students that you had at that time.

DAVIS: I can't remember all of them, but others included Judith Blake, Paul Jacobson, Monroe Lerner, Joe Stycos, Lincoln Day, Robert Parke, Jeanne Clare Ridley, and Eduardo Arriaga [Arriaga was Davis's student later at Berkeley, see below]. Lincoln Day and Alice Day met in my course.

VDT: A bit of romance! I know about that; I've interviewed Link for this series too. Some of the students you had may have been more mature, such as Andy Lunde and Art Campbell, who came back from the war.

DAVIS: Yes, some of them were a little older.

VDT: That department of sociology at Columbia, postwar, was very influential, very special. Why was that?

DAVIS: It wasn't a big department, fairly small. It was forward-thinking. They had Paul Lazarsfeld emphasizing methods, Robert Merton emphasizing theory, and Bob Lynd to carry the radical left. It was a pretty good setup.

VDT: It must have been a stimulating department; many people have talked about it.

DAVIS: I learned to do applied social research at the Bureau of Applied Social Research [at Columbia]. I came to the office one morning, talked to one of my aides about how things stood, and found we had 80 some people doing research.

VDT: All sociologists?

DAVIS: No, they were from economics and other fields too--doing contract research of one kind or another. I said to myself, "Why am I doing this?" And straightway decided to get Charles Glock to take over.

VDT: You didn't care for the administrative work?

DAVIS: I don't like contract research, especially survey research.

VDT: Have you avoided that in your career?

DAVIS: Oh yes, pretty much.

VDT: Why don't you like contract research?

DAVIS: I like the kind of problems you set yourself rather than somebody else's. That's the definition of applied research: you take somebody else's goal and try to figure out how to reach it. I did not want to do that.

VDT: Have you always been successful in getting funding to do the research you defined?

DAVIS: I've not had much trouble getting adequate support over the years, in one way or another.

VDT: You left Columbia. What took you to Berkeley?

DAVIS: More money, California, climate.

VDT: How did you come to establish International Population and Urban Research?

DAVIS: I wanted to do comparative research on urbanization and applied for the finances to do that from the Ford Foundation and they gave me most of what I asked for. We had to have an office, so we got one in the Institute of International Studies at the university. I made other proposals and got more money for other aspects of research and started publishing. A lot of the people who worked in the place were graduate students and also were doing their dissertations with me. After they got their degrees, they often published a book. There were 18 volumes in that series.

VDT: The famous series of orange Population Monographs, that went all over the world.

DAVIS: That's right.

VDT: I recently happened to pick up one by Jogindar Kumar on Population and Land in World Agriculture [1973]; obviously it had been his dissertation. He was very effusive in his thanks to you for the extra encouragement you gave him. You must have done that with all your students.

DAVIS: I thought that was a good problem. I said, "It certainly pays you to work on it; get it done." I did that with Andy Collver. He didn't get his dissertation done as fast as I thought he could, so I told him one day that I'd give him only so much more time; he'd have to get the thing done. And so he did. He published a very good book in the series. It was on birth rates in Latin American countries, using mainly censuses.

VDT: Which came first, the dissertation topics and then the promise to put them in the series, or did you suggest the topics to them?

DAVIS: Nobody had a promise to get into the series.

VDT: But you did see the series as in part a vehicle for these dissertations?

DAVIS: Not everybody at Berkeley wrote their dissertation with me. Kumar was a very good student; very well trained in statistics.

VDT: Eduardo Arriago was in the series. You have mentioned that you'd met him at a Milbank Memorial Fund meeting.

DAVIS: Yes, I forget what the meeting was on; it wasn't a very big one. I'd been looking for somebody to cover the Latin American field, because I had some money for that. I asked somebody who had been working on Latin America and he told me, "There's this fellow named Arriago from Argentina; smartest guy." He'd worked in Argentina some; got his first degree in Argentina. So he was at this meeting. I talked with him in Spanish; he didn't know English. You could see right off the bat that he was a very smart guy. I think he was working for the Pan American Union. When I described the job at Berkeley, he decided to come.

VDT: Had he done his Ph.D. by that time?

DAVIS: No, he did it under me. It was in the series--two volumes on mortality.

VDT: Right, those books are well known.

Just after you arrived at Berkeley, you and Judith Blake published your landmark article, "Social Structure and Fertility: An Analytic Framework" [Economic Development and Cultural Change, April 1956], on intermediate variables, which has set the direction of fertility research ever since. John Bongaarts honed it into his proximate determinants. It really was the framework for the World Fertility Survey--much as you criticize the World Fertility Survey--and for the National Survey of Family Growth; they collect their data to fit into that framework. Are you pleased about that?

DAVIS: Oh yes, sure.

VDT: It's one of the great pieces in demography. How did you come to write that?

DAVIS: I guess as a method to get things organized. You see, much of social science is published when you don't see clearly what the problem is. We published it; Joe Stycos, I think, was about to publish something similar. He was one of my students both at Princeton and Columbia.

VDT: Some people have criticized the WFS--you did it too in part--for just collecting the intermediate variable data, forgetting the sociocultural background, which you had meant to emphasize also in that article.

DAVIS: Yes. Well, presumably it helped in sorting things out.

VDT: It did indeed.

Now let's talk about Berkeley's department of demography which you helped to establish--the first and still almost the only department of demography in the U.S. It lasted from 1967 to 1972. Is it important to have a separate department of demography?

DAVIS: It turned out to be a very good basis for graduate education, because if you take the various elements in demography and push the analysis, you get into economics, sociology, and other disciplines. Take something like epidemiology. A person getting a Ph.D. in sociology would not think of taking epidemiology, but in the department of demography it's required. The epidemiology graduate studies causes of death; demography studies mortality, including causes of death. So as I department, I think it was very successful.

If you want, I can try to find in the files a copy of the proposal for the department which we presented to the administration of the university. A very good idea.

VDT: And California did buy the idea?

DAVIS: Oh yes.

VDT: Way ahead of its time?

DAVIS: Yes. And we were the victim of the student revolt.

VDT: In the late 1960s? You feel that was the problem that happened?

DAVIS: There was so much anger; things were just terrible. Never knew what was going to happen next. Students could go on a rampage and break \$250,000 worth of windows in 30 minutes.

VDT: You feel that brought down the department?

DAVIS: Well, funds were very scarce. Bowker came in as chancellor and this new department of demography was vulnerable. He could do away with that and use the money somewhere else. And, of course, like many innovations, some of the people you'd expect to be most for it were against it.

VDT: Like whom?

DAVIS: Like a lot of people in the PAA.

VDT: Why?

DAVIS: It wasn't the way they'd done things. They all had PhDs in some other field.

VDT: And felt that was the way things had to be done?

DAVIS: I think there was some fear among people who would be trained in the field [of demography] that their degrees would be devalued.

In a department, you can get a sequence of courses. If you need more faculty, you can appoint them. We had a Group in Demography [prior to forming the department] and we had a terrible task of persuading departments to make appointments that were relevant to demography but also relevant to the department. We tried to get Paul Demeny, who was at Michigan, but nobody in the economics department had ever heard of Paul Demeny. Everything he'd published had been in demography; he hadn't done anything the economists would recognize as economics. So we didn't get him. Things are difficult when you don't have a separate department of demography.

VDT: To get established and accepted by others in other disciplines?

DAVIS: Yes. And you get good training when you have a department, because you can organize the courses so as to make one thing dependent upon the next. When you get a Ph.D. in some other field, maybe you get two courses in demography but not much more. So I think there's quite a rationale for a separate department. It's coming; it's on its way. Princeton now has a Ph.D. in demography.

VDT: I guess you were a little ahead of your time. It's taken people a long time to come round to your way of thinking. A department of demography seems logical.

DAVIS: Better late than never.

VDT: Can you tell me a bit about your time with the UN Population Commission from 1954 to 1961. You were the second U.S. Representative, after Phil Hauser and before Ansley Coale. Later they had people like General Draper and Phil Claxton who were not demographers. Does that time stand out in your memory?

DAVIS: It was a busy time.

VDT: How often did you meet? Did you have to go to New York several times a year?

DAVIS: Once a year.

VDT: Did you find that the Commission was rather politicized? It was set up to advise on the program of research of the Population Division. Family planning was pretty much a taboo topic then. Did you find that?

DAVIS: No, not really. We didn't call it family planning; we called it birth control-- Margaret Sanger's old term. And there was remarkably little politicization. It was the usual mumbo-jumbo

about population being a function of economic development. But on the whole, the Commission stuck relatively well to scientific study until the family planning movement took over.

VDT: And that was after your time. The U.S. got into funding of family planning only in the late 1960s, after your time.

Could you tell something about your work at the University of Southern California and at the Hoover Institution. Do you still divide your time between the two?

DAVIS: Yes.

VDT: What happens? You teach down at the University of Southern California [in Los Angeles] for a semester?

DAVIS: I have an appointment as Distinguished Professor there. It's only part-time. It was full-time at first, but I couldn't take the climate. My bronchitis was much worse there.

VDT: From the smog?

DAVIS: Yes. A whole generation of children are affected; getting worse all the time.

I had an invitation to return here [Stanford] for a year [1980-81] at the Center for Advanced Study in the Behavioral Sciences, so I came. I had been there for a year before [1956-57], just after I came to Berkeley. So I cut down the time I spent at USC to, at most, a graduate seminar in the fall semester.

VDT: Then here at Hoover is your headquarters as a researcher? In recent years, you have organized conferences and edited the proceedings. There was the one on marriage, in 1982; Contemporary Marriage was the report that came out of that, in 1985. There was low fertility, in 1985, and the wonderful book that came out of that, Below-Replacement Fertility in Industrial Societies, in 1987. And you're going to do the same with the latest conference you've organized, in January this year [1989], on population, resources, and the environment?

DAVIS: I already have a reader, a book we got out to serve as a background.

VDT: Is that the Teitelbaum-edited one? No, it's still another set of papers [Kingsley Davis, Mikhail S. Bernstam and Helen M. Sellers, eds., Population and Resources in a Changing World: Current Readings, 1989].

DAVIS: I have a paper on migration in the Teitelbaum volume ["Social Science Approaches to International Migration,": in Michael S. Teitelbaum and Jay M. Winter, eds., Population and Resources in Western Intellectual Traditions, 1989].

VDT: These publications--how do you manage to accomplish all these things?

It seems a good field for you to be in, collecting together these experts on different topics.

DAVIS: On marriage, I had some ideas on it and thought I'd get some others interested. Somehow the proposal I wrote wound up in the hands of Gardner Lindzey and he said, "Come up to the Center [for Advanced Study in the Behavioral Sciences] to work on that." So I did that. Shortly after I got here [Stanford], Hoover asked if I'd be interested in being a research fellow after my year at the Center. I

said yes, so I've been here ever since.

The subject of below-replacement fertility seemed to cry out for some kind of investigation, so Mickhail Bernstam and I brought together a conference.

I've always been interested in population and resources but never published much on it. That's a good field too. There's a lot happening, but the writings on it are scarce. A lot of natural scientists have peculiar notions about human demography. So I organized a conference.

VDT: This is all an example of how you've always moved with the times. We've talked of how--well, you've explained that your views on the demographic transition never really did shift. You have shifted from concerns about rapid population growth in less developed countries to concerns about low fertility in developed countries and now to population, resources, and the environment. So you have really moved with the times, through the years. [Petersen in "Kingsley Davis," op. cit., p. 140, writes: "Nearly two decades before ecology became a fad, Davis was devoting a tenth of the assigned reading in his basic course to a highly sophisticated treatment of ecological relations as these affect the quality of life."]

And you have been flexible in your views--for instance, on the family. In your 1967 article, "Population Policy: Will Current Programs Succeed?", and in the article you wrote for the Commission on Population Growth and the American Future, in 1973 ["The American Family in Relation to Demographic Change," Research Reports, Vol. I, Demographic and Social Aspects of Population Growth], you suggested downgrading the family as one means to reducing fertility, because the familial institution can be a block to reducing fertility. Then in a chapter on "A Theory of Teenage Pregnancy in the U.S.," published in the 1980 monograph by Chilman, Adolescent Pregnancy and Childbearing, you urged a rehabilitation of the family to reduce teenage fertility in the U.S. So I think you shifted your views there: first the family should be downgraded in importance and then it should be upgraded, certainly in the U.S.

Were you the first to point out how much higher teenage fertility is in the U.S. than in other industrial countries? Of course, Charlie Westoff went on to point that out and then the Alan Guttmacher Institute researchers, under Elise Jones, did that excellent project and book on that. You suddenly noticed that U.S. teenage fertility was so much higher than that of other developed countries?

DAVIS: I can't remember.

VDT: Well, you were early on with that.

DAVIS: It's true, I think. It depends a bit on how you measure fertility.

VDT: The U.S. has very high teenage abortion rates, added to fertility. It was actually pregnancy rates that the Alan Guttmacher people were looking at.

Who have been the major influences in your career? You've mentioned already Notestein, and in your 1979 interview [with Anders Lunde and Abbott Ferriss for the PAA oral history project; the interview covered little of Davis's career and the tape is defective], you mentioned Lotka, who gave the presidential address at the 1939 PAA meeting, the first you attended.

DAVIS: Made a powerful impression on me.

VDT: You already were into demography; you went to that PAA meeting [in Washington, DC] because of that.

DAVIS: I was getting into it.

VDT: Can you think back on some other of your early influences?

DAVIS: Well, on the sociological side, Talcott Parsons. And Lloyd Warner, in anthropology. I was never much of a disciple. There are a number of schools of thought. In sociology, there's quite an interest now in the origins of functionalism and about the history of functionalism, interpretation. This doesn't please me much, because it means you get tagged and then everything is fitted into that, regardless of whether it is relevant or not. But I learned a good bit from Parsons on structural functionalism. And I learned a lot from the readings that these people gave me to do. They started me off and that continued to be a long interest.

Some theories I was quite interested in but never published much about them. I've got filing cabinets full of things I never published.

VDT: Can you give me an example of some field that's interested you but in which you never published?

DAVIS: When I went to Harvard there was a course organized by William Morton Wheeler, a great expert on ant society and insect society generally. Fascinating. I continued to read in this area but never published anything on it, except in a recent symposium. I wish I had had a chance to work more in the field of human evolution. Washburn at Berkeley has been an inspiration; he was with me at the Center for Advanced Study in the Behavioral Sciences.

VDT: Perhaps that will be your next topic. Of course, you are cited by practically everyone I have interviewed in this series--and this is the 26th interview--as a leading influence on them, whether or not they were your students or colleagues.

DAVIS: I appreciate that.

VDT: Nathan Keyfitz, for instance; he taped an interview on his own in Jakarta. He said: "Davis is one of my heroes." And some people who merely read your publications were greatly influenced by them. And, of course, many outstanding U.S. demographers have been your students.

It's probably not fair to ask but who do you think of as some of your leading students? You've already mentioned some at Columbia and Arriaga and Jogindar Kumar at Berkeley.

DAVIS: It's hard to say one is more outstanding than another; they do different things. Some of them have a lot of publications; others are administrators. If you look at the occupational structure of the people who've studied with me, it's not characteristic of the whole field. Some of them have gone into administrative work, like Andy Lunde, some into research. It's somewhat unpredictable what they're going to do. But some other names, not yet mentioned, include Harriet Presser, Ruth Dixon-Mueller, Jerry Rale, Woody Carlson, Don Hernandez, Nelly van den Oever.

VDT: You must have used your graduate students and assistants very well to have produced as much as you have.

DAVIS: Well, I employed my graduate students.

VDT: And you've always given them credit.

DAVIS: Yes, my intention was always to give them credit for what they've done. I don't like publications in which the first name on the list is the guy who's never seen the manuscript at all. That gets into some very difficult situations. We have many cases like that--a lot of fraud and bitterness over allocation of credits. That's worked out pretty well. Someone like Jogindar Kumar, for example. He put an awful lot of work into that dissertation; I put a little bit.

VDT: You didn't put your name on that book.

DAVIS: No.

VDT: Let's talk about your publications. You say you don't know how many you have, but here's a list. It runs on page after page.

DAVIS: There's some repetition there.

VDT: What do you consider among your most important publications and why? Well, The Population of India and Pakistan, to begin with.

DAVIS: And Human Society.

VDT: What stands out in your mind?

DAVIS: I feel that in some cases I was sufficiently ahead of events. Those publications deserve credit; they changed views. I think I might have been more courageous than some demographers.

VDT: Indeed, you have been.

DAVIS: I didn't go along with family planning ideology.

VDT: So you would regard the 1967 Science article, "Population Policy: Will Current Programs Succeed?", as a sort of landmark?

DAVIS: It was only an example of being first with something or other--in this case, with a critique. There have been other critiques that confirm mine.

VDT: It was very controversial at the time.

DAVIS: Yes, it was, but I went ahead and did it.

And my PAA presidential address, "The Theory of Change and Response in Modern Demographic History" [1963]. It is one of the things I'm most proud of, as a theory.

VDT: Multiphasic response theory. I mentioned recently to Harriet Presser that I'd be interviewing you and she said, "Oh, I always have his classic article on multiphasic response on the course agenda for my students, except it should have been `multi-faceted.'"

DAVIS: Right.

VDT: That was an important article, indeed. My own copy from my student days is so marked up.

That and, of course, the intermediate variables article you would put up there?

DAVIS: Yes. And the demographic transition article [in "World Population in Transition," Annals, 1945]. Actually, the "Change and Response" article can be viewed as a further discussion of the demographic transition, although in "Change and Response" I didn't use the term.

VDT: No, you didn't.

DAVIS: What I did in the original treatment of the demographic transition was to try to understand why the different elements behaved as they did. I think the demographic transition has often been treated as a kind of statistical pattern, but I tried to think why there is a pattern. In the "Change and Response" paper I stressed the range of possible means. I tried to avoid the extreme concentration on family planning and birth control. My sociological training had taught me that so far as goals are concerned, collective goals are different from individual goals. Family planners confuse individual motivation with goals for the society at large, but there's no society with a brain. The one brain, the only goals, are individual goals. It's a very interesting question, how human beings reach collective goals. There's no collectivity that thinks; just individuals who think. This is the basic problem of human organization: how the two get together.

VDT: How you get from individual goals to goals for the collectivity?

DAVIS: Yes. What Margaret Sanger was talking about in birth control was not population control. She had no conception of a national population policy. She was thinking in terms of individuals: Give them the means, contraception, and they'd have the number of children they want. But what people want for themselves is not necessarily what the society should have.

VDT: That's what you were bringing out in the 1967 article.

DAVIS: Trying to.

VDT: You write so well; what explains that? You had English as your undergraduate major and philosophy as your next choice [for the first master's degree].

DAVIS: Perhaps it's partly biological. In the third grade, we had to do essays, had to do some original writing. The teacher wouldn't believe that I'd written my story. I don't think it's purely a matter of training, although almost anyone can learn to write if he tries hard enough.

VDT: You were born with the gift of expressing yourself?

DAVIS: I have several relatives who are good writers. And I did a major in English, which was reinforcement. Because they thought I wrote pretty well, people encouraged me to write more. And so I got a good deal of practice and that encouraged me to think that English was what I should study in college.

When I was in junior high, I had a teacher, Peter Madrey, who gave us 31 rules of composition. We had to memorize these rules and use them in writing stories, and had to give the numbers for every punctuation mark. We had to say why that punctuation was used. It was wonderful training. Peter Madrey didn't stay long in junior high teaching; he became a salesman for a big publishing firm in Dallas.

VDT: He must have been a wonderful teacher, since you still remember his name.

You've always written for the popular press, for instance, your New York Times op-ed piece last October [October 18, 1988], "Our Idle Retirees Drag Down the Economy." The demographic establishment has sometimes criticized you for that.

DAVIS: Really? I hadn't realized that this was a basis for criticism.

VDT: In some ways--perhaps not now, but in the early days. Do you think it important that demographers, sociologists, academics talk to policymakers? Obviously you do, because you've written such articles as the 1967 population policy piece.

DAVIS: I think demographers should write as clearly as possible to get things out and in circulation. Why do research in demography to hide it in technical journals only? I think demographers have an obligation to get their findings as widely known as possible. If they get findings, the public should know about it.

VDT: Is that, by the way, one of the reasons that you supported the Population Reference Bureau at a time when it wasn't so respectable? [PRB vice president, 1952-55; trustee, 1952-70.]

DAVIS: Yes, sure--in terms of maximum publicity for new findings.

VDT: You've certainly contributed to that, having such interesting things to say, such things as your contribution to the September 1974 issue of Scientific American ["The Migrations of Human Populations"]. That was a great issue [The Human Population].

And you've always been something of an action demographer. In Hodgson's 1983 Population and Development Review article, he described how you shifted from being a social scientist to an action demographer, like Notestein, in the 1950s. But I think you've always had something of an action demographer in you.

DAVIS: That's a peculiar way of describing it.

VDT: In your 1979 interview, you said you felt--just as you say now--that academics and demographers have an obligation to get their research out. But how about the activists?

DAVIS: If they have funds for research, they have an obligation to defend their results, get them out. Give people back their money's worth.

VDT: That's a good way to put it. But what about trying to influence policy, as you did in your 1967 article? In your 1979 interview, you said you felt that the academic demographer-sociologist should not try to influence policy goals, but if there are goals, their role is to point out how to achieve them, demographically.*

***Davis footnote on "Influencing Policy," May 1990.**

There seems to be some confusion on the subject of demography and its influence on policy. I have

said that scientists, no matter what their field, have an obligation to bring their scientific findings to bear on important issues, if the findings are relevant. What I dislike is a scientist making pronouncements in other fields than his own, simply because he is a scientist, as when Nobel laureates in theoretical physics make unsupported statements about the drug problem. In other words, a demographer influences policy by providing authoritative evidence of a demographic character, not by simply proclaiming his unsupported opinions or preferences. The whole subject is complex and difficult to deal with in a short passage.

VDT: Following along that line and to repeat a bit of what you said in your 1979 interview about your philosophy of research in demography, your sociological approach. Incidentally, Ansley Coale in his interview said of you that "Davis is an original and insightful social theorist." You've said just now, again, that it's important to understand that demographic behavior takes place in a framework of human society and to analyze why demographic behavior happens, not just describe it.

What do you see as leading issues in demography over the years you've been involved? We've talked about your concerns with rapid population growth in developing countries, to which you first called attention with the India book. Now you've shifted your interest to low fertility, aging populations, in developed countries. But what do you see as leading issues in demography over all the years of your career?

DAVIS: I don't guess I've got any useful thoughts on that. It seems to me the main interests are abiding interests.

One interesting thing now is that to judge by attendance at PAA meetings at any rate, the profession is expanding more in the applied field than in teaching and research. That will give rise to almost unlimited prospects. We are beginning to develop a higher morale for those who are in applied demography.

VDT: Those who are in state and local government and in business? Not publishing, perhaps?

DAVIS: They have a difficult time trying to get enough time to publish. Also the job doesn't pay them to do research specifically for publication.

VDT: Do you think that they will, for instance, continue to come to PAA meetings?

DAVIS: Yes, especially if provision is made for discussion of their interests. I think that probably we ought to give them more attention.

VDT: Interesting point.

Jay Siegel gave a paper at a PAA meeting back in the 1950s ["The Teaching of Demography," 1951 PAA meeting] where he pointed out that there were far more people teaching some population in universities than there were members of PAA. Sociologists had to teach a course on population every five years or so, for instance, but were not really interested in it. But that's part of what I wanted to ask you in our talk on PAA.

One final question on your career: What accomplishments in your career have given you the most satisfaction?

DAVIS: [Laughter]

VDT: You've had so many.

DAVIS: I suppose one accomplishment has been to point out that family planning as the sole approach to population control is a monumental mistake.

Also, I was pleased to be one of the first to draw attention to the rapidly declining mortality in underdeveloped countries. I wrote an article ["The Amazing Decline of Mortality in Underdeveloped Areas," American Economic Review, May 1956], which was one of the first to call attention to the fact of unprecedented speed in mortality decline.

VDT: You were ahead of George Stolnitz?

DAVIS: George and I were running neck and neck. George followed me at the Office of Population Research, took over a lot of the files I had on mortality. We were both interested in comparative mortality trends and gathered data on them.

VDT: He had already done some thinking about it?

DAVIS: Yes. We came out about the same time with articles on the speed of mortality decline.

VDT: Well, that's an example of how many things you've been into. Frankly, I don't associate you with that. That's one of George Stolnitz's main claims to fame, but just one of your several claims to fame.

Presumably you are pleased with the impact you had on India's family planning program?

DAVIS: Yes--with reservations.

VDT: What about International Population and Urban Research, the institute at Berkeley, does it still exist?

DAVIS: Berkeley now has a Graduate Group in Demography.

VDT: That was a successor of both the department and IPUR? I interviewed Ron Lee there last Friday.

DAVIS: Ron Lee was a very good student of the separate department, but he went to Harvard for a Ph.D. in economics. The question again?

VDT: The accomplishments that have given you the most satisfaction.

DAVIS: I was pleased with the [1967] paper on family planning. I hoped it would stir up people in family planning and it did that. It started people thinking in relatively new channels about population policy. I guess my satisfaction is measured by the interest shown.

VDT: Which was a lot.

DAVIS: Whether the interest was favorable or not is another question. I would rather be discussed unfavorably than not discussed at all.

And the intermediate variables article, I'm satisfied with that. I still use it in my graduate

seminar.

VDT: The copy in the Population Reference Bureau library is all written over--some people criticizing you in the middle of that. And I've got a dog-eared copy of Below-Replacement Fertility in Industrial Societies.

DAVIS: I'm glad you found that useful.

VDT: Excellent. And right on target now, when we're wondering whether we have to be concerned about low fertility and an answer to Ben Wattenberg, who is concerned.

Let's jump to PAA. You said in your 1979 interview that the first meeting you attended was in Washington, in 1939, when Lotka made his presidential address. Dudley Kirk--it happened to be his first meeting too--said he recalled that it was in the Hay Adams Hotel. Lotka's presidential address really made an impact on you and encouraged you in your demographic career.

DAVIS: I am not sure about the date and place [of my first meeting]. For instance, I remember the place as Philadelphia, but it could have been Washington. [There was a "fall meeting" of PAA, under the auspices of the American Philosophical Association, in Philadelphia, November 18-19, 1938. On Friday evening, November 18, Alfred Lotka, who was PAA president at the time, having been elected at the sixth annual meeting of May 1938 in Princeton to hold office through the seventh annual meeting of May 12-13, 1939, in Washington, spoke on "Contacts of Population Study with Related Branches of Science," according to the announcement of the Philadelphia meeting in Population Index.]

VDT: What other highlights and people do you remember from the early meetings? I want to mention that a number of people have mentioned the wonderful debates there were between you and Frank Lorimer in the 1950s and early 1960s, in every fertility session. Apparently, you two sort of lit into each other; it almost came to a physical fight at one point, I'm told. [See Philip Hauser interview, above; Lincoln Day, below.] You took the socioeconomic approach to understanding fertility decline and his approach was cultural. I've never understood why those two should be different.

DAVIS: Essentially there aren't any differences.

VDT: Yes, but somehow you seemed to approach it differently enough to cause interesting fireworks in those meetings.

DAVIS: I guess it would be a long story to go into. I never thought of Lorimer as a rival, but he evidently thought of me as being one for him.

VDT: You never thought of him as important?

DAVIS: In my career, no. His material was too unsystematic to be worth serious consideration. I didn't think much about it.

VDT: I see. Well, you had some lively debates in those meetings. Charlie Nam, in particular, said everybody looked forward to the fertility session every year, because invariably you two would have lively discussions.

DAVIS: I guess that's an exaggeration.

VDT: What else do you remember from some of the early meetings?

DAVIS: Pat Whelpton. He was well known at the time; very effective demographer. He was an associate of Warren Thompson at the Scripps Foundation [for the Study of Population Problems]. He was always gifted in interpretation. I always enjoyed talking to him. In demography, he was a very talented technician and yet a master interpreter. He worked well with Thompson, and like Thompson, he was easy to talk to. One of the good things about the early meetings was that they were small enough to allow you to talk to most everybody you wanted to.

VDT: That's what many people say they found outstanding about the early meetings. Do you miss that?--although you have come faithfully to all the meetings.

DAVIS: Oh, yes. I liked the meetings better when they were smaller.

VDT: But you're one of the few members who's been coming that long who still presents papers and roundtable sessions.

DAVIS: Well, might as well.

VDT: Okay! Obviously, you still enjoy the meetings, because you come and you take part--surrounded by admirers.

Do you remember any outstanding meetings over the years? Here's a list of the meetings that Andy Lunde prepared. It includes the attendance numbers at the earliest meetings [through 1935] and from 1967 on. Your meeting, where you gave your "Change and Response" address, was in Philadelphia, in 1963. That was one of the few where they had an interesting setting for the dinner; it was in the museum of anthropology of the University of Pennsylvania.

DAVIS: I didn't see much of it. I was sitting in the hotel room finishing up the paper.

VDT: Were you! And did you change it at all before it was published in Population Index [October 1963]?

DAVIS: I went back over it, but I didn't do much changing. When I was in my hotel room, I'd known I wanted to use the Irish as an example, but I hadn't quite finished the analysis of the Irish situation. The presidential cocktail hour was going on and I got there just as everybody was being seated after the cocktail, ready to receive the presidential address. I got there just in time.

VDT: You mean just off the top of your head you were doing the analysis of the Irish situation! You had some data in front of you?

DAVIS: I'd already looked at some data.

VDT: Well, you are a quick study.

Let's talk about applied demography, where you say you feel the future is--that's where the jobs are.

DAVIS: Well, that's where a lot of the expansion is occurring.

VDT: Do you think there's still room in demography for basic researchers, overall theorists, multi-interested demographers like yourself?

DAVIS: Of course. I would not want to exclude anyone.

VDT: PAA is still pretty small--2,600 [2,679 at the end of 1989]. It's been fluctuating about that number since the mid-1970s. That's small in comparison to the American Sociological Association or the economists.

DAVIS: Yes.

VDT: But you feel that anyone who wants to come in should come in?

DAVIS: I don't hold any fear of imposters. As long as people are willing to pay their dues, I guess they show special interest in the field.

VDT: Who among the younger demographers do you think could assume your role, your mantle, as the grand over-arching theorist, who sees the whole picture?

DAVIS: [Much laughter] Well, I don't know.

VDT: Okay. Are you discouraged by the outlook for world population trends?

DAVIS: Yes.

VDT: Indeed. Here is the Population Reference Bureau 1989 World Population Data Sheet. The world population growth rate this year--though you're not supposed to use the Data Sheets for time series--has gone up from 1.7 to 1.8 percent. World population is at 5.2 billion and the projection for the year 2000 has gone up. Are you still pessimistic?

DAVIS: I'm not optimistic.

VDT: And what about aging populations in developed countries?

DAVIS: Well, what about them?

VDT: You're not too concerned about them?

DAVIS: Nothing you can do about them except modify their circumstances.

VDT: And what are your plans; still no retirement? You've deplored non-working retirees, in that New York Times op-ed article and in the article you wrote with Nelly van den Oever in the March 1981 Population and Development Review, "Age Relations and Public Policy in Industrial Societies."

DAVIS: Well, it gives demographers a lot of work to do.

DAVIS: Are you going to take time to write your autobiography?

DAVIS: I don't think so.

VDT: I hope someone will write of you. At least you should supervise the collection of your publications. Is somebody doing that?

DAVIS: That's in the cards.

VDT: Good. And I think you should write an article like Frank Notestein's last article, published in Population and Development Review [December 1982], "Demography in the United States: A Partial Account of the Development of the Field." From your perspective, you've got an important story to tell, that only you can tell.

DAVIS: I'd be interested in doing it if I get the time.

VDT: Well, you've had a wonderful life, on all fronts, including your new small son, Austin. Did you name him for Austin, Texas?

DAVIS: Yes.

VDT: And your wife Marta and your various contributions, as I say, on all fronts. To end up, I just have to tell you a story that Lincoln Day told me.

DAVIS: He's a great storyteller.

VDT: He is. He said you and he were both stuck in the Dallas airport after you'd attended a seminar at the University of Texas. You had been impressed by the caliber of the students you had met there. And you said that you probably wouldn't have made it into the University of Texas today, because you had not been in the top 25 percent of your high school graduating class. Now, is that true? In any case, you did get into Texas and look what a wonderful career you've had! Is that an apocryphal story or not?

DAVIS: [Laughter]

VDT: And I notice that you graduated cum laude from Texas, in English.

DAVIS: Yes.

VDT: But is it true that you weren't in the top 25 percent of your graduating class at high school?

DAVIS: That's apocryphal; I don't remember that. [Laughter]

VDT: Well, anyway, Lincoln used that as a wonderful example of universities being exclusive, making it so tough for people to get into universities; you might knock out the geniuses and late-bloomers. Though you were no late-bloomer; you were a very early bloomer.

DAVIS: It never occurred to me that they might not accept me at Texas. One of my satisfactions, several years back, was to get a letter from the editor of the campus literary magazine. I was editor when I was there. He referred to a particular editorial I wrote, wanted to reprint it. Gave me a lot of

satisfaction.

VDT: You were saying things back then still worth saying?

DAVIS: I had an editorial each month; the magazine came out monthly. But I was operating under a faculty committee that supervised student publications. At that time, of course, matters of freedom were differently conceived from what they are now. The printer was instructed always to hold up publication of questionable material until the committee could see it. So the printer called me over one day and showed me my editorial, all set in type. It was on campus morality. I'd said campus morals were pretty bad but not bad enough.

The committee called me over and said, "Kingsley, you can print this if you want to, but you can't remain a student if you do." So I said, "Okay, I'll continue my education." So I ran a big box with a black margin around it and the words: "Editorial this month has been censored." They said they weren't censoring my material, but if I published it, I couldn't remain in school. So I said, "Editorial this month censored." I circulated it, mimeographed, throughout the campus.

And I always remember, "Leads to industrial democracy"--that was a socialist line. I ran a radio program under socialist auspices while I was a student.

VDT: You ran a radio program too! You were into many things. And this editor, who wrote you a few years ago, did he find the original editorial, and it finally ran in the student magazine?

DAVIS: Yes.

Two Davis after-remarks:

Re running for PAA office: "I was always pitted against Irene Taeuber and she always won."

Re times away on consultancies, etc.: "I've never been more than six weeks away from a university campus. You don't need that long to understand a country" [re his time in India].

DONALD J. BOGUE

PAA President in 1963-64 (No. 27). Interview with Jean van der Tak during the PAA annual meeting, Omni Inner Harbor Hotel, Baltimore, Maryland, March 30, 1989.

CAREER HIGHLIGHTS: Donald Bogue grew up in Missouri and Iowa. He received all his degrees in sociology: the A.B. from the University of Iowa in 1939, the M.A. from Washington State College in 1940, and the Ph.D. from the University of Michigan in 1949. He was with the Scripps Foundation for Research in Population Problems at Miami University, Oxford, Ohio, from 1946 to 1953. In 1953 he moved to the University of Chicago, where he was Professor of Sociology, Associate Director of the Population Research and Training Center, and from 1961 to 1988, Director of the Community and Family Study Center. Since his retirement from the University of Chicago, he has continued work with his own Social Development Center. He has been consultant to the U.S. Census Bureau and other U.S. government agencies, the United Nations, including the UN demographic training centers in Bombay and Santiago, the Ford Foundation, the Population Council, and the Pan American Health Organization. He has published a prodigious number of monographs and articles in the field of population, including such bibles of the field as Principles of Demography (1969) and the two monographs entitled The Population of the United States, published in 1959 and 1985. He founded PAA's journal Demography and was its first editor from 1964 to 1968.

VDT: When and how did you first become interested in demography? Your first interest, I understand, was in human ecology. At Michigan, after you returned from your wartime service in the navy, you were the first Ph.D. student of Amos Hawley, who is well known in human ecology.

BOGUE: My first course in population was with a demographer whom everyone has now forgotten, Professor E.B. Reuter. He was the chairman of the department of sociology at the University of Iowa and he was particularly interested in race and social biology.

Then I went to Washington State and worked there in general sociology and came to the University of Michigan, where I took courses with Amos Hawley. While I was finishing my Ph.D. thesis, Warren Thompson invited me to join the staff of the Scripps Foundation. So I really became interested in population at Michigan. Then my first job offer was in this area.

VDT: Your 1949 publication, The Structure of the Metropolitan Community, was that your Ph.D. dissertation?

BOGUE: Yes. It was published by the University of Michigan Press. The work was actually done right after the war.

VDT: Then you went with Warren Thompson to the Scripps Foundation. I've seen some reference to you and the Social Science Research Council.

BOGUE: During the war, I was in the Navy for four years. I had finished my course work and prelims for the Ph.D. at Michigan before I left, but not the dissertation. So, immediately upon demobilization, I was recipient of a Social Science Research Council fellowship, kind of a rehabilitation grant, to return to civilian life and finish my dissertation.

VDT: Ansley Coale got one too and some others who became well known in demography. The idea

was to encourage you to go back into social sciences and finish your degree?

BOGUE: I guess so.

VDT: What did you do at Scripps?

BOGUE: Professor Thompson had obtained a grant from the Rockefeller Foundation to study population distribution with special reference to the United States, so he put me in charge of that project. I had been specializing in human ecology and the shift from human ecology to population distribution was a fairly small one. I studied migration and regionalization, urbanization, and sub-national demography. I spent most of those early years worrying about the United States population in terms of regions, metropolitan areas, and its sub-parts.

VDT: And later you did the chapters on "Population Distribution" and "Internal Migration" in The Study of Population [edited by Philip Hauser and Otis Dudley Duncan, 1959]. Tell me about Warren Thompson.

BOGUE: Warren Thompson was one of the grand old fathers of demography, of course. He was one of the founding members of the Population Association of America and one of its presidents [1936-38]. He was deeply concerned about Third World population and especially Asian population. He had spent time in China before the war and along with Irene Taeuber, was one of the first experts on Japan and the Far East population. His interest in U.S. demography and in population distribution came very late and when he obtained the funds from the Rockefeller Foundation, he really never participated in it very much personally. He continued to work in his own area and more or less turned it over to me.

VDT: Was it through him that you first became interested in the demography of developing countries?

BOGUE: More or less, yes. He had a colleague, as you know, P.K. Whelpton, another founding father, who was a tremendous expert in fertility and in cohort analysis, and they both had an international perspective. So working with them, I did become very much interested and tried to extend my own work in the U.S. to the study of population distribution in the rest of the world.

VDT: In 1953 you published Subregional Migration in the United States with Margaret Hagood. She was not at Scripps.

BOGUE: No, Margaret Hagood was the demographer at the U.S. Department of Agriculture in their Agricultural Economic Research Unit. She had been professor of demography at the University of North Carolina and had established what has now become the population and social science research center there. She was a civil servant and they were very interested in urbanization, the exodus of huge numbers of poor people from the South to the North even before the war and during the war, and then the movement off the farm to metropolitan areas. She later had an assistant, Calvin Beale, who did a great deal of work on migration with the Department of Agriculture. My book with her was a collaborative effort, in which I was using some of the money from the Rockefeller grant and she was using the budget from her office.

VDT: She, of course, was one of the early female titans in the PAA.

BOGUE: A very grand lady.

VDT: What took you to Chicago?

BOGUE: It was an accident, almost. Philip Hauser had accepted an assignment to help Burma take its first national census and he was to be away for one year. He asked me to come up from Scripps and teach courses in one quarter of that first year, which I did. Then the census assignment was extended and he could not return to Chicago, so he asked me to return the next year and do it for two quarters, which I did. At the end of that time, the University of Chicago offered me a permanent position with tenure, which I accepted.

VDT: Could we talk about some of your colleagues at Chicago at that time? When you went in 1953, Nathan Keyfitz had returned the year before to complete his Ph.D. and did not join the faculty until ten years later. But there was Philip Hauser, whom you worked with. Was Dudley Duncan there at that time?

BOGUE: Yes, Dudley Duncan and Beverly Duncan were there and Evelyn Kitagawa, all in demography. There were the three of them when I arrived and then Phil Hauser returned the next year from his work in Burma.

VDT: Among the many things that you were doing in those years was the work leading up to the mammoth Study of Population, in which you had two chapters.

BOGUE: Yes. There was a phase of trying to take an inventory of where various disciplines stood and Phil, when he came back, obtained a grant to do an inventory of population, which he and Dudley Duncan took on as their primary responsibility. I was still working on population distribution and was working on a book called The Population of the United States [1959], which was taking up much of my time. They assigned me, as I recall, two chapters in that volume, which I wrote, but the volume itself was their volume. They edited it and I was only a contributor.

VDT: In 1961, you became director of the Community and Family Study Center, which I understand had already existed under another name.

BOGUE: Professor Ernest Burgess had established a center called the Family Study Center in 1947. This center was focusing on the study of the family as a sociological unit and on the sociology of aging, with no interest in demography particularly. After I had been at Chicago some years, the United Nations invited me to go to Chembur, near Bombay, in India to work there during 1958-59 as an instructor at the demographic center. I had become very interested in fertility, working with P.K. Whelpton, and when I was in Bombay, I saw the fertility problem in its stark reality and became intensely interested in it as a population problem and began working with the people of the Indian family planning association, whose headquarters were in Bombay.

When I returned to Chicago, I was convinced that I had more or less completed the cycle of work that I had been doing on population distribution and The Population of the United States was published; I had finished that before I went to India. So I resolved that I would enter a cycle of work trying to do something about the world's population problem. I was convinced that the pessimism that was extant in those days need not be all that deep, that something could be done about it. So I began working with other social scientists: Elihu Katz, a sociologist of communication; he had a monograph called Communication and Social Change. And I began working in social psychology on the theories of inducing behavior change--theories of persuasion, motivation, attitude change theory--trying to

apply this to the problem of fertility control. And at that time, there were funds available for starting some international training for people who wanted to work in family planning.

VDT: Where were those funds from?

BOGUE: I cannot recall whether my first grant was from the Ford Foundation or the Population Council [Population Council first, then the Rockefeller Foundation, Ford from 1963, according to John and Pat Caldwell, Limiting Population Growth and the Ford Foundation Contribution, 1986, p. 72]. The Ford Foundation had selected population as one of its areas of concentration and one of its first grants [in 1954] was to the Population Council, which had been created as a separate entity [by John D. Rockefeller III in 1952] to try to deal with the world population crisis. The Milbank Memorial Fund was also interested in the world population crisis and had several hearings on it, which I attended. So this was a period of very intense concern about the impending population explosion, the high fertility in the developing countries.

I was very much caught up in that concern and wanted to--I guess there was one difference between what I wanted to do and some of the others. I not only wanted to study it but also I wanted to help design experiments to try to deal with it. I was convinced from these studies of social psychology and motivation and attitude change that some kind of intervention could accomplish something in this area.

I think our first funds came from the Population Council. They were small funds to allow us to hold summer training. So I held these workshops, starting in 1961, in which most of my guests experts were psychologists, anthropologists, and people from the mass media, really people who were not demographers, but who came in and would teach people how to induce change in behavior and how to do mass education. There were people who talked about changing individuals and sociological change theory too. These workshops seemed to meet a need, because I had recruited a group that was trying to show how to change fertility behavior directly, without having to wait for economic development and so forth. And this enterprise was well liked.

This was training in communication for family planning. The Ford Foundation set up its own program in this. They had an area where they were doing biological research on contraceptives, but they also had an area in which they were specializing in developing family planning infrastructure to provide services.

VDT: Which came first--the Ford Foundation program in communication or your work at the Community and Family Study Center?

BOGUE: The work at the Community and Family Study Center.

VDT: You inspired Ford to go more into the field?

BOGUE: No, this concern that I described was very extensive. The Milbank Memorial Fund held some special conferences at which a wide variety of experts--anthropologists, mass media people--all talked so. I was only one of the multitudes. This all preceded the Ford Foundation. Then Ford set up its own communication program under a journalist named Bill Sweeney, as a result of these hearings. The international family planning communication program was a result of Ford Foundation's explorations. But I had begun working on this immediately when I came back from Bombay in 1959.

VDT: So you really were a forerunner in that field, that you could bring about behavioral change which would encourage contraceptive use?

BOGUE: Well, not a forerunner, because these people in India--there was a very famous Indian lady, Lady Rama Rau, who had helped set up the family association in India, and her assistant Mrs. Wadia. They were doing experimental work in the slums of India. So all I did was to participate with them and come back to the United States with the awareness that there were people who thought that something could be done about this. I wanted rationally to go to work at it, applying social science theory. I may have been ahead of most demographers, but I was only abreast of what was already a very thriving family planning movement.

VDT: The people who came to take your courses were from the family planning programs of those countries?

BOGUE: That's right. The Ford Foundation and the Population Council, and also the Rockefeller Foundation, took the approach that this had to be part of a health and medical program; that you couldn't bring down the birth rate just by attacking it directly and ordering people to stop having so many children. It had to be institutionalized and the context would be health--maternal and child health--and family planning would be linked. This was a very early policy decision.

So this meant working with health educators and also with media people and educators of all types. And it was very strange to demography, that is, at that time most demographers regarded this as alien territory. And I discovered that in my own university this was the situation; that this special interest that I'd acquired overseas was not really fitting too well with the ongoing program in demography at the university.

I had worked with Professor Burgess for all of the years since I arrived at Chicago. I had helped him edit a book of his writings, called Basic Writings of Ernest W. Burgess, and then I helped him prepare a volume of thesis summaries of students whose work he had supervised, called Contributions to Urban Sociology, which was edited by him and myself. He was at that time very elderly and the Family Study Center had no funds. Meanwhile, the Ford Foundation was very interested in this training I was doing and was offering supporting funds. So I asked Professor Burgess if we could reorganize the Family Study Center and call it the Community and Family Study Center to incorporate my interests in human ecology and to begin this training. He agreed and the Population Council gave us a little bit of money and then the Ford Foundation gave us more to continue our summer workshops and also to start doing some field research of an advocacy nature--how to improve family planning services in developing countries.

For three or four years, I remained as a member of the Population Research and Training Center, with Phil, Evelyn, and Beverly and Duncan. So there was a period in there in which I participated in both centers. Then the demand for the family planning action work became so heavy that I just could no longer maintain productive work in both, so I began devoting 100 percent of my energy to the Community and Family Study Center.

VDT: But you continued as a professor in the department of sociology?

BOGUE: Oh, yes. I taught courses in research methodology and a course called "Principles of Communication," which was an outgrowth of this interest in modifying human behavior. We had a communication laboratory with semi-professional television, radio, photography, printing, and graphic arts equipment for training people for family planning communication. And I taught courses in social change. I even taught a course in introduction to social psychology for a couple of years, because there was no one teaching such a course in the department and my foreign students needed it. The Ford Foundation not only sent people to our summer training workshops but they also began sending people from overseas to Chicago for master's degree training. So we received a long stream of people coming

from overseas to receive training in population but with special emphasis in advocacy work, trying to help design family planning programs.

VDT: Who were some of those? In a recent 20th anniversary issue of Family Planning Perspectives [November/December 1988], Peter Donaldson and Charles Keely have an article on the international population scene ["Population and Family Planning: An International Perspective"] and they say in it that "during the 1960s and 1970s, a small group of demographers trained at the University of Chicago played decisive roles" in shaping population policy, family planning programs, and developing population research and training in their countries. They mention Lee-Jay Cho in Korea, Mercedes Concepcion in the Philippines, Visid Prachuabmoh in Thailand, and Harjono Sujono in Indonesia. Were they all your students? Are they in this group you're talking about?

BOGUE: They were in the department of sociology and they went through the general population program. Some of them did more of their work with the Population Research and Training Center; some did more with me. But all of them were exposed both to formal demography and to this action demography that I was sponsoring.

VDT: Donaldson and Keely go on to say that the cooperative programs with American universities, funded by the Population Council and AID, were important in establishing population research and family planning in Asia, but they have declined in importance. Do you feel that they should be revived, that developing countries still need that kind of training in the U.S.?

BOGUE: I think it was critically important in those days, because the idea of intervention for family planning was fairly new in most countries, except India; India had started in 1947. But, for example, in Pakistan--in those days there was East and West Pakistan--in the Philippines, in Korea, in Indonesia, this was a new idea. So these fellowships brought responsible people and trained them, who went home and did become very instrumental in setting up policy programs. I do think that the Chicago program was beneficial, because these people went away with good solid training in demography and they also went away with a belief that something could be done.

VDT: Let's talk about your belief that something could be done. You're quite right that the demographers at that time, some of them, were a bit leery of this new field of family planning activist research. It goes back a bit to the early days of PAA and Margaret Sanger. They tried to keep Margaret Sanger out of the direction of PAA, which she helped to found, because they wanted to keep PAA "professionally pure," is the way Frank Notestein put it, although you mentioned Notestein in your recent paper given at the Psychosocial Workshop ["Family Planning in the 1990s: The Unfinished Demographic Transition," 1987] as being an activist, really, early on.

BOGUE: That is one of the strange things that I never quite understood. All the old founding fathers of demography, although they had some quarrels with Margaret Sanger over the philosophy of science, I think saw a very thin line between action and research. For example, P.K. Whelpton was called to Japan by General MacArthur as soon as General MacArthur became commander of Japan after the surrender and was in charge of the transition government. One of the early things they did was to form a population advisory commission, what to do about Japan's population problem. The birth rates were very high. They were repatriating 9 million Japanese from the islands that were conquered, dumping them back on the mainland, and everybody foresaw economic disaster. So a group of very famous experts, physicians and demographers . . .

VDT: Was that the trip that Notestein and Irene Taeuber made, in the late 1940s?

BOGUE: Yes, P.K. Whelpton was a member of that team. [Notestein, Irene Taeuber, Marshall Balfour, and Roger Evans, but not Whelpton, made a trip in late 1948, sponsored by the Rockefeller Foundation, to report on public health and demography in a number of Far Eastern countries, including Japan, according to the Caldwells, op. cit., pp. 17-19.] They [the population advisory commission to General MacArthur?] worked very industriously to try to design a family planning program for Japan. One of the things that they recommended was the legalization of abortion, which Japan did do and which was very instrumental in . . .

VDT: Brought the birth rate down from 34 to 17 per thousand in ten years.

BOGUE: Yes. Well, that kind of interaction between demographers and action people, I saw it work in the very early days when I was at the Scripps Foundation, so when my cycle came around, I really had not been prepared for this. The division between advocacy and scientific research in demography is of comparatively recent origin. It is not indigenous to the Population Association of America. It is something created by people who entered demography in the 1960s, 1970s.

VDT: The early people, like Whelpton and Notestein, felt the responsibility to be activists also?

BOGUE: And Clyde Kiser and all that group, yes.

VDT: Not, apparently, Phil Hauser.

BOGUE: There were a group of people who reacted negatively to the idea. I think the leaders in the opposition were Phil Hauser and Kingsley Davis. They were two of the stalwarts who were skeptical of this idea, felt that social engineering and social science were two things that shouldn't mix.

VDT: Yes. And yet Kingsley Davis has always said that you have to change people's motivations; that their desired family size as shown in most surveys was too high and you had somehow to bring it down to two. How did he think that was going to happen?

BOGUE: I read Kingsley Davis, the sociologist, from the beginning and I could never understand why Kingsley Davis, the demographer, took this position; it was almost inconsistent with his own work. I would have predicted that Kingsley would be one of the first to support it. I never could understand why he took this stand, and still don't.

VDT: I'll have to ask him; I'm interviewing him in three weeks.

Tell me about some of your leading students, other than those we've mentioned. How many people did you train over the years in the Community and Family Study Center and in the workshops that you later held overseas?

BOGUE: We held workshops for 20 consecutive summers. Some summers we held two and one summer we even held three workshops at the same time. We held them in English, Spanish, and French and there was one summer when we had all three languages going simultaneously, with 128 people. Also, we began holding these workshops overseas. We held workshops in countries like Columbia, Korea, Costa Rica, Turkey, Guatemala, Liberia, Sierra Leone, Kenya, Lesotho, Ghana, Morocco, Egypt, Ecuador, Nigeria, and Senegal. This was 5-20 years ago. We trained--I think we kept count once--about 2,200 people in Chicago or overseas, about one-third of them in-country,

overseas. There were some 130 master's degrees awarded in population and family planning from the University of Chicago in this period.

Now I was involved in the training of the M.A. and Ph.D. students. They could not all be called my students, but they went to the department and all of them took a broad course in demography and in the courses I was teaching at the same time. There were some very influential people, of whom Lee-Jay Cho and the ones you mentioned were only a few. There are many, many more.

VDT: Visid Prachuabmoh, the Thai, who has a daughter at the university now, I understand, and Mercedes Concepcion?

BOGUE: Mercedes Concepcion studied more with Evelyn and Phil. I was in India when she graduated, so I cannot claim Mercedes as my student. But Prachuabmoh, yes, he was my research assistant, and Lee-Jay Cho also, and I was chairman of their dissertation committees and was very much involved in their preparation. But, to repeat, they took very solid courses in statistics and research methods, in addition to working with me.

VDT: Tell me about some of your other leading students, not necessarily just from developing countries. You have always been so good about involving your students in your projects. Your students and younger colleagues, for instance, were involved in your books and projects and the first issues of Demography.

BOGUE: There are many of them; some of them are very well known. There is Salustiana del Campo, who is chairman of the department of sociology at the University of Madrid; James Palmore, who is at the East-West Center in Hawaii; Walter Mertens, who's now at Harvard; Amy Tsui at the University of North Carolina; Jay Teachman at the University of Maryland; Michael White, who is just going to Brown University; Harvey Choldin in the department of sociology at the University of Illinois; Mahendra Premi and Bhaskar Misra of India; Edmund Murphy of Statistics Canada. And that's only some; there are many more [see lists at end of interview].

VDT: What a tremendous influence you have been!

BOGUE: Well, mostly I hired them as research assistants and paid them to work either at training or at research and insisted that if they were going to work with me, they had to take a basic course in mathematical statistics and basic courses in research methodology, taught in the department of sociology. I didn't want them on the payroll if they didn't get good statistics and good research methods. That tended to weed out all but the very best. So I can't take much credit for their accomplishments. It's just a matter of having selected and offered good employment to some very, very capable people.

VDT: And those who came from developing countries, this must have established a network that led to even more people coming to your programs.

BOGUE: Yes. South Korea is an example. In the early days of the family planning program in South Korea, we offered two workshops on family planning communication there and that helped result in a steady stream of people from Korea coming to the University of Chicago for training at the master's and Ph.D. level. Many of the leaders of the present Korean family planning movement are from there.

The same could be said for India. We trained a large number of people from India at the master's and Ph.D. level.

VDT: They knew of you because you had been there to help set up the training center?

BOGUE: Partially, but many of them were sent to our program by the Ford Foundation. Also USAID in those days selected, I think, 16 of its young middle-level professionals and they got interested in family planning and population and were sent to American universities for intensive training. Six of those came to Chicago and took master's degrees. Today, some of the leading people inside AID are people who took that course. There are Gerard Bowers, John Paul James, Scott Edmonds, Silverstein; all of them are or were top population officers.

VDT: We've covered pretty well your split with the later people entering demography who felt that the activists, the family planners, were not quite acceptable as part of the field. That seems a pity.

Of course, you aroused a lot of attention, both in the 1960s and 1970s, with your projections of eventual world population size, if family planning programs continued. In the mid-1960s, you were writing that world population could stabilize by the year 2000 at 5 billion, if family planning programs continued. Your projection for 2000 went up to 5.88 billion in the Population Reference Bureau's Population Bulletin which you did with Amy Tsui, "Declining World Fertility: Trends, Causes, Implications" [October 1978], and there was a big flap over that.

BOGUE: Yes. I was overenthusiastic about the power of modern media to persuade, so that in my early days I really underestimated the task of informing people. Also I underestimated the amount of resistance that would be encountered in these developing countries. I felt that in India the government had already seen the light and was trying to do something about it. Other countries seemed to be doing the same thing in rapid sequence. So I was overly optimistic in terms of how fast it could be done and how long it would take. I think the basic idea was correct, but my timetable was seriously off.

I'd like to emphasize that there was never any split between me and the demographers. I never felt any sense that I was outside demography. I've always felt that when I was working at family planning action, I was simply trying to do a demographic experiment, in which you'd try this and observe the results, just like a biologist performing a search for a cancer cure or something like that. So that I never felt alienated from demography. I felt more like a demographer who was being punished for being an active family planner as part of his job.

VDT: I think that's a good way to put it. You have tried to persuade demographers. In your 1964 PAA presidential address, "The Demographic Breakthrough: From Projection to Control" [Population Index, October 1964], you felt that in the 18 months prior to that there had been a breakthrough. That was June 1964 and you had been traveling in Asia and had seen more of these experimental programs set up and you had obviously come back full of optimism. However, you said that, "Some demographers seem to be apprehensive that the demographic breakthrough is causing many more familiar lines of research to lose their importance, or that our fraternity will be invaded by some new species of professional persons from whom they would prefer to be segregated." So I think you already had a sense that perhaps you were a bit of a pariah. Then you went on: "This fear of integration is wholly unfounded, in my opinion. The great responsibility of demography is still one of scientific research." And you went on to say that this would simply open up another branch of scientific research.

BOGUE: That has actually happened. You can look at this meeting today. In the early days of PAA, there would never have been any sessions on child care, the whole thing about family structure.

VDT: These spinoffs from family planning are now acceptable fields in demography.

BOGUE: I would like to emphasize that I was not alone in the faith that something could be done. Perhaps I was more vociferous than many. But there was also Ronald Freedman, Joe Stycos at Cornell, and, of course, the fact that Notestein himself resigned as director of the Office of Population Research at Princeton and went as second director to the Population Council [in 1959, following Frederick Osborn], which was an organization specifically set up to do something. There were several demographers who did have faith and did see that in order to do something about it you had to work with physicians and psychologists and mass media experts and so forth. So I was not alone at that time.

VDT: Can we talk about your current views, which you expressed very well two years ago in your Psychosocial Workshop paper, "Family Planning in the 1990s: The Unfinished Demographic Transition." You have told me that you hope to expand on that [later revised and published as "The Unfinished Demographic Transition: Family Planning in the 1990s," in Donald J. Bogue and David J. Hartmann, eds., Essays in Human Ecology--3, 1990]. You said there that now there needs to be a revival of concern about the population problem, because demographers and others have been rather complacent although the population explosion has certainly not been tamed.

BOGUE: Yes, I'm still trying to think this through. The basic point is that the demographic transition has dragged on longer, so the world population has grown to a huge size and, meanwhile, the age structure has not changed as dramatically as I expected it to, so that the eye of the storm, the strain on national economies, has yet to hit. It is true that birth rates are falling in many countries, but they've only been falling for a few years. Meanwhile, this mass of humanity that's been born in the last 20 years is now entering the labor force, establishing families, and placing huge demands on national economies for employment, so that the really terribly drastic period of adjustment to the population explosion will take place between 1990 and the year 2010.

I do think that if birth rates continue to decline, the world will have an easier time after 2010. But between now and then, these developing countries desperately need help, not only for contraception but in helping build up their economies to provide employment. And it's exactly at this point in time when the Ford Foundation has zero money for family planning, ISAID is cutting back, and the United Nations Fund for Population Activities is being inundated with requests. So it seems to me that the demographic transition is being abandoned just at the point when it is most in need of external support.

VDT: I absolutely agree and I hope you'll go on writing on that. Are you discouraged? You end up that paper, in its version two years ago, saying that you try not to be too discouraged.

BOGUE: Well, originally, in the first phase of the "demographic bomb" or the "population explosion," people were predicting mass starvation in the late 1970s and in the 1980s and 1990s. That's no longer a threat, except maybe in specific instances. Now what's threatening is the idea that the world will not be able to substantially improve its level of living in the next 20 years.

VDT: There'll be that bomb of young people needing jobs.

BOGUE: And probably there will be no more employment, but if fertility can be brought under control even faster, it would still make it easier for those young couples entering the labor force to achieve a better life faster than if they are allowed to continue to have three and four children for lack of family planning services. This is why my concern is that we're giving up too soon on the population

problem, in terms of strong, strong support. We're giving it second-rate priority at least ten and 15 years too soon.

VDT: Well, there's the current concern over the environment and some people are recognizing that population enters there. I hope that will give it more attention.

I'd like to switch back to you and ask about the role of your wife, Elizabeth. She was very involved in the summer workshops and she did the computer programming for Principles of Demography [1969], I notice. Where did you meet?

BOGUE: I met Elizabeth in Washington, D.C. I was stationed there as an industrial statistician in the Navy Department in the early days of the war. While I was at sea, she entered the University of California and started her degree in mathematics. She finished after the war and became interested in computers. I was an old IBM operator and computers were an interest we shared.

VDT: You must have worked with the first computers that ever came out.

BOGUE: IBM machines, before the computers. I learned to operate IBM machines as a student at the University of Michigan; they were called accounting machines in those days. Then during the war, I became quite experienced with IBM computing equipment. After the war, the Scripps Foundation loaned me to the Census Bureau for a year and a half to work on the 1950 census and they were just introducing computers. I helped put out the migration publications for the 1950 census and I did some work on economic areas with Calvin Beale in that time while I was in Washington.

So I had maintained contact with regular IBM computers and then when mainframe computers were developed, there were three people at the University of Chicago in our social science division who learned them and I was one of the three. So I started with mainframe computers as quickly as they were invented. In those days, I held a special contract with AID for family planning evaluation, in the Community and Family Study Center, and one of the things we did was to adapt family planning records, family planning research procedures, to mainframe computers, especially small ones that were being installed overseas. So we began writing computer programs that could be used on these small computers.

My wife Elizabeth took charge of this computer work. She had meanwhile become a very good computer programmer. With mathematics and statistics and her computer programming skill, she not only programmed the routines for use in computers in developing countries, but she also taught this to the students who were trained. One of the reasons why computers went so quickly into family planning in Asia, particularly, was that these people who did master's degrees with us went back not only with computer programs that they could run on their own computers but they also knew how to program for them. So Elizabeth was a partner in everything that we did and was a very, very good teacher in this area.

VDT: And now you're writing programs for microcomputers in the field. You have a new software manual, I notice, is that correct?

BOGUE: We have a new computer program called POPSYN, written by David Wilmsen, who is at John Short Associates, and we're applying it to demography. I'm working on that with Eduardo Arriaga and that's to be completed within this coming month. We hope it will make possible high-level demographic research anywhere in the world where there's a good PC. It will be published at a very reasonable cost, because we produced this for use, not for money. So we will be distributing it at the lowest possible cost and hope that it will enable people in-country to take their own survey and

analyze and interpret it without having to go through the cycle of dependency on an external source.

VDT: The Demographic and Health Surveys are now using microcomputers to collect their data in the field and are able to analyze it very rapidly. Have you worked with them at all?

BOGUE: I know their system, but it's still heavily dependent on the central office at Westinghouse in Columbia [Maryland]. They're using a program called ISSA, which is a bit difficult to learn; it is not a user-friendly program. Yet when they're finished, their files can be translated into a simpler language outside ISSA, which will enable people with the programs that we're sponsoring to analyze the data further in their own countries. The programs that I'm now sponsoring will enable people to analyze both the sample survey data and their census data completely within their own countries and not have to go through an intermediary, such as DHS.

VDT: Well, that answers my question about whether countries can now stand on their own in demographic research and training. If they're going to be able to, it will be largely thanks to you.

BOGUE: No, no--thanks to a large number of people with the same idea. There are many people who have been working to help the developing countries become more self-sufficient in terms of research and I'm only one of several who have this philosophy. For example, Eduardo Arriaga and the Census Bureau have been training people over the years to try to take their own censuses. They have a complete system now whereby a nation--a smallish nation at least--can take a census completely with microcomputers and tabulate it. Now this new cycle of programs that we're producing and sponsoring will allow them to analyze it--follow up a census or survey immediately with sophisticated analysis.

We've given them the training. They come to the United States. They get a good degree from Harvard or Columbia or Princeton or Pennsylvania. They go home with good technical knowledge. And now they're being given good software. This lack of software has been a kind of a bottleneck, and solving that software problem will allow somebody to sit down in some remote corner of a place like Ivory Coast and do very sophisticated work, which only five years ago could be done only maybe at ten places in the United States and Europe.

VDT: That's great.

Going back, one thing I wanted to ask you about was your early work in Chicago and the rural South, your efforts to work with low-income, high-fertility populations in the U.S. That was, I understand, an important part of your work at Chicago.

BOGUE: That was a part of the family planning experimental work. As I said, I read about social psychology and persuasion and cognitive dissonance and all that theory, but no one had really tried to apply it to the population problem; this was just abstract psychological reading. So I set out to do some experiments to see if it worked. I worked in Chicago in the slums. At that time, the black population of Chicago had a crude birth rate of 33, which was the same as India.

VDT: In the early 1960s?

BOGUE: I started this as soon as I returned from Bombay, using some research money from the Ford Foundation. We started conducting fertility surveys in the slums and we asked nice attitude questions. I worked with psychologists to formulate these. We discovered that the black people of Chicago were having many more children than they wanted; that there was something called an unmet need. And I worked with Planned Parenthood of Chicago to help set up special family planning centers in those

areas, to see if people would come. We did experimental mass communication to influence them to make use of these services. These were small family planning experiments in Chicago and they were working. We brought out a couple of monographs on that.

Then we thought we would try it out in the rural South to see if it would work in rural areas. We chose the very poorest set of counties, black counties, in the South, in central Alabama, and a pocket of the very poorest white counties in the Appalachians. Working with local people, we set up experimental family planning programs and distribution systems in each of these. We evaluated those and they worked.

VDT: Over a period of several years?

BOGUE: About four years.

VDT: And the main thrust was communication?

BOGUE: That's right. The main thrust was that if you take people who have high fertility and give them good information, correct information, about family planning and provide them with good services at low prices, they will use the services.

And it worked in every case; it worked in Chicago and in both the black and the white areas in Alabama. We were using these as demonstrations in our workshops in the early days. The people we were teaching could go see the centers we had in Chicago and we showed them the results of the southern experiments. These are all written up a series of monographs. Our center produced research reports on all of these experiments.

Meanwhile, I was working with the Ford Foundation and other people helping to set up new family programs too.

VDT: The actual programs or the communications components?

BOGUE: The communications components.

VDT: You mentioned Chile; was that one of them?

BOGUE: Yes and no. I went to Chile to work with CELADE. Carmen Miro invited me to come down to do some work with them on migration. While I was there, I worked with Benjamin Viel and the family planning people at the University of Chile medical school. They had some experiments going in the slums of Santiago; this was in 1968, 69. I had been doing similar work in Chicago. I observed and worked with them. So I did both family planning research and migration research at the same time while I was there.

VDT: What were some of the places you did go to set up family planning communications pilot programs?

BOGUE: I was involved in establishing a family planning communications program in Colombia. There was APROFAM, an association of medical faculty, and a very charismatic leader, Hernan Mendoza, set up a family planning program for the medical schools of Colombia. Every year for four or five years, the Ford Foundation held an annual meeting to review this and help plan for the future. I attended those. Then the Ford Foundation gave us funds to establish a research adviser there and Henry Elkins, who's now with MSH in Boston, spent a year there, helping them set up a record-

keeping system for their family planning program. So I worked very industriously there.

I went to the Philippines for the Ford Foundation when they were doing their first exploratory work there. I was sent to Pakistan to work with Dr. Aktar Khan. He set up experimental family planning projects in what was then East Pakistan, now Bangladesh.

VDT: That was in the 1960s too?

BOGUE: Yes.

VDT: How did you manage to fit in these consultancies? You had your workshops going in the summers; you were teaching. When did you go?

BOGUE: These were shortish trips--two or three weeks, sometimes five. Mostly they were accomplished by working with local people, making arrangements and then establishing one of our staff in residence.

We helped set up the Indonesian program also. I went there and stayed several weeks, very early on in the Indonesian program. Harjono Sujono was just graduating from Chicago and he went back and later became director of that program. But meanwhile, I sent Jay Teachman and Jeanne Sinquefield and they resided in Indonesia as research advisers and our representatives for three years, I think. They were not only helping with the work but also doing local training. Henry Elkins and Teachman were very influential. The family planning success in Colombia and in Jakarta is due in part--on the research side--to these two people whom we supported there.

VDT: And all the time, meanwhile, you were producing books and articles, in part, as you explained, on your experiments, such as the ones in Chicago and the rural South. Jay Siegel in his interview mentioned your prodigious output and I have quoted to you what Nathan Keyfitz said. He mentioned you as one of the leading influences on his career. I haven't asked you about your influences; I'll let you think about it. But this is what Nathan said of you: "I thought there was a good deal of no-nonsense in Don. He had an incredible capacity for work. He produced books the way other people produce articles. His books are very solid, very well organized. I learned a great deal from Don."

So, I'll ask you two questions: Who were your leading influences, and something about your publications.

BOGUE: I'll mention publications first. I have tried to write a tremendous amount--more than I should have, because in this family planning action phase, we didn't have any training materials. There were abstract articles from psychology and so forth, but the health educators had produced nothing on family planning, so that we were offering workshops every summer and there was no way to give them something to take home, except to write it. Later on, it became easy because the writing began to flow in and all we needed to do was to duplicate it. So there's a lot of writing that I did in those days that I don't really think I'm proud of as an academician. They were just potboilers turned out because something had to be done for training. There's a lot of that stuff that I hope people don't use to rate my academic skills on.

But meanwhile, I did always try to be a demographer and a responsible scientist and do solid research when I had the time. I didn't do as much of it as I should or could have, maybe, but there was enough of it. There was The Population of the United States [1959 and 1985] and Principles of Demography and then some of these manuals that we turned out with computer programs that I think belong in the mainstream of demography.

VDT: Do you know how many books and articles you've done?

BOGUE: No.

VDT: You never kept a list?

BOGUE: I have a list but I have not kept a count. And I haven't rated the ones that I would like to see forgotten.

VDT: Well, which publications do you consider your most important, and why?

BOGUE: Well, Principles of Demography is important, in my mind, because--almost like the training materials--there was not a book in demography that had an international orientation that could be used as a textbook at an advanced level. And, unfortunately, there still isn't one, here in 1989. David Yaukey has a very nice book which is good for seniors and maybe first-year graduate students, but it is strongly loaded with U.S. materials [Demography: The Study of Human Population, 1985]. So I deliberately took time out to write Principles of Demography, much as these training materials, because there was nothing you could use for training these international students. And it did serve that purpose.

VDT: Its publication [in 1969] was perfect timing for me. I was just then at Georgetown. I'd returned to university--a leader among housewives going back to school--because we'd lived in Asia and I was concerned about population. I looked at my copy the other day; my husband had given it to me, inscribed "to a budding demographer." It is 916 pages and it then cost \$16.50, hardcover. Your Population of the United States, the later one of 1985, is 738 pages and the retail price in 1987 was \$138. The cost of producing books these days! . . . Well, of course, the manuals you put out were published by Community and Family Study Center.

BOGUE: Those prices are put on by the publisher. I would have given them away if I could. Principles of Demography, I think, has served to organize thinking about population research, taught basic principles, so I am pleased that it served that purpose.

The one single piece of writing that I did that I am most proud of, however, is this little article that was published in The Public Interest, called "The End of the Population Explosion."

VDT: That was a controversial one!

BOGUE: It was published in 1966 [Spring 1967] when people were in the depths of the demographic gloom and talking about how the world was going to be torn asunder by the population bomb. And the fact that it was controversial and caused a lot of discussion, I think ultimately brought the field of demography around to the idea that something could be done. In retrospect, it may have been--it was definitely overoptimistic, but it was in the right direction. I think coming at that time and shaking the demographic tree and causing this discussion hastened the time when the demographers are finally now coming together with the public health educators and everybody else.

VDT: Great. Well, I think that's plausible. You're right, that was an interesting article.

BOGUE: But, to repeat, there are several people of the same conviction, among them, Stycos and Ron Freedman. Of the people who are/were most optimistic about the population situation, Stycos,

Freedman and myself were probably the three leading ones in those days. Plus many at the Population Council; all of the professionals at the Population Council were of that philosophy.

VDT: Well, not some who are there today, like Paul Demeny, who has taken you to task on occasion.

BOGUE: Yes.

VDT: Well, he's listening to all sides. That session we just attended ["Economic Consequences of Population Growth," chaired by Demeny, session 17 at 1989 PAA meeting]; he'd invited Julian Simon to give a paper.

This is somewhat in the same vein: What accomplishments in your career have given you the most satisfaction?

BOGUE: I really believe training all those people in those summer and overseas workshops. I've enjoyed teaching at the university and I am very pleased and proud to have been a part of turning out some of the intellectual leaders in the field of population studies. But those workshops--2200 people in 20 years from, I think at one time the count was 101 different countries from which we received people--and all of them going home with the sole intent of trying to do something about the population explosion in their country. I honestly believe that the batting average is pretty good and that they did go home and did make a difference. So if I had to point to one thing, it would be something that most demographers would laugh at. But having trained these advocacy people was to me a thing that I'm really pleased to have done.

VDT: Great, it's a wonderful accomplishment.

BOGUE: The second is to have established the journal Demography.

VDT: Okay! Just one more question and then we'll get onto PAA. I want to ask about the Social Development Center. I notice that you're going to have summer training there this year. What is it doing?

BOGUE: The Social Development Center is really a sequel to the Community and Family Study Center. The University of Chicago ultimately became disenchanted with family planning advocacy. It began to have racial overtones. When population moved into Africa, the public press and the right-to-life movement aroused opposition to international family planning, the controversy concerning various compulsory aspects of family planning in some countries. So it was very easy to see that the university was feeling uncomfortable and that they were wanting to put more and more limitations on grants. So we set up the Social Development Center, on the advice of international donors, as a device for allowing me to continue to work in this area. The Social Development Center accepted grants and contracts for international work. Then I simply purchased half of my salary and paid it to the university and this allowed me to teach half-time and I could spend the other half working as I chose on these advocacy and other programs.

Then when I retired last year, the Community and Family Study Center was disbanded as an entity, but the Social Development Center is continuing along. And we have had very good support from the U.S. Agency for International Development, with special emphasis on Latin America. In recent years, we've been specializing in promoting training in microcomputers for fertility surveys and demographic research. We've held quite a few workshops, very much of the same old philosophy, but just under a new name. It's an independent entity; it's affiliated with the university but financially

independent. I'm working there without salary, just using my retirement salary, and hope to continue doing it as long as there's any use for that center.

VDT: Which I'm sure there will be for years.

Now, let's go on to your PAA recollections. What was your first meeting? I notice that at the 1948 meeting you gave a paper on "Metropolitan Decentralization"; that was at the University of Pennsylvania in Philadelphia. Was that your first meeting?

BOGUE: No, my first meetings were in Princeton; we used to hold them there every spring. I went to Scripps Foundation in 1946 and I think in the autumn of 1946 . . .

VDT: In 1946 there was a meeting in Princeton in May, the first since 1942, and another one in the fall in New York, to catch up with foreign demographers after the war. May 1947 was back in Princeton.

BOGUE: I attended two at Princeton, so I think 1946 and 1947. Meanwhile, I had been working at Scripps on population distribution and so my first paper at PAA, which you mentioned, was based on this work.

VDT: Can you recall what the early meetings were like and who the leading figures were then?

BOGUE: The early meetings were very small, family-like meetings. Everyone could stay at the Princeton Inn. And there was only one session at a time. The whole group would take up one topic and discuss it, because there were not enough people to have two or three sessions at the same time, as now. And it was always very well attended. People didn't stand around the halls and talk. When the meeting took up, everyone went in and then they would interact after the meeting. But they were very businesslike, that is, professional; people were serious about what they were doing. They are now, but it was much more informal then.

The big people, of course, were Notestein and Whelpton and Thompson and Clyde Kiser and Irene Taeuber.

VDT: And Frank Lorimer?

BOGUE: Yes, Frank Lorimer was very active in those days. There was a big age gap because of the war. There were these biggies who had established the Population Association of America before the war and then there was this group of young people who had graduated just after the war. But there was a large number of young people just interested in this, especially as the population crisis deepened. So that the Population Association of America was growing rapidly in those days.

Among the younger generation in those days were, of course, Norman Ryder and George Stolnitz and a group of graduate students who were coming out of Princeton, because Princeton was the principal training school at that time. The programs hadn't really started at Michigan; the Chicago program didn't start until 1954. So in the late 1940s, it was really the East coast group from the Census Bureau and the Office of Population Research, and then the United Nations professionals; the United Nations formed a Population Commission [and Population Division, in 1947]. Then there were some very famous people from the South: Margaret Hagood and Rupert Vance from North Carolina . . .

VDT: And Hope Eldridge?

BOGUE: And Hope Eldridge. They would come every year. It was a very nice group; everybody

was family.

VDT: Do you regret the change, the meetings have grown so?

BOGUE: No, if you don't grow you die. I think that this air of close-knittedness was one of the things that made it possible for the PAA to grow and expand as it has and still hold together. There is still that air of informality and friendship, I think, today in the PAA, much more than in the American Sociological Association, which I know. We have been able to ride out the crisis of the 1968 student rebellion and so forth, with all the various quasi-radical offshoots of the younger generation.

VDT: Concerned Demographers?--for a while.

BOGUE: But they never did challenge the establishment, because the establishment was always tolerant of them; it didn't try to reject them. I think that early family feeling still persists. And I'm glad to see the Association as big as it is today and I'm very happy with the diversity, heterogeneity, of the meetings. The only thing I miss right now is there's not a decent session on human ecology, which interests me still.

VDT: So you would encourage some of the offshoots? Applied demography gets much more attention now.

BOGUE: Fine, that's wonderful.

VDT: PAA is still somewhat elitist. There are just 2,600 members [2,679 at the end of 1989], which is small for a professional association. But it's obviously very much larger than it was.

BOGUE: I think that sooner or later a professional association has to plateau in its membership, unless it becomes diluted with too many people who join with no professional interest. For example, the geographers became too popular and their membership became almost anybody who liked to get a good magazine once a month. So the geographers had to start forming more restrictive associations to keep the professional core more oriented. The PAA could grow more, but I'm not unhappy that it isn't 10,000 people. I think it's more or less grown at the rate that it can maintain professional cohesion.

VDT: You say you like the diversity of the sessions that are offered in the meetings, so you don't mind that there are eight simultaneous sessions?

BOGUE: No, not at all.

VDT: Great. You go to the Psychosocial Workshop; the people there are delighted that you do. Obviously that dates back to your interest in social psychology.

BOGUE: Yes. The Psychosocial Workshop group were a group who were interested in family planning as motivating and educating people. Dr. [Henry] David has always been one of those fellow travelers, from psychology, but with an interest in population. That group was more or less people who were not demographers, but from other disciplines of a psychological nature who were interested in population. And if I had had my way in the early days of family planning, those people would have been welcomed right into the fold and their sessions would have been a part of PAA. It didn't turn out that way, for reasons that you know as well as I. But the fact that this group comes every year and holds its sessions two days before the PAA meeting and that there are members of PAA who do share

an interest in common, interact with them. Now this year, there's a joint session for the first time ["Population Policy and Private Behavior: Potential Conflict?", session 8 at PAA 1989 meeting, co-sponsored by the Psychosocial Workshop].

VDT: Yes, I'm amazed. It only happened in the 17th year of the workshop, although many people from the Psychosocial Workshop do give formal papers in the main PAA meeting. You say if you had had your druthers, the Psychosocial Workshop people would have been welcomed to the fold much sooner.

BOGUE: And I think that they will. For example, some of the newer problems, such as AIDS research, has such a huge psychological component. Demographers cannot avoid working on AIDS and they cannot avoid the psychological process. And the same thing is proving to be true of fertility and of migration and nuptiality. And the psychosocial group is interested in all of those things. I don't know what the future holds, but . . .

VDT: They started as abortion research. The title for the first two years was Abortion Research Workshop. I was working for Henry David when it was started in 1973 and his abortion research was funded by AID--a long time ago! Now abortion is in again, as you heard from Jackie Darroch Forrest and Henry David, both of whom were asked by Surgeon General Koop to be on the panel for his abortion report. Abortion research is timely right now because of the threat to Roe v. Wade.

BOGUE: I think the Psychosocial Workshop people have been courageous in places where the Population Association of America has been cowardly.

VDT: It's also a smaller group where there can be a lot of give and take. I think you've mentioned that you like that too.

Now, let's talk about your founding of Demography, which you say has given you the second most satisfaction in your career. That happened in the year that you were PAA president, in 1964. You did write an excellent vignette on "How Demography was Born" [PAA Affairs, Fall 1983], but I'd like to ask some other questions. One thing is that you mention in that vignette that PAA had discussed for some time having a professional journal; there had been two committees set up for that. But Norm Ryder claims that he can take credit for the idea of Demography. However, there was no support at Wisconsin, where he then was; there could be no university support for this outside activity. And he went to Michigan and they were in the same position. Then you agreed to take it on at the Community and Family Study Center.

BOGUE: I would not argue with Norm. I don't remember all the details leading up to it, but there was consensus at the PAA membership meeting that we should have a journal. There was no one speaking against it.

VDT: Except for cost.

BOGUE: Yes, it was simply a matter of dollars; nothing more than dollars. So that year that I became president [1963-64], I went to Oscar Harkavy of the Ford Foundation and said that I would like to try to divert some of the money that he had given us for our work to editing a journal. I assured him the costs would be minimal. I would do the editorial work without pay and that all I would need would be the salary of a person to help me edit it and for referees--mail costs and so forth. And he approved it.

So that got it going for one or two years. We put a very low subscription rate on it. It was

optional; if you were a member of PAA, you didn't have to buy Demography. But it immediately took off. People liked it, the idea. Then we got a little National Science Foundation money to help support it.

So I don't take credit at all for having the idea of Demography. I take credit for precipitating it; just saying this is the year it has to happen.

VDT: And it happened in a magnificent way. That first issue of 1964, which was 374 pages, is a bible for students; again something I used in my studies. It had articles ranging from Conrad Taeuber on the 1960 census to Lee-Jay Cho's fertility estimates for major countries of the world. You had a tremendous number of outstanding contributors in all your issues. And you corraled all your young students and assistants, like Lee-Jay Cho, Walter Mertens, Jay Palmore, and Gerry Hendershot, to work on it.

BOGUE: Yes, there was a tremendous demand for it, so when I became editor I wrote to everybody I knew and asked them to submit; I asked at the PAA meeting that nobody send their article off to another journal until we had had a chance to review it. And the stuff just poured in. It was just part of the truth that the demand for this journal was there.

VDT: Right. Meanwhile, the PAA membership nearly doubled, from 800 in 1963, the year before Demography first appeared, to 1,500 in 1968 [802 to 1,495]. That had much to do with Demography.

BOGUE: I think the fact that there was an official publication to which people could send refereed articles was part of that. But the field of population was exploding at that time anyway. So there was an interaction: exploding membership helped finance the journal and the journal helped make membership more attractive. Those were exciting years, because things were changing so quickly, and Demography was born just at the right time to be both a cause and an effect of that.

VDT: Andy Lunde was secretary-treasurer in the mid-1960s [1965-68] and because of the increasing membership, he found the business work much too much and that's when PAA contracted with Ed Bisgyer and the American Statistical Association to take on the business affairs.

In 1966, you began running news and announcements about the different population centers; obviously, you felt the need for a newsletter.

BOGUE: I did not start that, I don't think.

VDT: You have to have done so, because you were editor. I did not see those in any issue before that. There was no PAA newsletter.

BOGUE: Okay, then we did.

VDT: In 1966, you began announcements of what your center was doing, and others.

BOGUE: Ansley Coale had always done a nice job in Population Index of keeping PAA affairs announced, but just announcements about the field of population in general were lacking. Yes, that went into Demography.

VDT: When Demography changed, there was need for that newsletter. Abbott Ferriss followed Andy as secretary-treasurer and he started what became PAA Affairs [see Ellen Jamison, "The Story of PAA

Affairs," vignette of PAA history, PAA Affairs, Summer 1986]. You had advertisements in 1968, two or three, not many.

BOGUE: Yes, we tried, because our word with the Ford Foundation was that they wouldn't have to subsidize Demography more than two or three years. I was so confident that it would float that when I went to Bud Harkavy and asked for a diversion of funds, I said I thought it would be self-sufficient within a very short period of time. So we went out thumping the bushes for advertisements to help pay the printing bill.

VDT: Then, of course, there came the famous Volume 5, Number 2, special issue in 1968, "Progress and Problems of Fertility Control Around the World," which was a splendid summary of family planning programs and action around the world. But it aroused the ire of a lot of PAA members, perhaps because it had that Indian family planning inverted red triangle on the front and the slogan, "Two or three--that's enough."

BOGUE: I never knew quite what happened with that issue. I still don't understand what the furor was all about.

VDT: Ansley Coale [PAA president 1967-68] said in his interview for this series that he got a dozen phone calls in two weeks from people, PAA members, saying, "What is . . .

BOGUE: Don Bogue up to?

VDT: Yes.

BOGUE: Well, first of all, the world was undergoing a dramatic revolution in family planning activity. And because of my work in India and I had been to CELADE in South America and was working with the family planning movement, I felt that my Demography readers really needed to know what was going on. The PAA, I thought, needed to be much more international. Population Studies in London was getting all the international articles and I was getting all of the American demography stuff. I wanted Demography to become a competitor for Population Studies, as a good place to publish stuff internationally.

So--maybe I shouldn't have done it--but as editor, I invited leading people in each of the developing countries where these programs were going on to write a summary of what was going on in their country. And it was a special issue, updating the world family planning development. And to advertise the fact that it was a special issue, I just slapped on the cover the symbol of the Indian family planning movement and inside in my introductory preface, I said this is an illustration of what is happening in the world. And people looked at the cover and never read my prefatory statement. Tempers exploded.

I still am proud of that issue and if I were in the same position, I would do it again, because I think it did have the effect of waking PAA up to the fact that there was a family planning revolution under way. And probably their awareness was even deepened by the little red triangle that irritated them and the uproar that followed the issue.

It is true that I was officially censured by the Association at its annual business meeting--a status which I still hold, I guess; it's never been rescinded.

VDT: Well, it's remembered by some of the oldtimers. But the great thing was that Community and Family Study Center paid all the bills for that issue. That was another thing they were quite concerned

about--the voluminous size of those volumes. But, you paid all your bills.

BOGUE: Well, it is true that maybe NSF money--if that could be considered advocacy--NSF money did help pay those printing bills. [Calvin Schmid and Paul Glick, who succeeded Donald Bogue as PAA president, obtained a \$30,000 grant from the National Science Foundation to support Demography for three years after the first edition of 1964.]

But to me, as I mentioned before, I have never been able to feel that this was not part of professional demography. Maybe that's a blind spot that I have. But bringing out that issue was not a non-demographic act. In my view, then and still, it was a scientific act.

VDT: Then Beverly Duncan took over as editor of Demography and it shrank enormously. In recent years, there have been efforts to make it a little less narrowly focused. What do you think of it now?

BOGUE: Well, it never filled the role that I hoped it would play. The fact of the matter is that Studies in Family Planning was born because Demography had this new policy. If Demography had stayed along the track that I had set it in the first five years, many of the scientific articles that went into Studies in Family Planning would have been in Demography. No great damage was done; there was just another journal created. But I think that's to the detriment of Demography, because Demography would be a more powerful journal and perhaps the membership of PAA would be a thousand members larger today if that had happened.

I'm not going to quarrel with what Beverly and Dudley did. [Beverly Duncan alone was editor; Dudley Duncan stressed that in his interview of May 3, 1989.] Under their editorship, they did entice a great many scientific articles of very high quality and I think that they helped place high standards for statistical competence. Perhaps their system of refereeing was too severe, in my book; they would reject people who were having great fresh ideas but not much data. But I do think that they did help establish standards of rigorousness which helped the journal acquire a status as a scientific publication.

I think slowly there's a relaxation and the journal is drifting back in the direction that I originally had. The last issue has a very nice article on family planning effort--what do you call it?, you know, the Mauldin-Lapham index--a very nice statistical analysis comparing what's happening in the various countries. This was a very solid piece [Barbara Entwisle, "Measuring Components of Family Planning Program Effort," Demography, February 1989]. That's the kind of thing that we were having in the first five volumes, so I think that it's drifting back in that direction. And a lot of the work that's now being done on family structure--Larry Bumpass--that attitude stuff is creeping back in. So Demography will come back full circle, I think, by its 25th volume [1989 was Volume 26].

VDT: That feeds into my next question: What do you see as the future of demography as a discipline? Do you think demography is getting narrower or broader?

BOGUE: I have thought about this quite a lot--not in terms of the field of demography but in terms of where a population research center should go. I think demography is a basic discipline that serves all of the social sciences. You just can't mention a social science that doesn't somehow rely on demography: political science, education, economics, business, even religion. There's just no field that doesn't. This little group, American Demographics, is a demonstration of that.

VDT: You mean a demonstration of demography in business?

BOGUE: Yes. Demography just cannot escape that role of being a basic science that a lot of people use, although demography isn't their central way of life. I think there's a tendency in demography to

treat those people as a nuisance or as some kind of parasites or fellow travelers that we should dismiss.

I think that's a mistake. I think that these people really need demography; that demography should make an honest effort to service their needs the best way it can. And it isn't necessarily non-scientific; sometimes to satisfy these special needs, you have to use very elaborate demographic methodology and even discover new things. So the problem of trying to satisfy these ancillary needs keeps stimulating the field and bringing new ideas into it. So I hope that demography tries to extend its base farther and farther, the basic services, rather than to try to restrict it more and more narrowly to a small group of people who are highly skilled mathematically and highly sophisticated in model-building, with comparatively little concern for this broad need.

I think this is an issue that will not be resolved in the near future. But I think that the ultimate--well, there will still be this hard core of methodologists, but I'm hoping that the PAA pays more and more attention to this general service need. If they want to have a huge membership, that's the way to go.

VDT: Well, I'm not sure they do, but certainly the business demographers are very much in evidence now. You've answered my next question: Are demographers needed more than ever?

BOGUE: Yes, people with demographic training. It is getting now to the point where if you are an MBA from a reputable business school, you have to have had demography. If you are a reputable political scientist wanting to serve in the foreign service, you have to have demography, and the same in educational planning. So there is a whole field of teaching demography for practical use. I think that there is a group working very effectively at this and I think that their agitation will keep the Population Association of America oriented toward meeting this very large demand.

Finally, I do wish that the PAA would become more international. It is still provincial.

VDT: You mean in people's research interests or more sessions on international demography?

BOGUE: Both. There are people working internationally and they are participating in our sessions. But it still doesn't have the flavor of Population Studies. When you read that, you get the feeling that the whole world is communicating there. You pick up Demography and you still . . .

VDT: You're saying that Demography, the journal, should be more international?

BOGUE: Yes. And that is reflected in our program arrangements too. I think that the international demographers still are not getting enough attention in our national meetings; what is going on internationally just does not get enough attention. For example, this special session on China tonight has to take place from eight to ten.

VDT: There's also one on Saturday morning, chaired by Sidney Goldstein, but you're right. However, there are certainly a lot of foreign students in evidence here, those who are being trained in American universities.

Is U.S. training still needed for Third World demographers? Well, you're supplying the software for them to use in their computers.

BOGUE: You can get good demographic training in Bombay, the demographic training center in India. I think in Latin America, you can get reasonably good training at El Colegio in Mexico, and CELADE in Santiago now offers a master's degree and also Ph.D., I think.

But if you really want to know the latest and the best state of the art, the cutting edge, you still

have to come to an American university or go to Europe. I think that the days of mass training of M.A. students to go overseas to get basic training in demography have passed, except perhaps for sub-Saharan Africans. But there is this new era in which people who are going to be top demographers--or even for postdoctoral training, to come into residence at one of these centers for a year of methodological update--there is still urgent need for that. I think that that is a role which American centers should play and play conscientiously for the next several years.

VDT: My last question is: Are you ever going to write your autobiography? You've written so much . . .

BOGUE: No, I'll never bother writing an autobiography. I hope to revise Principles of Demography in the next few years, if I have time. That was a thousand pages the first time and it can't be any smaller the next time.

APPENDIX: Donald Bogue supplied the following lists, July 31, 1990.

Partial List of Ph.D. Students Who Studied and/or Worked with Donald Bogue

Forni, Floreal	Argentina (U. of Buenos Aires)
Khan, Md. Aminur Rohman	Bangladesh (ESCAP)
Mertens, Walter	Belgium (Harvard University)
Vandeportaele, Dan	Belgium (U.S. Census Bureau)
Heredia, Rodolfo Antonio	Colombia (Regional Population Center)
Londono, Juan B.	Colombia (Columbia University)
El-Kamel, Farag	Egypt
Elkhamialy, Hekmat A.	Egypt (Roosevelt University)
Heiskanen, Veronica	Finland (U. of Helsinki)
Kwakye, Sylvester	Ghana (Ministry of Information)
Ho, Adalia	Hong Kong (U. of Maryland/Baltimore)
Bagchi, Sourendra Nath	India
Kurup, R.S.	India (Ministry of Health)
Misra, Bhaskar D.	India (Indian Inst. of Technology)
Misra, Jaya Krishna	India
Premi, Mahendra Kumar	India (Nehru University)
Martokoesoemo, Budisoeradji	Indonesia (U. of Indonesia)
Sujono, Harjono	Indonesia (BKKBN)
Saraie, Hassan	Iran
Ahn, Kye-Choon	Korea (Yonsei University)
Cho, Lee-Jay	Korea (East-West Center)
Brambila, Carlos	Mexico (El Colegio)
Vernon, Ricardo	Mexico (Population Council/Lima)
Uche, Chukwudum	Nigeria (U. of Benin)
Okorafor, Apia Ekpe	Nigeria

Aghai, Mohammed	Pakistan
Hashmi, Sultan	Pakistan (UNDP/Sudan)
Nizamuddin, Md.	Pakistan (UNDP/Addis Ababa)
Rafiq, Muhammed	Pakistan
Barcelona, Delia	Philippines (U. of Philippines)
Bulatao, Rodolfo A.	Philippines (World Bank)
Pascual, Elvira Mendoza	Philippines
Thavarajah, Arumugam	Sri Lanka (United Nations)
Boonlue, Tania	Thailand
Navawongs, Tippan	Thailand
Prachuabmoh, Visid	Thailand (Chulalongkorn U.)
Allen, Walter	U.S. (U. of Michigan)
Bertrand, Jane Trowbridge	U.S. (Tulane)
Bursik, Robert	U.S. (U. of Oklahoma)
Dizard, Jan	U.S. (U. of Massachusetts)
Elkins, Henry	U.S. (MSH/Boston)
Farley, Reynolds	U.S. (U. of Michigan)
Hannenburg, Robert	U.S. (ESCAP/Thailand)
Hartmann, David	U.S. (Southwest Missouri)
Hendershot, Gerry	U.S. (NCHS)
Hinze, Kenneth	U.S. (Louisiana State U.)
Jaret, Charles	U.S. (U. of Georgia)
Keller, Alan B.	U.S.
Laing, John E.	U.S. (East-West Center)
Mayo, Judith	U.S. (U. of Arizona)
Monsees, David	U.S.
Moore, Maurice	U.S. (formerly U.S. Census Bureau)
Mulder, Ronald	U.S. (Albion College)
Murphy, Edmund	U.S. (Statistics Canada)
Nelson, James	U.S. (SUNY/Albany)
Palmore, James	U.S. (East-West Center)
Peterson, James	U.S. (U. of Pennsylvania)
Sinquefield, Jeanne Cairns	U.S. (formerly Ford Foundation)
Straits, Bruce	U.S. (U. of California)
Surgeon, George	U.S. (City of Chicago)
Teachman, Jay	U.S. (U. of Maryland)
Tsui, Amy Ong	U.S. (U. of North Carolina)
Way, Ann Adams	U.S. (Westinghouse)
Way, Peter Orville	U.S. (U.S. Census Bureau)
White, Michael	U.S. (Brown University)

Woolbright, Albert	U.S. (NCHS)
Buu-Tap, Nguyenphuc	Vietnam

Partial List of M.A. Students Who Studied and/or Worked with Donald Bogue

Andkhoie, Mohammed Akbar	Afghanistan
Forni, Floreal	Argentina
Ahmed, Ashraf Uddin	Bangladesh
Hoque, Mozammel	Bangladesh
Cosneros, Antonio	Bolivia
Machado, Alayde Gouveia	Brazil
Nouthe, Francois	Cameroon
Orrego, Felipe	Chile
Heredia, Rodolfo Antonio	Colombia
Londono, Juan B.	Colombia
Maldonado-Gomez, Inez	Colombia
Mirkow-Ospina, Italo	Colombia
Ordonez, Myriam	Colombia
Prada, Elena	Colombia
Vallllenzuela, Margarita	Dominican Republic
Elkhamialy, Hekmat A.	Egypt
El-Wafaey, Md. Amin	Egypt
Feteha, Mohamed Md. Md.	Egypt
Mohamed, Salwa Emam Aly	Egypt
Yimer, Erku	Ethiopia
Ampah-Kwofie, James	Ghana
Brefo-Boateng, Joe	Ghana
Jumfuoh, Akwasi Dabankah	Ghana
Kwakye, Sylvester	Ghana
Obeng, Mercy	Ghana
Amal, A.S. Bharathy	India
Bagchi, Sourenda Nath	India
Bebarta, Prafulla C.	India
Chaterjee, Pranab	India
Dang, Krishna Lall	India

Gupta, Prithuis Das	India
Kohli, Bal Ram	India
Kohli, Krishna Lal	India
Kumar, R.T. Sampath	India
Mishra, Uma Shanker	India
Misra, B.D.	India
Misra, Jaya Krishna	India
Neog, Prafulla	India
Premi, Mahendra Kumar	India
Rajan, Vanaja	India
Saksena, Devendra Narain	India
Shamsuddin, Mohammed	India
Sharma, Devindra Lal	India
Lesmana, Tjipto	Indonesia
Martokoesoemo, Budisoeradji	Indonesia
Suharto, Bar	Indonesia
Sujono, Harjono	Indonesia
Sungkono, Bambang	Indonesia
Adu-Bobie, Gemma	Kenya
Mbai, David	Kenya
Ahn, Kye Choon	Korea
Cho, Lee-Jay	Korea
Chung, Kyung-Kyoon	Korea
Chung, Sang-Yun	Korea
Han, Insook	Korea
Kim, So-Yong	Korea
Lee, See Baick	Korea
Moon, Hyun-Sang	Korea
Park, Heung-Soo	Korea
Roh, Mihye	Korea
Lim, Meow Khim	Malaysia
Macias, Hector	Mexico
Hamal, Hem B.	Nepal
Uche, Chukwudum	Nigeria
Ahmed, Ghyasuddin	Pakistan
Ahmad, Hafizuddin	Pakistan
Akbar, Mohammed Javed	Pakistan
Ali, Mohammad Akbar	Pakistan
Ali, Mohammad Nawab	Pakistan
Amin, Ruhul	Pakistan

Hakeem, S. Abdul	Pakistan
Haygt, Muhammad Feroze	Pakistan
Iqbal, Syed Javaid	Pakistan
Karim, Mehtab S.	Pakistan
Karim, Muhammad Azuzul	Pakistan
Malik, Bashir Ahmad	Pakistan
Nizamuddin	Pakistan
Nuruddin, Muhammad	Pakistan
Rafiq, M.	Pakistan
Shafiullah, A.B.D.	Pakistan
Sjaikh, Haq Nawaz	Pakistan
Sharih, Khalid	Pakistan
Tehseen, Zafar	Pakistan
Tenvir, Fayyaz Ahmad	Pakistan
Chang, Vielka	Panama
Alfaro-Alvarez	Peru
Bellosillo, Lina R.A.	Philippines
Soliven, Aida G.	Philippines
Bam, Brigalia N.B.	South Africa
Goonasekera, Sriyawansa Anura	Sri Lanka
Gunasekera, Anton	Sri Lanka
Perera, Stephen	Sri Lanka
Thavarajah, Arumugam	Sri Lanka
Hus, Ying-Yang	Taiwan
Lin, Lan Ching	Taiwan
Mgalula, Justin	Taiwan
Bhiromrut, Patama	Thailand
Boonlue, Tania	Thailand
Bunnag, Achara	Thailand
Chandavimol, Pisamai	Thailand
Keoprasom, Phaisal	Thailand
Ketudat, Pungsi	Thailand
Navawongs, Tippan	Thailand
Piampiti, Sauvaluck	Thailand
Pongjarean, Chalee	Thailand
Siripak, Wiwit	Thailand
Voraponsathorn, Thavatchai	Thailand
Wansorn, Sommai	Thailand
Abdi, Abdelwahab	Tunisia
Othmann, Kefi	Tunisia
Riza, Mohammed	Tunisia

Bicep, Joyce	Trinidad/Tobago
Bishop, Joan	Trinidad/Tobago
Blair, Annie	U.S.
Champion, Phyllis	U.S.
Channock, Foster	U.S.
Copp, Brian	U.S.
Crimmins, James	U.S.
Crimmins, Mary	U.S.
Frederick, Daniel	U.S.
Hudson, Stanley	U.S.
Jacynta, Rita	U.S.
Kronus, Sidney	U.S.
Milkereit, John	U.S.
Morse, Mary	U.S.
Porter, Jeff	U.S.
Redman, Cynthia	U.S.
Sachs, Nancy	U.S.
Speer, Mary	U.S.
Whitfield, Randall	U.S.
de Segarra, Isabel Colon	Venezuela
Garcia, Maria del Pilar	Venezuela
Buu-Tap, Nguyenphuc	Vietnam
Dac, Dinh Cong	Vietnam

RONALD FREEDMAN

PAA President in 1964-65 (No. 28). Interview with Jean van der Tak at the Population Studies Center, University of Michigan, Ann Arbor, June 12, 1989, with excerpts from Dr. Freedman's interview with Anders Lunde at the PAA meeting in Philadelphia, April 26, 1979.

Dr. Freedman requested (in a letter of April 25, 1990, to Jean van der Tak) that the following statement appear at the beginning of this text:

"On reading the transcribed interview, Ron Freedman decided that the 'stream of consciousness' flow should stand, complete with grammatical errors and incomplete and repetitious sentences. Reader, beware!" (Subsequently, however, this transcript, like all those in the series, has been lightly edited.)

CAREER HIGHLIGHTS: Ronald Freedman was born in Winnipeg, Canada, which makes him one of PAA's three Canadian-born presidents, along with Nathan Keyfitz (president in 1970-71) and Norman Ryder (1972-73). He came to the U.S. as a child and grew up in Waukegan, Illinois, a suburb of Chicago. He received his B.A. in history and economics from the University of Michigan in 1939 and the M.A. in sociology from Michigan in 1940. He then went to the University of Chicago and completed the prelims for his Ph.D. in sociology before joining the Navy in 1942. In 1946 he returned to Chicago to work on his dissertation and received the Ph.D. in 1947. He also joined the faculty of the department of sociology at the University of Michigan in 1946 and has been associated with Michigan ever since, variously, as Professor of Sociology, Research Associate with Michigan's Survey Research Center, and first Director, from 1962 to 1971, and then Associate Director of the Population Studies Center, which he founded. He has been adviser and consultant to many national and international institutions in the U.S. and Asia. He was Vice-President of IUSSP in 1965-69. Among his awards, he was elected to the National Academy of Sciences in 1974 and received PAA's Irene B. Taeuber Award for Excellence in Demographic Research in 1981.

Ronald Freedman is famous in the field of demography for his work and publications on fertility from the sociological perspective, beginning in the 1940s with analysis of the Indianapolis Fertility Study. He was co-director of the first U.S. national fertility survey, the Growth of American Families survey of 1955, and originator of Michigan's ongoing Detroit Area Study. He is best known, perhaps, for his work in the Third World, especially Taiwan. In the early 1960s, he set up the Taichung experiment which led to Taiwan's very successful family planning program and inspired other programs in Asia. He was co-director of Taiwan's Population Studies Center in 1962-64 and has been consultant to the Taiwan government ever since. He has been family planning research consultant to several other Asian governments, including, most recently, China. Throughout his career, he has produced a large number of influential books and articles on family planning programs, fertility surveys, and the interface between social science research and human fertility.

VDT: When did you first become interested in demography?

FREEDMAN [This combines Dr. Freedman's replies in 1979 and 1989]: That happened in 1939 quite by accident. I'd had one course in sociology as an undergraduate and, for various reasons, decided that I wanted to be a group social worker and that the way to do that was to get a graduate degree in sociology. So I entered the sociology department at the University of Michigan. Almost by chance, I registered for two courses, one in population, one in human ecology. I had expected these courses to

be taught by the late R.D. McKenzie, but when I got to class, I found that he was ill and a young teaching fellow named Amos Hawley was giving those courses. It took about two weeks before I became entranced with what was going on, and that changed my life. At the same time, I was taking my first course in quantitative research methods with Clark Tibbitts and the two things together opened my eyes. The idea that one could quantitatively study social problems was new to me. I was fascinated by that from the methodological point of view and then fascinated by the subject matter. By the end of that term, I knew that somehow I was going to work in this general field.

Previously, I had really been going into sociology with the view of being a director of one of the Hillel foundations that Jewish students have on many campuses. As a student at Michigan, I was the student director of the Hillel foundation. My salary of \$5 a week and my room there sustained me during most of my student years at the University of Michigan.

VDT: Then you decided to go to the University of Chicago, as the mecca of sociology at that time, after getting your master's in sociology at Michigan?

FREEDMAN: Right. I had received fellowship offers from Michigan and Chicago. Michigan's department was then small and my friends here felt that I had learned what they had to give and encouraged me to go to Chicago. I must say that at the end of the first semester there, I came back and said I wanted to come back to Michigan because I didn't feel I was learning enough and felt I knew more about human ecology than they did there, on the basis of what I'd learned here. Fortunately, at the end of the second semester things opened up. At that time, there were something like 300 graduate students at Chicago; about half the Ph.D.s in sociology in the United States were being produced there and it was an anomic situation; you were a wanderer in the wilderness.

VDT: Three hundred in the department of sociology alone?

FREEDMAN: Yes. Well, many of them were part-time, on the fringe. What turned it all around for me was that I became a member of a much smaller, inner group of people who communicated with each other, used the famous Phil Hauser notes for studies; we had private study sessions. I was research assistant first to William Ogburn and then to Louis Wirth. A high point of the year was that Phil Hauser, then working in the Census Bureau, came back to Chicago and gave us some lectures on the introduction of the new idea of sampling into the data collection of the federal government. I got caught up with fascinating, interesting people like Sam Stouffer, who was a marvelous teacher and inspiring figure, and I never thought of leaving after that.

LUNDE [from the 1979 interview]: Could you tell us something about Stouffer and W.F. Ogburn.

FREEDMAN: Stouffer was a very exciting, disorganized teacher. He would get a bright idea, rush into the room, rush to the blackboard, and before the period was over, his coat and trousers were full of chalk. It frequently had nothing to do with what we were studying at the time. He got all of us excited. Kingsley Davis, who happened to be a visiting postdoc, was in that class. We learned a great deal about population statistics and sampling, the two courses I took with Sam. His style of teaching was disorganized but inspirational.

W.F. Ogburn was a Southern gentleman of the old school and a very kindly man. He was a man with nobility of character, clarity of mind, but a very dull teacher. He spoke very slowly from 3 by 5 index cards. I took my first course in multivariate analysis with Ogburn. The course consisted of algebraic derivations of the various formulae for multivariate analysis. We had a classroom that looked out on the Medway and I remember vividly one day--this was before the day of xerox--Ogburn told us he would have the notes he had on cards reproduced and give them to us, but he put them on

the blackboard and this day he had filled about four blackboards. Then he stopped and looked pensively out the window, then looked back and said, "I guess that's a mistake," and he erased a few blackboards and started over again.

VDT: You completed your prelims before going into the Air Force in 1942 and came back to Chicago in 1946 to work on your dissertation on recent migration to Chicago. You found that Professors Ernest Burgess and Louis Wirth had obtained some 1940 census data for Chicago from the question, "Where did you live five years ago?", the first time that question was asked in the census, and you were going to use that to test the Burgess zonal hypothesis.

FREEDMAN: That's right. That was an interesting story, because it turned out that the simple version of the Burgess hypothesis didn't work. That would have been that mobility and social disorganization decreased together with distance from the center of the city. I told my wife this is going to be a textbook thesis. I had a hypothesis; it was obviously going to be right. We should be able to organize the data in three months, I'd write it up, and we'd go home. Well, it took six or seven months of prodigious work. We had to combine the 900 and some census tabs into the 75 areas into which Chicago was divided on about 18 characteristics by age. This was before the day of the computer. Deborah and I did this on nine-key adding machines, working days and days and days. It turned out that the Burgess hypothesis didn't work.

When I found that out I was deeply distressed, because I had assumed it was going to work because much of the Chicago school of work on social disorganization was based on the idea that mobility produced social disorganization and since high rates of social disorganization decreased with distance from the center of the city that ought to be true about migration as an index of mobility as well. What I subsequently decided, changed the whole thrust of the dissertation. That was that one had to take into account the social framework of the migration and that migration in itself would not be disorganizing if one moved between similar social environments. For example, I found that migrants who came from farm areas into the city were concentrated in the center of the city and that was a source of disorganization, along with immigrants from other places who were also moving between dissimilar environments. On the other hand, far out in Hyde Park, for example, that was an upper middle class professional area, with a lot of migrants and a lot of mobility, but those moving there from out of Chicago were people who moved with sophistication from one area to another. So I recast what I had and it came out very well.

When things first didn't work out, I went to see Professor Burgess. He was not on my committee, which consisted of W.F. Ogburn and Louis Wirth. I said, "Professor Burgess, I've been working for six months with the data that you know about, testing the Burgess zonal hypothesis, and the data don't seem to fit the hypothesis." To which he replied, "Young man, something is wrong with the data."

VDT: I think your resolution of that lived up beautifully to a remark you made in your 1988 PAA paper on the Michigan model of graduate training ["Graduate School Training of Demographers: The Michigan Model"]. You said that in the Michigan apprenticeship system, "We believed that learning to do research should involve learning to meet the unexpected," and as an example of that, you mentioned "relationships contrary to the initial hypothesis." So you recast your original hypothesis and the data then did fit.

FREEDMAN: Yes. By the way, my dissertation was published. Philip Hauser, who was at the Census Bureau most of the time I was at Chicago and came back occasionally, came back in 1947 when I came to Michigan to set up what became the Population Research and Training Center. He

began a publication series in demography and population at the University of Chicago Press and I was fortunate enough to have my dissertation selected as the first monograph, Recent Migration to Chicago [1950], which was the first book publication I ever had. It was a nice thing for an instructor--in those days, one was instructor for four years before becoming assistant professor--to have in his second year at the university; gave me a boost there.

VDT: You described in your 1979 interview how you switched from migration to fertility, specifically from migration to the analysis of the Indianapolis Fertility Study.

FREEDMAN [combines answers in 1979 and 1989]: I had always had some interest in fertility but had never done any research on it. My first five or six publications were all in migration, most of them jointly with Amos Hawley. I gave a migration paper, probably in 1948, at the American Statistical Association in a session arranged by people from PAA, and Clyde Kiser and Pat Whelpton were in the audience. They were the central figures in the Indianapolis Fertility Study. They invited me to have dinner with them and propositioned me: "We've got this marvelous body of data, collected just before the war. We haven't gotten it to the analysis. It's too big for any small group. We'd like to know if you're interested in that." I'd heard about the study and said, "I'm sure I'll be interested in looking at it, send me some of the materials and I'll get back to you." They did that almost immediately.

Those were the days before the big grants and before people had to be paid for doing research. I was teaching four courses a term at the time and we talked over the logistics of it. All this was on punched cards at the Milbank Memorial Fund [where Clyde Kiser was, in New York City] and they promised that if I would send tabulation requests to Clyde Kiser, he had a 101 tabulator and would produce tables for me. I think they also gave me about \$500 for clerical help. I had learned at an early age to be a bargainer, so I said I'd be glad to do it, but I'd be much more interested and we could do more work at Michigan if they would find fellowships for graduate students who would also work on it. So in addition to the work I did, which involved three pieces jointly authored with Pat Whelpton, two graduate students got dissertations out of it. The person who is still known in the field was my first Ph.D. dissertation student, Jack Kantner, who has recently retired from Johns Hopkins.

VDT: I interviewed Jack a year ago in Bedford, Pennsylvania, and he raved about what a wonderful experience it was. He mentioned, for instance, that once he and some other graduate students drove down with you to see Whelpton at the Scripps Foundation in Oxford, Ohio. It was a wild ride in a car that you'd borrowed from your brother-in-law. Your mind was on the fertility survey and not on the road, but you got there all right.

The Indianapolis Fertility Study set the pace for the series of national fertility surveys that followed. But one thing that is always pointed to in it is that it was looking at the social and psychological variables in fertility but there was little yield from the psychological variables. Nor was there in the longitudinal Princeton Fertility Study which followed it. I think you too felt at the time that there wasn't much yield from psychological variables. Do you still think psychological variables can't tell you much?

FREEDMAN: I would say that in the sense that we worked on them then that not much came of it. In a much broader social-psychological sense, I think there still may be a great deal in it. For example, the ideational hypothesis of fertility change has re-emerged in a kind of classic recent article in Population Studies by Cleland and Wilson and work by Ron Lesthaeghe that emphasizes the role of ideas in this field. That's something that's gone up and down. Demographers in general have been rather skeptical about these things. I was trained as a human ecologist to be skeptical about it; ecology does not in general involve those concepts. But I've always felt that one should keep an open mind on

matters. I think that's an important hypothesis now. I think of what emerged from the Princeton European Fertility Study, looking back at the demographic transition in Europe; ideational elements were very important then. And that's especially important in this age of instantaneous communication of information around the world.

I've never felt that individual psychological variables yielded very much and I think they still don't. The longitudinal Princeton Fertility Study [of women in the U.S.'s eight largest metropolitan areas who had their second child in September 1956] was an offshoot of the Indianapolis study. Some people were involved in both. Charlie Westoff [first director of the Princeton study] was a research associate on the Indianapolis study [at the Milbank Fund]. Two psychologists who were on the Indianapolis study board, Dan Katz, who is still here at Michigan, an emeritus, and Lowell Kelly, who was also a Michigan person, and Donald Marquis, who built up Michigan's very important psychology department after World War II, were all members of a group that met prior to the Princeton study to talk about what was going to be done. One of the basic thrusts of the Princeton study design, which involved Bob Potter, Philip Sagi, Charlie Westoff, and a psychologist named Elliot Mishler, who was also a Michigan person at that time, was the exploration of a whole series of psychological variables. I think the record shows that those didn't work out very well either.

VDT: In the 1979 interview, you also told how P.K. Whelpton got you involved, along with Michigan's Survey Research Center, in the 1955 Growth of American Families study. You also described that very well in the videotaped interview you had in Barbara Wilson's series on national fertility surveys [described by Barbara Wilson in "Videotaped Interviews about American Fertility Surveys," *Vignettes of PAA/U.S. Fertility History*, *PAA Affairs*, Winter 1985].

FREEDMAN [from the 1979 interview]: After the war, Pat Whelpton led in putting out population forecasts for the Census Bureau and he had the unfortunate experience that comes to demographers: those forecasts couldn't be printed fast enough to be right. By the time they got to the Government Printing Office, they were wrong. Pat was trying to use cohort fertility approaches and looking at historical data to make projections. Around 1954 he came to see me in Ann Arbor; by that time, I had become involved with the Survey Research Center at Michigan. He said it might be worthwhile if we could produce a survey in which we would get women's fertility history and ask them how many additional children they expected to have. I got enthusiastic. We got a grant from the Rockefeller Foundation and that began the series of national surveys. We did it in 1955 and Pat and Art Campbell did it in 1960. In 1965 and 1970, it moved to Princeton and then--the goal which we had worked toward for a long time--it was taken over by the federal government in the National Center for Health Statistics [National Survey of Family Growth, beginning in 1973].

I worked very closely with Pat Whelpton. He was a good man to work with. We had a lot of arguments. Pat was a stubborn fellow; he got an idea and he wanted to stick to it. I'm not entirely flexible either. We argued a lot, but we were never angry; we convinced each other of one thing or another.

VDT: Are you pleased that the GAF led to the series of U.S. national fertility surveys and the World Fertility Survey and its successors?

FREEDMAN: Yes. I thought it was very important at the time, and I think in retrospect it's very important.

I might say something here that is relevant to my own evaluation of my career. I reflected on it some over the weekend in preparation for meeting with you. I think that overall whatever I may have contributed to the field is not so much in the substantive research I have done; most of that will die,

much of it is dead now, some of it that's still there will die--the field changes. I think that what I have contributed lies in two areas. One is that I have been fortunate enough to be in a position to take some initiatives which I think had a lasting impact in that they influenced what other people were doing; they opened a whole area of work. The Growth of American Families study was certainly one of those areas; perhaps, in many respects, the most important. The other area is through my students. I think that's more important, probably, than the first thing. That is, all of these other things pass away. People forget about them, and they will forget about me. What effect I'll have in the future, I think, will be through my students and their students. In my last two years of teaching, I had the privilege of having in my class three students whose fathers or mothers were students in that course 25 years earlier.

VDT: Wonderful! I was going to get onto your students, because that is indeed a tremendous contribution, going on through your career.

On the GAF, out of that you wrote the splendid book, with Whelpton and Art Campbell, Family Planning, Sterility and Population Growth [1959]. The 1955 survey took place almost at the peak of the baby boom [1957] and, of course, your projections for U.S. population were off, though not as much as some of Whelpton's earlier projections. Your medium projection for the U.S. population in 1990 was 273 million and in 1989, this year, the estimate is actually about 249 million. You were off about 24 million; that isn't so bad. Why did you not continue with the Growth of American Families study?

FREEDMAN: I got involved in working in the Third World. I had arranged for the second study to be done here at Michigan and then the question was what could be done later. Charlie Westoff and Norm Ryder were interested in taking up the task and I was glad to have them do it. In 1960 the Population Council subverted me by sending me to the Third World for the first time. They sent me to India for two months and on the way back I stopped in Thailand, Hong Kong, Japan, and I became interested in what was going on there. The Population Council then asked me if I wanted to be involved in the Taiwan work. And at the same time, I founded the Population Studies Center.

So the question was how to handle all of this. I felt that the GAF was well launched; there were good people who were going to take it up, and I went on to other things.

VDT: Before we get into the Population Studies Center and Taiwan, could we finish up on your domestic side? About 1954 you included fertility questions in the Detroit Area Study. Did you start the Detroit Area Study?

FREEDMAN: Yes, I started that with the inspiration of the late Angus Campbell, who was director of the Survey Research Center of the Institute of Social Science--a remarkable man. He really introduced me to survey research, which is relevant to my whole career. GAF would not have happened without this. Angus and I had become friends and he knew that I had worked on the Indianapolis study but had never done a survey and didn't know a lot about survey research. He gave me a faculty fellowship for a year, gave me half-time to wander around the Survey Research Center and look at the data sets there and I found I'd published a few things based on those data. I learned a lot from Leslie Kish, who was head of the sampling section, and Charlie Cannell, who was head of the field section. These people are still at Ann Arbor, still friends of mine. And I became interested in survey work.

Then Angus and I had the idea--I think he really was the originator--of starting what we then called a laboratory for sociologists and other social scientists, political science and economics, by doing an annual survey in Detroit, using good methods, doing a real project in the sense that we always had a faculty investigator who came in with a problem and the students would be involved for a semester working on the substantive problem. They would simultaneously get didactic training on

interviewing, sampling, what have you, and participate in every phase of the work, from helping to draw up the questionnaire; they all had to interview and code. And that lasted.

As director in the first few years, I took the privilege of including some questions on desired and expected family size. Pat Whelpton knew that I had done that and knew that I had spent time at the Survey Research Center. So that was the background for my getting into survey work.

I've used the more standard, traditional demographic data sources: census and vital statistics data. At that time, very few demographers used surveys; the Indianapolis survey was a unique thing. But most of my career has been involved with the application of survey methodology to demographic and related social science problems. And it came about in that way. It was a fortunate thing for me that I was at the University of Michigan which had, and has, what I think is the world's leading survey research center and that I became a friend and colleague of people like Angus Campbell and Leslie Kish. Our careers have intertwined ever since. When we come to China, I can tell you about involving Leslie Kish in survey sampling in China.

VDT: Did you have the idea of the longitudinal study that began with the Detroit Area Study of 1962?

FREEDMAN: That was the joint brainchild of David Goldberg and myself. We were the principal investigators for the Detroit Area Study that year. We thought it would be a good idea to make this longitudinal and we added something that at that time people didn't do. It was a survey on family life and fertility and at the end of the interview we told the people we were likely to want to come back and talk to them again, because we were interested in how their families were going to fare. We asked them for the names and addresses and telephone numbers of three friends or relatives who would know where they were if they moved. We've used that and asked those questions again ever since.

David went on to other things and I brought into the Population Studies Center Lolagene Coombs--she was W.F. Ogburn's assistant at the University of Chicago as Lolagene Convis--and she worked with me on that longitudinal study. One thing we did was we began using the telephone for the re-interviews. That's very common now; it was not common at all at that time.

After a number of years and quite a few publications in which Lolagene was involved with me, we put that to bed when I got heavily involved in Taiwan. Then some years later, my good wife Deborah and my former student and colleague, Arland Thornton, picked it up and it's still going strong and major analysis on that is still going out.

VDT: I want to take that up with Deborah [in the following interview, later the same day].

At the same time, you started the Population Studies Center and you said the Population Council got you involved in Taiwan. Jack and Pat Caldwell in their book on Limiting Population Growth and the Ford Foundation Contribution [1986] described how Michigan's center began. You went to the Population Council with a rather modest funding proposal and Dudley Kirk and Parker Mauldin took you to Oscar Harkavy at Ford. In 1961 Ford gave the center more than you asked for, while Penn, who were applying for their center at the same time, got less than they asked for. How did all this come together? It was your idea to set up the Population Studies Center and you said your interest in Asia was aroused by your Population Council trip to India. How did it all fit together?

FREEDMAN: I think one reason the Pop Council and Bud Harkavy were interested in Michigan was that I had already indicated my interest in the Third World and had begun negotiations to work in Taiwan. And that was a major objective of the Ford Foundation; they were interested in Third World fertility and family planning programs. My work on the Indianapolis study and GAF were preparations for applying these methods overseas. I had indicated that I thought they should be, and was beginning to work in those directions.

Now, when I went to Ford for that first interview with Bud, I didn't have the concept of the Population Studies Center. I just wanted to expand a very small internal program we had. I didn't know that anybody was going to provide money for overseas activity, although we had begun to talk about Taiwan on a small scale. I was quick to jump, however, when Bud outlined these larger opportunities; he wanted me to create an all-university institute, to found a big empire, involving public health, psychology, anthropology, and everything else.

I talked to the people back here and went back and talked to Bud. I said that I didn't want to be an empire-builder and I didn't want to be an administrator; I wanted to remain a scholar. In any case, I thought that we could do better work if we had a reasonable size group which had their base in specific social science activity and we would do it in sociology and economics. We didn't have an economic base at that time; I said we would develop that. But I also told him I would be happy to help foster other things. What I did was to work with Myron Wegman, who was dean of the School of Public Health, and Moye Freymann, who was out in India, North Carolina later, was brought here for a time and I helped found the Population Planning Center, but as an independent enterprise.

VDT: Michgian's population program, set up in 1965, was to have three branches?

FREEDMAN: Right. We had at that time also an activity in the medical school which died out, but population planning is thriving. It is a full department of the School of Public Health and we have many interrelations with them.

VDT: Although there were strains for a time, weren't there?

FREEDMAN: Yes, the second dean of the School of Public Health abolished the population planning center. The current dean, June Osborn--great authority on AIDS--revived it. She had decided either to kill it as a program or make a department of it and she made it a department, the Department of Population Planning in the School of Public Health.

VDT: About Taiwan, did you conceive of the Taichung experiment? The Caldwells call that and your later work with the Taiwan family planning program a landmark in the demographic study of developing countries.

FREEDMAN: The background of this is that the Population Council told me they had been visiting people in Taiwan in the rural health division of what was called the Joint Commission of Rural Reconstruction. That was an organization that did a tremendous job and laid the basis for the current development of Taiwan, beginning with rural land reform. The rural health division was working with the provincial department of health. At that time, Taiwan was one province and then and still is only one province of the Republic of China, which claims sovereignty over what they consider all of China. Taiwan now has Taipei and Taichung as separate entities.

The rural health people had some contact with the provincial health people who had begun some family planning activity through the maternal and child health division and they were interested in some statistical and social science background. The Council asked if I would be interested in working on that. I was just starting the center and told them I didn't want to get involved unless I had somebody who was competent and would stay there for three years. I would spend as much time there as possible. Deborah will tell you that I spent a lot of time. I took it out of my hide, if I may say so. I spent all my vacations there, would try to have my courses collapsed into shorter periods and so forth.

But I did not go for an exploratory visit until Yuzuru Takeshita became available. He was then known as John Takeshita. He spoke Japanese fluently. He was born in the U.S. but spent his grade

school years in Japan in his family's home village, came back to the U.S. a month before Pearl Harbor and was incarcerated in one of the Japanese camps. He had finished his dissertation at Michigan, which was based on a survey in Osaka, Japan, similar to the Detroit Area Study, and was willing to go to Taiwan. When I found that he was available, I told the Council, "Now I'm ready to go. I'll go and we'll see if we and the Taiwanese like each other; if we can work together." I went with the late Marshall Balfour, who knew East Asia and the Taiwanese; he'd been in Asia in 1948 with Frank Notestein and Irene Taeuber. We went and I fell in love with the Chinese. They were my kind of people. They were clearly pragmatic and empirical in orientation.

The people in Taichung were medical people and they were doing surveys in a very rough kind of way. When I told them how we did surveys and solved problems, I could see their eyes light up with interest. I also found that unlike many observers, including some professors in Taiwan I talked to, they weren't afraid of the problems. I had talked to a prominent professor at the National Taiwan University who had some demographic training and he said, "A family planning program will never work here and you can't interview people about how many children they want because people who've had more children than they want would never say so because that would imply that they wanted those children dead and the spirits of the dead would come after them. You couldn't do it." The public health medical people had a completely different idea. They said, "We're talking to people all the time about pregnancy and birth and the beginnings of the use of contraception and we find they're very interested in those things." These were the people in maternal and child health in the Taiwan provincial institute. The director, J.Y. Pong, now retired, was my counterpart. We agreed that we were going to do some work together.

Before I went to Taiwan, I had been in interaction with the late Bernard Berelson at the Population Council, who was a major influence on my career and life; close friend, close collaborator. Barney had the idea that we ought to do a large social experiment someplace, applying the best we knew from social science methodology, to test whether people in Third World countries would talk to you about these things, whether if you offered contraception to them, it would work. When I came back from that first trip, I told Barney, "I think I have the place. I think this is manageable, they're ready, they'll cooperate with us."

We had agreed to do a pretest and we went back and interviewed about 300 women who were below 30 years of age and had at least two children and at least one son. We interviewed those 300 women and if they weren't using contraception--most of them weren't--we asked if they'd like somebody to visit them, and we pretested whether they would accept. J.Y. Pong did that: "We Chinese did this," you understand. By that time, Yuzuru Takeshita was there with his family.

And the experiment worked! We had something like a 98 percent response rate. Something like 25 percent of the women who said they wanted contraception began right away.

I called Barney from Taiwan and said, "This is the place," and Barney flew out there and we sat with our Chinese friends for three or four days and sketched out the design for the Taichung study. He left and we went to work on that. That was the beginning of the Taichung study.

We got heavily involved in that, of course. We published a good many things before the book, Family Planning in Taiwan: An Experiment in Social Change, which didn't come out until 1969.

Everything we did was oriented to the idea that the research was not to interfere with the public policy action that was going on. So we would first analyze what was needed to get quick feedback for the people who were running the program. For example, in the Taichung study--at that time we were using IBM cards--and we had a program card first on which we put the background things that we thought were of greatest importance--social and demographic things but also the program things with reference to what people wanted. We coded and ran those things first. That was always the pattern of our work in Taiwan, because we felt we had to persuade them that the scientific work we were going to be doing was going to be useful to them as well as relevant for our publications.

VDT: Wonderful way to work together. The Caldwells point out that the Taiwan program, started with the Taichung experiment, was an inspiration for other Asian countries--particularly South Korea, wasn't it?

FREEDMAN: Yuzuru and I consulted there. They asked us to come after we got the Taichung study under way. And Marshall Balfour asked us to go there again. He went with me and introduced me.

VDT: Did you have a similar experiment there?

FREEDMAN: No, there we were purely consultants. We brought a couple of Koreans here for training. Dr. Sook Bang, who was an important figure in those early days, got his Ph.D. with Population Planning here. He was an M.D., so that was appropriate, but actually he sat in our center when he got his dissertation done.

VDT: So there was this contact with you Michigan people and then they spread through Southeast Asia?

FREEDMAN: An example was John Ross. He's been an important figure over the years in fertility and family planning research. He was teaching sociology at a Michigan college when he heard a talk I gave on the Taichung experiment at a meeting of the Michigan sociological society and he came and asked what he'd have to do to get to be a demographer. We got a postgraduate fellowship for him at Michigan. He subsequently worked in Korea and did a good many other things for which I don't claim any credit--John's a very bright guy. But that's some idea of the linkage.

VDT: You've written about the networking that went on among your colleagues and former students, especially in Asia with the Organization of Demographic Associates in the 1970s.

You have explained that you yourself didn't stay too long in the developing countries where you worked and the Caldwells said that faculty members set up the projects that were carried out by your collaborators, in this case, Takeshita in Taichung.

FREEDMAN: Takeshita was a staff member here. He was there for three years, but the work all the time was done by the Chinese. There were very few Americans involved.

VDT: Who actually wrote the book and the articles, you or the collaborators? Of course, all your names were on them.

FREEDMAN: The Chinese wrote some of the articles, but basically we did most of the writing, but not me personally. Many people were involved in that book. Yuzuru was a primary coauthor, but we had James Palmore, Bob Potter, and L.P. Chow, director of the Taiwan work for a number of years and now a professor at Johns Hopkins, was active in this. The Chinese weren't prepared for scientific social science writing at that time. Over the years, they gradually became adept and we became consultants. They have very competent people like Dr. T.H. Sun, one of my Ph.D. students who was director for many years, and the current director, Dr. Ming-Cheng Chang, at the Taiwan Provincial Institute for Family Planning, which is the successor to the Taiwan Population Studies Center. They've got other PhDs and other people we've trained. We helped a lot in the early years, maybe five years, but these Chinese became increasingly independent in doing the work. I've been there as a consultant at least once a year every year since then, except the year I had my heart attack and the year I had heart

surgery. I was there last year and I'll be going again this fall. Currently, we're writing two books about the whole Taiwan experience jointly, a group at Michigan under Al Hermalin and Arland Thornton--not my leadership--on this side and Dr. Sun and Dr. Chang on that side.

VDT: Why two books?

FREEDMAN: We're writing one on the changing Taiwan family and another on the demographic transition in Taiwan. There's so much on the family and Arland is interested in that, so we're writing two books.

VDT: Is the family book the one Deborah is involved in?

FREEDMAN: Yes, she's involved in that also. While we were writing the first book, I brought Al Hermalin in from Princeton. A little story on that. Larry Bumpass did his doctoral dissertation here and had been my research assistant. A very distinguished person now in the field; I just got through reading his latest article. Charlie Westoff hired Larry to work with him on the continuation of the GAF study and I called Charlie and said, "Charlie, we must talk baseball here, you got my prize first baseman. You've got to give me somebody in return. I need somebody." I had offered Larry a job but the opportunity came at Princeton and I said, "You should go because there's a different tradition there; it's a great place." I didn't try to keep him; didn't sway him at all. Then Al came and he had a hand in the finishing of that book, but also became a leader and gradually became one of the important figures in the continuing work on Taiwan. Then he branched out, of course, in many other things. He's very distinguished and won PAA's [first] Robert Lapham Award [for "distinguished contributions to the application of demographic knowledge"] last year [1988].

BREAK HERE

VDT: We've just taken a break and I've been introduced to the famous coffee break that takes place every morning and afternoon at Michigan's Population Center, with interesting visitors as well as all the staff and students gathered here.

One question I haven't asked is did you ever learn Chinese?

FREEDMAN: No. I thought about it. I talked about it with my friend Alex Eckstein, a specialist on Chinese economics, who spent 18 months in Taiwan trying to perfect his Chinese. I was on a five-ring circus and the question was should I take a couple of years to learn Chinese or should I proceed with the Taiwan work and the Michigan center and everything else. I decided that I might very well stop and not learn Chinese very well anyway. It looked as if I'd be involved in other countries; I went to Korea, India, and people wanted me to go to Indonesia. I decided that, yes, it would be much better to know Chinese, but that just wasn't going to work; there was too much to do.

When I went to India in 1960 on that first trip for the Population Council, I had begun to practice interviewing people with the aid of an interpreter and I think I got to be pretty good at it. It's not the same as interviewing yourself but you can do a pretty good job if you use some tricks that you have to know. For example, always look at the person you're talking to, never at the interpreter. The interpreter is in the background interpreting; a good interpreter will be as inconspicuous as possible.

But the fact that I can't read Chinese still grates me. I've been fortunate enough most of the time in having good people to translate for me. But it was a question of what to do with my time. I regret that, but there it is.

VDT: The Caldwells say the Michigan program had more measurable impact on the Third World situation than any other U.S. university population program. We've talked about the connections with Korea. You sent some people to, or worked in, Hong Kong, Malaysia, Thailand?

FREEDMAN: Yes. Let me say first that the program in Taiwan had a great deal of influence because, in the first place, it was a pioneer and, in the second place, it was well documented and we had the Taichung survey instruments, which were modeled and adapted from things we had been using in the Detroit Area Study and GAF. We had more influence, I think, than Korea did for two reasons. One was that we published things right away and we made all the instruments available. Secondly, at an early point our friends in Taiwan were very entrepreneurial and set up an international training center to which people came and were briefed in what was going on. An example is Dr. Harjono Sujono, who has been head of the Indonesian family planning program for many years. After he got his Ph.D. at Chicago, he stopped on the way home in Taiwan and spent some time looking at what was going on there. Then the Indonesians went on to do things their own way, which is the reason the Indonesian program is a success. Hundreds of people passed through that training center. It was a whole apparatus which had Population Council and other funding support for a time, in addition to what was going on in the program. So that had quite a lot of influence.

Plus the fact that I was on the hustings talking about Taiwan in various places. Yuzuru and I had a paper at the famous Geneva meeting in 1965, which was the first really big international meeting on fertility and family planning. Out of it came a big book edited by Berelson et al. [Family Planning and Population Programs: A Review of World Developments, 1965]. I think the influence fanned out from that.

Also, of course, while this was going on we trained a lot of students here.

Now, all of this began to have an impact in other places. Jay Palmore, who is a major figure in the field, very important person at the East-West Population Institute, was an assistant professor here at Michigan when we were writing the Taiwan book and helped write one of the chapters, although he hadn't been to Taiwan at that time, but he was brought up in the Philippines so had had a background in the Third World. We were asked along with the people in the Taiwan Population Studies Center to do a benchmark survey for the Malaysian family planning program. I organized a team consisting of Jay Palmore with two of our graduate students, Allan Schnaiberg and Chris Langford, who is now a professor at the London School of Economics. We did the survey, which was the background for the beginning of the family planning program.

At the same time, our friends in Michigan's population planning center were advising about the family planning program. They wanted a field man and I got my friend, Dr. J.Y. Peng, whom I worked with in Taiwan, to go to Malaysia; they have a Chinese as well as Malaysian population. I also got a young Ph.D., recently graduated from the University of Chicago, by the name of Lee-Jay Cho, and gave him his first postdoctoral job as adviser in demographics in the Malaysian department of statistics. Lee-Jay honors me as his guru because of that. Another network connection.

Other offshoots. I've mentioned Korea. At an early point, I became consultant to the Hong Kong Family Planning Association. Hong Kong's birth certificate at that time did not have the age of mother on it, so they didn't have any age-specific birth rates. I had done some preliminary work using indirect estimation methods. I'd published an article with a couple of our Indian students here which demonstrated that the decline in fertility that had been trumpeted in Hong Kong between 1960 and 1965 was really a result of age and nuptiality factors and not a decline in marital fertility. We set up a program at the Hong Kong Family Planning Association to estimate age-specific fertility rates by drawing a sample of 10,000 birth records from the nursing homes and hospitals where most of Hong Kong's births occurred; almost all the births at that time were occurring in institutions. We did that study for three years and published a number of things in which we were able to chart what was

happening. I have had a continuing association with Hong Kong. In 1987 they did their fifth KAP survey and while I was at Hebrew University I was consulting with them. And I was there in 1988, when we had a press conference about what had happened there. Their total fertility rate is down to about 1.4 and they've got an 82 percent contraceptive prevalence rate, and so on.

VDT: Talking about KAP surveys, did you set up the series of ongoing KAP surveys in Taiwan?

FREEDMAN: We get involved with most of those; more heavily with some than with others. They're quite capable of doing their own surveys. They do many surveys without us, but we collaborate. For many years after each of the KAP surveys, we have been publishing an article on trends in Studies in Family Planning. The one on the basis of the 1985 survey was published in 1987, I think.

VDT: That's right, you've kept it absolutely up to date. You also have connections in Thailand?

FREEDMAN: Yes. They started their first work in family planning with a pilot project somewhat modeled after Taichung.

VDT: I was going to ask, were there any pilot projects other than Taichung?

FREEDMAN: Yes, there were the ones in Korea. The one in Thailand, the Potharam project, was the one that had a heavy Michigan involvement. They approached me about doing it and I was too busy and I talked to Amos Hawley and he went as the principal demographic consultant. He needed a field man and I recommended Dr. J.Y. Peng again and he went there. That was a great success.

After that I was in Bangkok occasionally as a consultant, but other people did much more; Allan Rosenfield and Warren Robinson were there. We used to visit to consult, but I won't say we had a heavy role in that. More recently, my colleague John Knodel has become, I think, the leading non-Thai demographer working on Thailand, so he has a continuing association there. We've had Thai students. One of our prize students was Dr. Napaporn Chavoyan, who is at Chulalongkorn and doing wonderful work. She has done a lot of work with John Knodel. Brown University has had a long connection there and has had many Thai students as well.

I guess I should insert that in this short period in which I'm talking about myself, I may sound megalomaniac and it may give you the impression that I think Michigan did it all.

VDT: Some people think so!

FREEDMAN: I want to emphasize the fact that there were a lot of other centers involved who were doing important things. We crossed paths with Brown many places. They had a much more important role in Thailand, for example, than we did, until John got involved. After John came to us, I think we became an important element there. The fact that John spends half his time there is important. He knows Thai and . . .

VDT: Has done a lot of innovative work, such as his focus groups.

FREEDMAN: Yes.

VDT: Let me throw this at you. Peter Donaldson and Charles Keely had an article in the Family Planning Perspectives 20th anniversary issue of November/December last year, 1988, on the

international population scene ["Population and Family Planning: An International Perspective"]. They wrote that centers like Michigan and Chicago which trained so many Asian family planning leaders--they mentioned, for instance, Harjono--have declined in importance in that respect and that those programs need to be revived or alternatives developed if both family planning programs and population studies are to become stronger in countries where they are not yet well established, especially Africa. Do you believe that Third World people still need U.S. training in demography?

FREEDMAN: Yes, I think so. They have their own training institutions, but I would say that at the Ph.D. level, they still need to go other places. There are many more resources in the countries than existed at the time we were doing this earlier work. I think what's needed now is more networking, more strong relationships of the kind that we had with Taiwan and that Brown has had with Thailand, in which you had people going back and forth and there's a great deal of collaborative work going on. And in which the people who go back home know that they've got technical backing and the opportunity to come back to the place where they were trained and a great deal of collaborative work can be done, so that they've don't get isolated from what's going on in the outside.

That I think is certainly true with respect to Africa, which is a much more difficult place in terms of creating institutions and so forth. There, of course, Pennsylvania has taken . . .

VDT: A lead. Etienne van de Walle.

FREEDMAN: Right.

VDT: Now on a related topic. In the 1960s, there was some strain between demographers like yourself who were becoming involved in international population programs and the more traditional demographers. Donald Bogue in his interview in this series said that he, you, and Joe Stycos were leaders in the demographic group who had faith that something could be done about the population explosion and you were opposed by others who, as you yourself put it in your 1979 interview and you also mentioned it in your 1965 PAA presidential address ["The Transition from High to Low Fertility: Challenge to Demographers," Population Index, October 1965]: these others felt that involvement in family planning programs was not quite scientific, just social work. Could you say something about that controversy?

FREEDMAN: I never regarded that as a real dichotomy; I didn't think one had to choose. My point of view always was that one of the facts of social life in the Third World were these programs and they were to be studied. They were elements of the demographic situation. I felt, still feel, always felt, that they needed to be studied, as objectively as possible.

I was not as great an enthusiast and optimist as Don Bogue was. I always felt that one had to have a proper amount of skepticism about what was going on and not go overboard. I think I did go overboard myself. I not long ago re-read my PAA presidential address and while there are hedges in there about what was going to happen in India and Pakistan, for example, I thought there were signs that things were going to change much more rapidly than they have.

VDT: That's right. You said that within five years, there was to be a significant fertility decline in India and Pakistan.

FREEDMAN: That's one of the many ways in which I was wrong. On that score, by the way, I don't have any apologies, because most of the prominent demographers in the field have been wrong whenever they make projections. One of the principles I've learned is that social scientists greatly

under- or over-estimate the rate of social change and, in any case, the projections are very often wrong. But I think that's part of the scientific process. I remember thinking about these things and what I should say in my presidential address and I thought I should say what I thought was going to happen, the possibility, perhaps colored by my experience. I tried to be as objective as possible and I tried to instill that in the organizations that I was working with.

On the other hand, I must say that I felt that in order to be credible to the people I was working with, to get access to study what was going on, I also had to help them with the problems they had. They would not have been very satisfied if I had said, "Well, I'm here just to study these things and I'm not going to talk to you about the program."

At first I was reluctant to say anything about policy and the program, because I felt I really didn't have any training in that. I thought public health people knew the answers to those questions. I came back and started to talk to public health people and decided that despite all their merits, they didn't really know much more than I did. They were trying to find out what they knew; they didn't know very much. They hadn't really researched this sort of thing. So I felt that I should just give the best advice possible.

I tried to maintain a balance between these things and to express proper skepticism. For example, I think in our 1969 book on the Taichung experiment there is some extrapolation with skepticism. Usually while I was there, I would say I spent two-thirds of my time on what I would say was social scientific work and perhaps a third of my time finding out what was going on in the program. And I usually left them a four- or five-page memorandum about--given what I knew from the social science point of view--what I thought was happening on the program side and suggestions about what was feasible and might be done. And I feel that there's nothing wrong with that. I feel that those of us who did these things got information about what was going on in the program that many of our critics did not have. People like Kingsley Davis, for example, more recently Don Hernandez, and other people rarely studied--Kingsley, so far as I know, not at all--what was happening in the program. They had a kind of macro view of the situation and didn't have what I think are essentials to assess the role of these things.

In any case, it seemed to me--and still seems to me--that the way these questions get resolved is that people publish as openly as possible what's been done, open themselves to criticisms and corrections.

VDT: You and Barney Berelson certainly did that with your January 1976 Studies in Family Planning article on "The Record of Family Planning Programs." You were certainly then speaking to policymakers--I would say U.S. international policymakers, not just the program people in the countries. Presumably you set out to do that to refute the Bucharest "development is the best contraceptive" theory that had been espoused for a year or two before that. You showed that family planning programs had had a measurable, independent effect on fertility decline. But you hedged your conclusions there: the record was mixed, you said at that time.

FREEDMAN: That's still true, of course. It's much more complicated than we thought it was.

About the other thing I've learned over the years. When I began in the field, it was easy to concoct theories because we had few facts; it was much easier to deal with the theories when we had few facts. We've learned a lot since then, which has made simple theories much more difficult.

VDT: That's right. I was going to ask you about your contribution to modifying the simplistic demographic transition theory.

You've just said you felt it was important to speak to policymakers--I'm also talking about policymakers in the U.S.--and you also must have talked to the press. In your 1965 PAA presidential

address, you added a paragraph before it was printed in Population Index because apparently it had been covered by the press and they had interpreted you as predicting rapid fertility decline within the near future all over the world. Have you always felt it important to talk to the press as well as policymakers?

FREEDMAN: Yes.

VDT: Was there press coverage of that speech?

FREEDMAN: Yes, people called me afterward. I had quite a lot to do with the press on the Growth of American Families study, because that was very unusual. There were columns written about it and I learned the hard way that once you speak to the press, you have no control over what goes forth.

Yes, I've spoken to the press. I've testified before congressional committees. I suppose half a dozen or more times, I've briefed various AID officials. When I was coming back from China after the one-in-a-thousand study [of 1982] that we worked on there, the cultural affairs and economic attaches in the U.S. embassy came to a seminar we had in Beijing and asked if I'd do something with the State Department [in Washington]. They called me in together with Bill Lavelle, my young collaborator on the China work. I've had a fair amount of that in the past, speaking to the press, interviews, things of that sort. I've always felt that was a responsibility.

I've been very happy with the growth of two institutions which I think are important. One is the Population Reference Bureau, the other is the Alan Guttmacher Institute, which I think have played a very important role in providing data and information--people who are expert at dealing with the media. I was chairman of one of the first committees that the PAA had on public affairs, before I was president of the PAA. [From the 1979 interview: At the meeting before the 1960 census, an ad hoc subcommittee of the PAA was formed, consisting of Dudley Duncan, Dorothy Thomas, Frank Notestein, Phil Hauser, Con Taeuber and myself. It was before COPS (Committee on Population Statistics) was formed, but we were trying to perform a similar function. That committee decided that it would be a good idea if the Census Bureau had a one-in-a-thousand sample and Con told me later that that was the shove that produced the one-in-a-thousand sample of the 1960 census. After that I became chairman of COPS; Dudley Duncan took it over when I became president. There were a number of things that the committee did at that time in connection with both the Census Bureau and Vital Statistics. We had an important role in connection with what I think is still going on, the questionnaire of followback studies of samples of mothers taken from birth statistics. We had a very important role in pushing and shaping that.]

I was in Hong Kong in 1987 for a press briefing on their KAP study and appeared on the front page of three Hong Kong newspapers, because they interviewed me about Hong Kong and then, of course, they wanted to know about Mainland China.

Yes, I've done a fair amount of that over the years.

VDT: Your family planning research, which raised some controversy among mainstream demographers in the 1960s, would you say it's now respectable among mainstream demographers? If so, you helped to make it so.

FREEDMAN: Well, it's moderately respectable. There's still a lot of controversy. But I think the legitimacy of doing things in this field is there. It depends what one does. I think I'm one of those people who have kept their foot in social science and in demography. I think people like Jack Caldwell, for example, and many others recognize that I'm a reasonable person on these things; that I listen to what they say and read what they say. Some people get swallowed up entirely in the internal

dynamics of the family planning programs and they have lost credibility.

I wouldn't say it's a standard part of the field, although there still are a lot of publications in the standard journals, Demography, Population Studies, and so forth, that deal with these issues. I think that some of us have helped to make that more respectable.

VDT: What are your views now on population growth in the less developed countries? As you know, the population growth rate decline has slowed. In the Population Reference Bureau World Population Data Sheets--of course, you're not supposed to use them as a time series--but the world's population growth rate this year, 1989, is estimated to be 1.8 percent, up from 1.7 percent in last year's data sheet. And the UN has revised its projections upward. Are you depressed?

FREEDMAN: No. A large part of that recent increase is because of the little upswing in China. I think that we ought to remember one thing, that is that the fact that population growth rates haven't come down so fast is largely the result of the fact that the fertility decline has been matched--and in some cases exceeded--by the mortality downturn, and that's all to the good. It's still the case that, with ups and downs, in an increasing number of countries contraceptive prevalence rates are going up. About China, people are saying terrible things are happening, the birth rate is going up and there's a very high birth rate there. I think that's sort of a ridiculous view, that is, the total fertility rate has come down from 6 to 2.0 and it's bounced up to about 2.4. The thing to focus on is 6.0 to 2.4, in some places a range close to 2. It's just a remarkable change. It goes with fits and starts. Many people were pessimistic about Latin America. Many Latin American countries have had very substantial increases in contraceptive prevalence that were not expected at the time.

There are still many points of difficulty. I would say there's a long way to go in Africa. Change is still very slow in the Indian subcontinent. There has been some fertility decline in India, but not as much as some had initially expected. I might say that after that 1965 PAA speech in which I was so wrong, when I had an opportunity to observe the Indian family planning program and its difficulties, I became much more pessimistic. And nothing has happened in Pakistan; some progress in Bangladesh. And, of course, in the Muslim world, by and large, where you have Muslim fundamentalists not much has happened. But even there there are exceptions. There are parts of Indonesia which are very strong Muslim areas in which the TFR is down, the contraceptive prevalence rate is up. The latest data from Egypt and Tunisia show contraceptive prevalence up, birth rates down.

No, I'm not pessimistic. But when I talk about demography, I always apply what I learned as a weather forecaster: that is, don't look out the window when you're making a weather forecast. Short-run trends are not the significant thing. I still feel that if one looks ahead for 25 or 30 years, there are going to be significant declines in fertility. But there are going to be a lot of difficulties in the period--no question about that.

Another way to look at this is to ask how many people back in 1960 thought the places that have had the fertility declines were going to have them. I would say very few. I was told by very distinguished demographers when I began to work in Taiwan that any program there would just skim the cream off the top and nothing much would happen. I remember that in the early 1960s there was a Milbank Memorial Fund meeting and Clyde Kiser edited the proceedings, Research in Family Planning [1962]. Taiwan and Korea are not mentioned in that book. And very few people expected that Thailand would have gone as far as it has. So we have to be very careful, I think, in making these projections.

VDT: Could you tell something of your view of "revisionism"--the idea that rapid population growth is not necessarily a barrier to economic growth. You and Bernard Berelson were already saying that in your lead article in the Scientific American September 1974 issue on "The Human Population." This

was long before that idea became fashionable, with Julian Simon and the U.S. policy turnaround at the 1984 Mexico International Population Conference. Obviously, revisionism was not a new idea with the 1980s. Do you deplore, for example, the cuts in U.S. population aid and the shift to justifying family planning aid on the grounds of health: not being able to come right out and say it's on the grounds of limiting population growth?

FREEDMAN: I think it should be on both. Under Ray Ravenholt, the USAID policy was a very bad policy. Here is where I think that extremism did a very great disservice to the field: the idea that access to family planning would immediately lead to large fertility declines. I never held that point of view and I would say anybody with a real socio-demographic background would understand that that wouldn't happen. I think they greatly oversold what was possible.

I have never believed that you're going to convince anybody that they ought to be using family planning by citing figures about the GNP or macro growth rates. In Taiwan we were emphasizing from the beginning the health and welfare of the families that were involved. So I think that both of those have to be elements in the situation.

There's enough evidence for the revisionist view of the demographic transition. The Princeton European Fertility Study and other work have indicated that simple theories with respect to the role of economic and social development in fertility decline simply don't work. There are too many exceptions to that. But it's a long, complicated subject.

VDT: Right. But I'm glad we've touched on it and how you are among those who have revised the idea of the demographic transition.

Now that we're on your theoretical work, I'd like to talk a bit of your theoretical work on the sociology of fertility. Along with everything else you were doing in these years, you put out your first 1962 piece on "The Sociology of Human Fertility" [*A Trend Report and Bibliography*, "Current Sociology, Vol. X/XI, No. 2, 1961-62]. Was that the first time that you laid out your funnel model?

FREEDMAN: That was the first time I published it; I had been using that in my teaching for quite a long time. That is a very simple model, but it seems to have had a lot of effect.

VDT: A marvelous organizing framework. I used it in my first term paper when I was an M.A. student at Georgetown in the late 1960s, a paper I did for Murray Gendell on 300 years of French-Canadian fertility, from the 1600s, and got an A on it: 300 years of fertility laid out and what would explain it. That was just a wonderful organizing system and has been celebrated ever since. Were you conscious of the need to organize?

FREEDMAN: Yes. I did something there that nobody can do again, that I can't do again. I read everything in that long bibliography of 300 and some items that was attached to it. My essay was simply trying to relate to that bibliography, which I organized as far as I could in terms of that framework, and I read everything at that time. You can't do that anymore. I discovered that when I revised. My revision [*The Sociology of Human Fertility: An Annotated Bibliography*, 1975] was not as good as the original, I think, because I quickly discovered that I didn't know a lot of the literature and couldn't possibly cover it. I had it under my personal command when I did that first work.

I think I helped in a small way to pave the way for the important concept of the proximate determinants. Not because I invented it; I used the Davis-Blake intermediate variables framework and that had had some influence. But I think that I helped to popularize that and give it international currency, because I made it an essential element. I thought it was very important. Then John Bongaarts later picked it up and did what I didn't do and it's now quantitative and can be made part of

models; there are various ways of using it.

VDT: He just worked on the proximate variables.

FREEDMAN: Yes. The proximate variables, which is simply another name for the intermediate variables, is, I think, one of the important empirical and theoretical contributions in the last 15 years of fertility study. That's being used all over the world. I don't want to detract in any way from the credit that John Bongaarts gets for really putting this front and center.

VDT: Yes, but his was only that one box and yours was the whole picture.

FREEDMAN: Yes, I had the framework going behind that. In general when I did this diagram, the box farthest over to the left was the social and economic setting and a sub-part of that box was family planning programs. I don't remember whether that's clear in the 1962 version; I've had other versions since then. I've always put the family planning programs as a small part of the big box. The family planning program is part of the social and economic environment, part of the background. And I always made it small, as part of this larger thing, to indicate what I thought was its proper role; that is, it is not the determining role, it is part of the social and economic environment.

VDT: You summed up much of your years of experience and your philosophy, your approach to research, in your March/April 1987 Studies in Family Planning article, "The Contribution of Social Science Research to Population Policy and Family Planning Program Effectiveness." I wish all leaders in the field would write such articles.

Back to Michigan and the Population Studies Center. Did you ever consider setting up a separate department of demography?

FREEDMAN: No, we never had any idea of doing that. In our initial proposal to the Ford Foundation and in every report of the Population Studies Center, we always had the idea that work in demography and on population issues should be grounded in the social sciences.

Now, there are other ways, as I indicated in my little message on graduate training ["Graduate School Training of Demographers: The Michigan Model, PAA meeting 1988]. There are programs which are demography and you can seat them in various departments in various ways; there are other ways of making that connection. I always felt it desirable that our people should be trained as sociologists, with a major emphasis on population issues. My colleagues here also felt very strongly in that way. That was one point I emphasized to Bud Harkavy when we set up the center. I said, "We are not going to be a demographic center; we're going to be a population study center." As I conceive population study, it is demography in its social and economic setting. So, no, we never had that idea.

When the department of demography was set up at Berkeley, I was offered a chair there. The year before, I was there on a sabbatical, in fact finishing the book on Taiwan, and Deborah was finishing her doctoral dissertation. Judith Blake, who was setting that up along with Kingsley, arranged for me to be offered a chair, be part of that program. I thought it over and on a number of grounds decided that I didn't want to do this. One thing that influenced me was the fact that it was going to be completely separate.

VDT: Draw from many different disciplines, right. You, of course, have your bias but do you think sociology is the best background for a demographer?

FREEDMAN: No, I won't say that. Yes . . . I like sociology. I think it's a broader field. We've

trained a lot of economic demographers here, about a third of our graduates are economic demographers.

VDT: More and more. And you now have several on the staff.

FREEDMAN: We've had somebody on staff for a long time. Paul Demeny was our first economic demographer; I brought him here. When he left, I brought Ron Lee here; Ron began his academic career here. Then we had a series of other people. Eva Mueller was here during this whole period. And my good wife got her degree and joined the group as well.

I think sociology is a very good basis for this connection. And most demographic training in the United States has been in sociology departments. I think economic departments are not in general as hospitable. In so far as they tolerate population people, they have to be people who will demonstrate first of all that they are echte [real] economists; otherwise they're suspicious of them. But I think that's a good base. I think there are many disciplines that have substantial contributions to make--for example, anthropology, very important, public health, other disciplines. But we can't be in all of these places.

There are some models of centers that involve all of those people. The North Carolina center is an example; they've done a very good job of that. It's a difficult thing to do. That hasn't been my view and I think the one we followed here was the one I was comfortable with. I decided I wanted a model I could work with. If I was going to lead this, it was going to be something I could work in. I wanted to continue to be a scholar; I wanted to continue to be identified with the discipline I was in. And I got a group of people who were comfortable in doing that.

Early on, we brought on Dudley and Beverly Duncan, who were for ten years very important elements of the center. They helped set up our apprenticeship program. They had the emphasis on sociology, as I did. We brought Ren Farley in. Dave Goldberg was with us at that time as well. We brought Paul Siegel in, various other people. We grew. And then, as quickly as we could, we started population work in economics, and that has been with us for a long, long time.

VDT: You also had history: John Knodel. You mentioned that in your PAA paper, bringing in history.

FREEDMAN: Yes. I was an undergraduate major in history. Yes, demographic historical work; John helped bring that here. And we've had some other connections with Charles Tilly, who was here; he had a joint appointment in sociology and history. There are many disciplines that are relevant. The question is how these can be organized. And I'm a great believer in diversity. There's the Pennsylvania model, different from ours; North Carolina is another model. They all seemed to have worked very well.

VDT: Yours in particular worked very well. The Caldwells say in their book that Chicago staffed Michigan and Michigan staffed Wisconsin. Norman Ryder, of course, set up Wisconsin's population center and he said in his interview that Michigan graduates were the best. You pointed out in your PAA paper on the Michigan model that you stress the apprenticeship system, so your graduates "hit the ground running" and need little in-service training when they're launched.

FREEDMAN: The UN likes our people for that reason; we have a number of people there.

Yes, all the leading demographic people at Wisconsin, with one or two exceptions, were our students. They were members of a very unusual cohort over a couple of years: Larry Bumpass, Jim Sweet, Robert Hauser, David Featherman, who has just become president of the Social Science Research Council, Doris Slesinger. These are all Michigan people who reassembled at Wisconsin. Quite a few years ago, I gave a couple of lectures at Wisconsin and at that time I counted 17 Michigan

PhDs in the sociology department there.

Those people have gone on to make their own careers. They're very distinguished, very able. If they had gone some place else, they--Bob Hauser, Larry Bumpass, people like that, had such ability that--well, the way I would put this is that with people like that we couldn't do them any serious harm. They would have made it anyway. But we had the facility and I think we had a significant influence in that way. That's what I was talking about when I talked about our former students.

VDT: This is not quite a fair question--you've mentioned many of them--but who have been some of your leading students? I think your list would go on and on.

FREEDMAN: When I knew you were coming, I wrote down the names of a few others I would mention. One of them is Krishnan Namboodiri, who is at Ohio State now with a fine chair, was chairman at North Carolina. Krishnan was another indication of outreach. Amos Hawley and I, at the request of the Population Council, set up a population center at the University of Kerala [India]. I told them I didn't want to do it until I had some people to send back. Krishnan was finishing his Ph.D., one of the best students we ever had. He went back along with a man named Pillai, who is still there, to organize that center. He stayed for five years; we backstopped him. It didn't work out very well, for reasons I won't go into now, not as a result of Krishnan's difficulties. We put a lot of emphasis on our Third World students going home and we felt we could do that because we were going to support them. Gerhard Lenski, who had been a colleague of mine at Michigan, was chairman at North Carolina and about five years after Krishnan was back in India, he called me and said, "I know how you feel about students going back to the Third World, but we need somebody in demography and Krishnan is my number one candidate." And I said, "Well, he's paid his dues; call him." He went to North Carolina and had a distinguished career and is still having a distinguished career. I was delighted to be able to select him, along with Jane Menken, as the two people for lead papers in a session which I've organized for the IUSSP meeting in New Delhi [September 1989].

Anrudh Jain I consider one of our distinguished students; has had a distinguished career since he left here. I went to India with Anrudh when he graduated to make sure he would get a decent start there. I had to do a lot of table-pounding in many of these places to make sure these students were given an opportunity to do the work they were capable of doing. That was one of the things I thought was important.

Dr. T.H. Sun, who directed for many years the family planning institute in Taichung. He is now the vice-director of the research and evaluation division of the executive office of the president of Taiwan, very important position.

Another of our students who I think is doing extremely well--publishes all the time; a consultant--is John Casterline at Brown; one of the students I work with and have a lot of regard for. Jack Kantner, one of my first students. I've mentioned Napaporn Chavoyan. And then finally--I could go on for a long time--but I'll mention Jim Phillips, who's at the Pop Council now and who is the key figure in making the Matlab project [Bangladesh] so well known in family planning and demography. So those are students that . . . There's a long list of others.

VDT: And you mentioned right at the beginning of the interview that if you're remembered in history it will be through the ongoing influence of your students.

Who have been some of the leading influences on your career? You've mentioned Hawley.

FREEDMAN: I mentioned Amos Hawley, Clark Tibbits, and Angus Campbell.

VDT: Remind me, Angus Campbell was . . .

FREEDMAN: Angus Campbell was director of the Survey Research Center. He not only helped conceive the Detroit Area Study, but when we started GAF, I went to him and said, "We're going to do this study." That was a very controversial thing I did when I did that study--now everybody takes all this for granted--the idea of interviewing people in a cross-sectional sample of the United States about the use of contraception and miscarriages and how many children they wanted, things like that. The newspapers did not deal with those issues in those days. And I was warned by a number of people that this was going to blow up: the Bishop of so-and-so and the Catholics would be against this; the fundamentalists would be against this, and so forth. So then I had a talk with Angus and he said, "Let's try it; let's do it." The whole budget for that study was \$75,000, from the Rockefeller Foundation. But because of what I knew might be the problems, we proceeded very carefully. First of all, I organized two sponsoring committees. One was a medical committee, distinguished ob/gyn's and heads of medical schools, who wrote a letter and went on our letterhead. Then I organized a list of distinguished lay people; I had Judge Learned Hand and other people on that.

VDT: And you say Angus Campbell was such an influence?

FREEDMAN: He was an influence because he backed the idea that the Survey Research Center would put its influence on the line. When the grant from the Rockefeller Foundation went through--at that time there weren't that many grants in a university like this--when it went through our Board of Regents, I got a call from the Secretary of the university saying the Board of Regents were concerned about what the backlash might be from a study like this. I spoke with a Catholic physician who was one of the regents and he said they didn't want to infringe on my academic liberty in any way, but they were just concerned about public reaction. I said, "I think it will work all right. We're going to do this very carefully, we're going to pretest it. But I can't guarantee there won't be any problem." He asked me if I'd be willing to speak with Monsigneur Somebody from the Archdiocese of Detroit and I said, "Sure." I went to Detroit and showed the Monsigneur the schedule and he looked at it and said, "Well, there's no reason why any good Catholic can't answer your questions. You're asking questions of fact. But if you use these in a biased way, such as Mr. Kinsey has got in his book, we will feel free to attack you." And I said, "Of course."

Well, to indicate how careful we were, we did elaborate pretests. First of all, we had some problems in that the interviewing staff of the Survey Research Center indicated that they were very worried about asking these questions. So we picked a number of counties for a pretest. We went to an area of Brooklyn that was heavily Roman Catholic. We went to Jeff Davis County, Georgia, which was then the poorest county in the United States. We went to a rural county in Iowa and a rural Catholic county. We tried it out. And the interviewers who had this experience said, "This is easy." This is one of the important things about GAF that people don't know now, because they don't remember how difficult the subject was then. So one of the primary things that GAF showed was that you could do studies like this. We had the highest response rate that the Survey Research Center has ever had on a survey with a non-captive population.

Now, we had some backlash. In one area in the Southwest, two women in a neighborhood who had been interviewed talked to each other about the interview and said, "They shouldn't ask us these questions," so they went to the chief of police. At that time, the supervisor of the interviewers always registered with the local police department to tell them what we were going to do. And he called me and I told what we had done and he said, "Well, there's nothing illegal about asking people any of those questions. They don't have to answer, do they?" I said, "No." And it went very well.

So I would say it is not simply the substance of what we did, but this broke the idea that you couldn't do these things. You remember that the Indianapolis Fertility Study did not interview

Catholics; they interviewed only white Protestants. That was because they were worried about what would happen.

Now, when I began to work in Taiwan--wherever I began to do these things, I would get the same thing, "Oh, you could do that in the United States but you can't do it here." And my response was always, "Well, let's try it out." And in general our experience was that this was a subject that was close to the hearts of the people that we were talking with. The interviewers in the end said--now everybody takes this for granted--that it was much easier to interview them about their family's fertility and family planning than it was to interview them about inflation, prices, things like that.

VDT: Right. Well, we were on the list of who had been influences on your career.

FREEDMAN: I've mentioned P.K. Whelpton--not so much from an intellectual point of view, although his cohort analysis had a great influence on me--as because he gave me the opportunity to do the Indianapolis study and without Pat I don't know whether I would ever have done the GAF study. So that was an important influence.

I've mentioned Barney Berelson. He had an influence on me in many, many ways. We worked together. We did the first systematic evaluation of population work in the World Bank. We did a number of research things together. We were constantly on the phone with each other, exchanging manuscripts, and we had a great deal of influence on each other.

I have to mention my wife, Deborah, with whom I've had a long professional interaction. She got into this field after our kids were in junior high school and we've collaborated. She reads most of what I write; I read most of what she has written. For a while, we were on separate tracks; we've collaborated in recent years, and that was important.

There's a long list of other people; I can't give you all of these. Irene Taeuber was somebody with whom I communicated a lot and I think she was a major influence on my views on what was possible in East Asia. There are some important, charismatic people in the Third World. I mention just one, Dr. S.C. Hsu, who was head of the rural health department in Taiwan, who really made all that possible and was a great influence on my view of what could be done there. I could name other people like that. But these are a representative, good sample of leading influences.

VDT: Good. How do you get everything done? In the 1960s, for example, you've mentioned it was a five-ring circus. At that time, you produced "The Sociology of Human Fertility." You were setting up the Population Studies Center and the Taiwan program simultaneously. You were teaching. You edited Population--The Vital Revolution [1964], which was a series of Voice of America radio broadcasts. Did you set those up?

FREEDMAN: Yes.

VDT: You organized the 1967 Fertility and Family Planning Conference here on the Michigan campus and coedited the book that came of it. You were PAA's first vice-president in 1963-64 and had to plan the annual meeting in 1964. You were president in 1964-65; there was your address for the 1965 meeting. The presidency of PAA involved more work than you'd expected; in your 1979 interview, you mentioned the difficulties with Don Bogue and the startup of Demography. There was the Detroit Area Study. How did you get all that done?

FREEDMAN: I've often wondered. I get tired when I think about it. Well, when I was a college student and had to work my way through college--I had a 40-hour-a-week job while I was going to school--I learned to get up at five o'clock in the morning. And I've done that most of my life. I've

reverted. I gave that up a while ago, but I find in my old age I don't need as much sleep. But I worked a lot. I worked long hours.

But I'd say that the main thing that made this possible was that I have always had very strong, good collaborators. I think I've had a knack for finding good people and turning things over to them. I ran the Detroit Area Study for just two or three years and then I turned it over to a series of my students and then it went on by itself. I've had no contact with it since then. I started the longitudinal study in Detroit and worked with it; I got Lolagene to work on it with me and she did a lot of work on that. I got Yuzuru Takeshita, Al Hermalin, Alden Speare, and other people to work with me on Taiwan and I gradually phased that work out. When I did some work on Indonesia, I had Sieu-Can Khoo and Boudon Supratilla to work with. I had Indian students working with me on the Hong Kong material.

More recently, I've been working in Mainland China--we haven't talked about that--and I have a young collaborator who is now the principal figure; I'm the junior author now for work we're doing on China. This is William Lavelly, who is a real China scholar. He has lived both in Taiwan and Mainland China, speaks and writes Chinese, is trained as a sociologist and a demographer. We've published jointly, but now he is the main person doing that work.

All of these things. I think that goes back to something I said earlier. I started something; I was an institution builder in a sense and got other people involved. When I brought the Duncans here, Beverly Duncan for many years did not want an appointment in the sociology department. She could have had it any time she wanted; she got it in the last few years of her ten-year stay here. But for about seven years, she was my righthand person in the center. The reason I could be abroad doing all these other things was that I knew that Beverly was handling administrative problems, in addition to doing her own research. So the idea has always been to get others involved.

I needed them for other reasons as well, namely, that I became obsolete from the statistical point of view. I would try periodically to keep up, but I could only keep up so far. When Bill Mason, who is our center director, came here and began teaching our statistics course, I took the course to upgrade myself.

The answer is my colleagues, partners, and coworkers.

VDT: You chose them.

FREEDMAN: Well, they chose me and I chose them. I was lucky. I also had some that didn't work out so well. And there were some things in which I didn't work out so well.

VDT: One thing I forgot to mention about those years was that you were vice-president and on the executive council of the IUSSP in 1965-73.

FREEDMAN: That didn't take much time.

VDT: And now you're going to the IUSSP meeting in Delhi.

FREEDMAN: No, I'm not going. I've organized a session but I'm not planning to go.

VDT: Do you feel the IUSSP is important?

FREEDMAN: Yes, it's an important organization. It is now a much bigger organization than it ever was before; having many seminars. As a matter of fact, on the 23rd of this month [June 1989], Deborah and I are going to Tunis. We have the leadoff paper in a conference that Jim Phillips and

John Ross have organized for IUSSP on essentially the relationship between family planning programs and their effect on fertility. Another replay on that theme.

VDT: What is your current work on China--somewhat interrupted now because of the troubles there? [A planned trip to China had just been derailed by the bloody events in Tiananmen Square and other parts of China.]

FREEDMAN: When I think about things I've done in the last ten years or so, I would emphasize something that happened there. I went there in the early 1980s on a WHO mission with Ed Wright, chairman of the mission, and Srinivasan to advise the State Family Planning Commission about evaluation of their work. At that time, China was very closed; it was hard to get figures on anything.

VDT: This was before the 1982 census?

FREEDMAN: It was before the 1982 census and the 1982 one-in-a-thousand survey. The last week of that mission, I learned that they were going to do what is now the famous one-in-a-thousand fertility survey--one-in-a-thousand meaning a sample of a million. Mr. Jow, the person who was traveling with us, didn't tell me about this until the last ten days. I was about to go to Beijing to participate in a training workshop on evaluation for all the statisticians of the provincial family planning programs. I was doing the stuff on surveys. And I was appalled by the fact that he hadn't told me they were doing this big survey.

This was in Sichuan. He came at ten o'clock one night to tell me about this. I was outraged that he hadn't told me before, but I said, "Okay, tell me about it." And he told me that within a month they were going to do this big survey. I went over the plans with him and I told him I thought they were not ready. The sampling wasn't well designed; the questionnaire wasn't good; they could not in the period of time they were planning to do it train the survey people to do all of this. I told him they ought to postpone it for a year. He didn't know if they could do that. We stayed up all night.

We went on back to Beijing and I gave this seminar and got his permission to use this as a case sample. I indicated what I felt the problems were and told them if they conducted the survey as it was, China would lose face because a survey this big was going to become internationally known. And in the report which we wrote to them and then gave to WHO, in my section we recommended that they wait a year. But before we wrote our report, the day after I gave this talk, we met with the Chinese leaders and they agreed to postpone it for a year.

I arranged for them to come to Michigan and to Princeton for consultation on the questionnaire and consultation with Leslie Kish on sampling. I arranged that Leslie would go to China--his first trip to China--to help them design the sampling. He gave a workshop on sampling and began to train people in sampling. After the survey, Ansley Coale did a publication for the National Academy in which he indicated that it was an excellent survey. My role in persuading the Chinese to postpone it may be the most important thing I've done in the last ten years. Some of the important things you do don't result in a publication.

Now Bill Lavelly and I have two publications based on our analysis of the survey ["Local Area Variations in Reproductive Behaviour in the People's Republic of China, 1973-1982," also with Xiao Zhenyu and Li Bohua, Population Studies, March 1988; and "Education and Fertility in Two Chinese Provinces: 1967-70 to 1979-82," also with Xiao Zhenyu and Li Bohua, Asia-Pacific Population Journal, March 1988]. We have a couple of other publications in progress on that work.

Last November, I was in China with Shelly Segal for the Rockefeller Foundation talking to the State Family Planning Commission about setting up some Taichung-type studies in which they would study the introduction of new contraceptives and analyzing the interface between the clients and the

provision of services. I told them at an earlier session and again in the session with the Minister that if they were going to do these studies, they had to be prepared for bad news; that any systematic evaluation was going to find out the problems. Were they ready for that? I said, "There's no point in doing this if you're not ready to get at the problems." And both at an earlier session with a lower-ranking person and with the Minister, they agreed they were ready. They said, "We're meeting a lot of problems; we need to know the facts. Deng Xiaoping says, 'Truth from facts.'" That seems a little ironic in view of what's happened recently, but that was the case. Everybody was citing Deng Xiaoping: "Truth from facts."

The two-week trip that has just been postponed--the middle of it would have been the time when the bloody happenings in Tiananmen Square took place--was to follow up on that. I was going with Parker Mauldin and Joan Kaufman to carry this one step further. We had developed some suggestions for experimental design and were going to try to develop this. Temporarily that's been postponed till next October, but I think it's going to be much later. We'll see what happens.

VDT: That's great. Perhaps you can add at the end some other things that are not on the record that you want to put on record.

FREEDMAN: I think I've talked enough. Any of us who have been presidents of the PAA, after 35 or 40 years in the field there's a lot to talk about. All we can do in an oral history is to get the high points. I think you've gotten on the record some things that I think are the important turning points of my career; places where maybe there's a little signal that got on the network that might have some repercussions afterwards.

VDT: What do you consider your leading publications, and why? Things that are on the record.

FREEDMAN: I think those are the two books, the GAF book [Family Planning, Sterility and Population Growth, 1959] and the Taiwan book [Family Planning in Taiwan: An Experiment in Social Change, 1969], and the 1961 sociological piece, with the bibliography and essay, sponsored by UNESCO ["The Sociology of Human Fertility: A Trend Report and Bibliography," Current Sociology, Vol. X/XI, No. 2, 1961-62]. I think perhaps those have had the most influence and are the most widely cited.

I guess also the joint piece with Barney Berelson on family planning programs ["The Record of Family Planning Programs," Studies in Family Planning, January 1976], which was rather crude at the time. A much more sophisticated, important work that followed was, first, Parker Mauldin's piece with Barney ["Conditions of Fertility Decline in Developing Countries, 1965-75," Studies in Family Planning, May 1978] and then the Mauldin-Lapham work [e.g., Robert J. Lapham and W. Parker Mauldin, "Contraceptive Prevalence: The Influence of Organized Family Planning Programs," Studies in Family Planning, May/June 1985]. In terms of influence on what happened later, those were more important.

You know, I think in the course of this discussion I've mentioned Parker Mauldin as much as anybody else and that would be certainly a person who had a lot of influence on my life. That goes way back, because Parker and I were graduate students together at the University of Chicago. That was a notable time, by the way, at the University of Chicago before World War II. Kingsley Davis was a postdoc when I was there with Parker.

VDT: I've interviewed Kingsley recently and he recalled that.

You write so well. You lay out all your points so neatly--one, two, three, four--easy for reading and remembering. That organizing showed also in your funnel model. What explains that?

FREEDMAN: That goes back to my high school days as a debater, where one did things like that. And at an early point, I developed what I subsequently talked to students about when they couldn't organize their doctoral dissertations. That is, first tell people what it is you're going to say, then say it, and then give them a review of what you've said.

When I had the first draft of my doctoral dissertation, W.F. Ogburn at that time was giving a seminar. This was after World War II when I came back to do my doctoral dissertation in the spring of 1946; he had a big seminar. There were still a lot of students at Chicago and I remember there were about 75 students there. We had a seminar on dissertation writing and Ogburn had seen a draft of a chapter of my dissertation. He asked me if I would talk in the seminar about the way I did this. If you would look at my doctoral dissertation, you would see that it was all done this way. That is, I had a first chapter that said what I was going to say and each chapter began with an outline of what was to come, and I said it, and then I had a summary of what happened.

It doesn't make for elegant writing. It makes for clarity and it leads people to follow what you're saying. But it's not the kind of writing that literary people do.

VDT: Well, it's excellent. Now on PAA. You covered some of your PAA recollections in your 1979 interview with Andy Lunde: for example, on your first PAA meeting and the people you met at the early meetings.

FREEDMAN [from the 1979 interview]: I think I joined the PAA in either 1947 or 1948. Amos Hawley suggested that I join. I can't remember if I went to the meeting in the spring of 1946 or the spring of 1947. It had to be at least that early because I got involved in research and was reporting research to the Pop Association at a fairly early point.

At the time, the Association was very small but wonderful. I went to my first meeting and the great giants were there: Warren Thompson, P.K. Whelpton, Clyde Kiser, Lowell Reed, Frank Notestein. And the thing that was so marvelous was these were not only the intellectual giants in the field, but they talked to me and became my friends while I was an instructor.

The thing that thrilled me apart from the fact that the giants spoke to us young ones was that there was a continuity from one meeting to the next, that is, an argument that had been taken up at the 1948 meeting continued in 1949. And if Dorothy Thomas was speaking, I knew the assumptions she was making. If I had any doubts on that score--and there were times in my teaching career when I had taken different research and teaching routes--they were erased by the enthusiasm. That Association was extremely important to me and I think all the other young people who were coming into the field at that time. There weren't very many people to talk to and those meetings were important for stimulation.

VDT: You obviously still enjoy PAA meetings; you come and you participate. You have a core group of people whom you know and see every year at PAA. Do you think PAA meetings are getting a little out of hand? We've just had a record turnout of close to 1,200 [1,193] at our Baltimore meeting [1989], 84 sessions, eight overlapping at a time, many spinoffs.

FREEDMAN: It's not as cozy as it was when there were three or four hundred of us, but I think that's inevitable. When I went to my first PAA meeting, I got on a first-name basis with all the leading figures by the time of the second meeting. I knew Warren Thompson, Frank Notestein, Frank Lorimer, Irene Taeuber, Dorothy Thomas, people like this, and that was very nice. Now those days are past; that can't be anymore. I still think that the PAA is much more manageable than the sociological or economics or statistical meetings. But it can't be as cozy as it was.

[On membership size, from the 1979 interview.] We're sometimes nostalgic for the days when we were small enough to meet at Princeton, but I think the growth has been good. I never have favored a special policy to bring in all of the people on what I regard as the margins of the field who have some sort of interest in this area. I think it is important that the organization continue to have a central core of activities and people who are concerned with the scientific study of population. Now, we have M.D.s, we've got public health people, geographers, and others. Many of them didn't get professional training in demography and are doing the work and that's fine. I'm against any kind of rigid exclusion policy. I think it's working pretty well in the sense that if you're not interested in the scientific aspect of population the meetings won't interest you. So I have no objection to somebody whose main interest is the family planning clinic in Fayetteville, Arkansas. If they want to join, fine, and if they become interested in the scientific aspect, fine; if not, I think they will fall away. We are growing, I think, at a reasonable rate.

One aspect of that question is better answered by newcomers, because those of us who've been in the organization for a long time don't find increasing membership a terrible burden because we've always known a core and each year we meet new people. Somebody who comes in this year, for example, will have a harder time, simply because there are more sessions, there are more people. I would have to say that they're at a disadvantage as compared with those of us who began when the organization was smaller. It's inevitable that we're larger, but I don't think it's out of hand.

[On Lowell Reed, PAA president in 1942-45; from the 1979 interview.] He was a courtly man. He was my picture of a Southern or an English gentleman; big, courtly man. He had a wonderful capacity for listening. I always felt he was genuinely interested in what I was doing. Sometimes I feel when people ask me what I'm doing, it's really an introduction so they can talk about what they're doing. Lowell was very good at bringing out young people. He also did a great deal in helping to organize activity in the Indianapolis Fertility Study, which I was involved in. Lowell was involved in a series of meetings of people who got involved in that study after World War II and I always found him very helpful in that respect. I had a different entree to him in another sense, because his son, Robert Reed, was a classmate of mine at the University of Chicago and I met Lowell through the son at an earlier point than I would otherwise. I always think of him as a man who was very important on the organizational side of the field. By the time I got to know him, he was an administrator; he was not doing research in population. But he was probably more impressive than anybody doing research in facilitating the work of other people.

VDT: Your first paper at a PAA meeting, I believe, was in 1949. You had a paper on "Some Aspects of Research in Differential Fertility"--it must have been the Indianapolis study--but that was read by Wilbert Moore, in a session chaired by Clyde Kiser.

FREEDMAN: Yes, I couldn't go to that meeting.

VDT: Was that the only one that you ever missed? In 1979 you said that you'd probably missed only one meeting.

FREEDMAN: I was not present at the meeting when I got the Irene Taeuber Award in 1981. I was in China. Al Hermalin accepted for me and I wrote a few words and said that Irene Taeuber would understand that being in China was an excuse for not being present when I got the award.

I think I missed a meeting with one of my heart episodes and the Minneapolis meeting in 1984; I think I was abroad for that one. But I've certainly been at the large majority of these meetings. I

don't think I've missed very many.

VDT: And you still participate.

FREEDMAN: I think there are very few PAA meetings in which I haven't participated in one way or another on the program, giving a paper or being a discussant or running a roundtable or something.

A story on that. When I had my first heart attack in 1981 and was recovering, Frank Notestein wrote me a note and said, "Ron, it's time to get off the center of the stage; the young actors are waiting eagerly in the wings." I've always remembered that.

VDT: Well, thank goodness you're not too much off the center of the stage--for China.

FREEDMAN [on the "big" issue of his PAA presidency, 1964-65; from the 1979 interview]: The big issue of my term of office was the foundation of Demography. It was a rough road. That involved Donald Bogue as first editor of Demography. In those days the budget of the Population Association was a very small number, maybe a couple of thousand dollars and here we were going to start a journal. It was clear that the costs for such a journal were much larger than the total funds we had and if we published for five years, we couldn't cover it. I don't know what would have happened if Don Bogue hadn't been as stubborn as he was. He laid out a set of rules of what should happen with Demography in order to be fiscally conservative. Don is a wonderful person, fine scholar, and he has a steel-like determination to do whatever he thinks is necessary to do. Every several weeks I had either a meeting or a telephone conversation with Don. I'd say, "Don, you're going beyond the guidelines. You're spending more money than we can possibly raise." And Don always said, "You're right; gotta change it." And he always went ahead and did exactly what he had done before.

One of the things that Don was doing was to print much bigger editions of Demography than we needed. And they were expensive. We felt we should print enough for the membership and a couple of hundred extra. Don was printing maybe a thousand or so extra on the argument that when this became established, the libraries and other institutions would want to buy back copies. But we didn't have the money to pay for them. In the end, we had a big deficit. Don told us that if we allowed him to take responsibility for selling those back issues, he'd cover the deficit. And he did. It was successful and Demography, of course, is well established and it's a fine journal. But if you talk to Paul Glick, he'll verify that that was a troubled period.

LUNDE [from the 1979 interview]: Not only that but something else happened. When we made a study of the membership of the Association, we found that it increased dramatically after the publication of Demography. There was a long period from 1931 when the membership remained small--gets up to about 300 or so, and even that we're not sure of. There were many lapsed members carried on year after year; nobody really knew how many members there were. After the publication of Demography, there was a jump within a year to about 875. [Membership was 660 in 1962, 802 in 1963, 1,142 in 1964. Demography began publication in 1964.]

FREEDMAN: Well, it was a good thing. I don't want to give the impression that I or any members of the Board at that time were against it; we were all for it. I suppose there had to be a whacky period there, but I think that it was an important event. It's obviously a well respected international journal now. I'm interested in what you tell me about the membership, because I never followed up on that, but that was one of the arguments, that, first of all, Demography would call attention to PAA and, secondly, it would be a specific benefit to people who joined. I'm happy about the way that story turned out.

Between one thing and another, the presidency took quite a lot of time. I had thought the presidency would not be very time-consuming, because the year before as first vice-president I had arranged the meeting program. At that time we didn't have a president-elect and the first vice-president served as the program chairman. It was a big job and I had done that the year before and I thought I could rest on my laurels. But Demography, especially, turned out to be time-consuming, but interesting and a worry.

Sometimes I got angry, like people do, but the thing that made all of this all right was the people were good people. I'm very fond of the central core whom I've known all these years. They're all decent people. I found that, with a few exceptions, if they said they were going to do a job they did it. It's not just a cold professional matter; there are deep personal loyalties involved and I feel a warm affection for the people involved, including some with whom I've never been in agreement on professional issues.

VDT: What do you see as the outlook for demography and demographers in the U.S.? Is there still room for the basic research and involvement in Third World programs, as you've done? Or does the future lie with business demography and state and local government; is that where the jobs are?

FREEDMAN: I can't foresee a time in the next decades--we can't talk about what's going to happen a century from now--when demographers and population study will not be important. The reason people are getting into business demography is because it's becoming evident there, as it is with so many other fields, that there is nothing in the world of social and economic arrangements in which there is not a demographic element. People have to come back to us even if they don't want us. That is, they've got to come back if you're dealing with teenage pregnancy, the problems of the aged.

One of the things I've done in the last few years is to help our friends in Taiwan to answer the question: What do you do with the use for the family planning and the apparatus it has for surveys and for action work after you have saturation use of contraception and a TFR of 1.6? What they are doing is using their family planning workers and their survey apparatus, (a), to study chronic disease, and (b), Al Hermalin is going out there soon, because they are one of five countries in which they're working on problems of aging, because they all have significant problems of aging.

Let me go back to what I said earlier about AID and I feel this way about the Population Crisis Committee and others who feel we're always going off the edge of the cliff--we're about to go into catastrophe. That all hinges on the idea that there is a population problem. There is no such thing. There are a series of population problems that keep changing. And therefore there's a great future for people in the population field. In some respects, many places, that will involve fertility and family planning. In others, it involves mortality, migration, problems in aging, the demography of AIDS, and all kinds of things. So on that score, I'm a great optimist.

There may be periods of time when funding is difficult and so forth. But so far as I can see, we're flourishing. Those people who are coming to the PAA meetings, who're overwhelming us and so forth, very few of them are unemployed. They're working.

VDT: Are you going to write your autobiography.

FREEDMAN: No.

VDT: What about your collected works? You're summing up Taiwan, in a sense.

FREEDMAN: I'm working on some of those. I did that piece in Studies in which there's quite a lot ["The Contribution of Social Science Research to Population Policy and Family Planning Program

Effectiveness," Studies in Family Planning, March/April 1987]. Apart from everything else, I don't think I'm important enough to write an autobiography. I've written pieces and people will be able to draw on them if they like.

VDT: You've had a wonderful career and you must have enjoyed it.

FREEDMAN: Yes.

INTERVIEW CONTINUES

VDT: Ron said a number of things during lunch that I'd like to capture. Right now he is talking about the fact that he is not going to the IUSSP meeting in September, although his name appears on the program as having organized a session. You explained that it costs quite a bit of money and that IUSSP meetings are . . .

FREEDMAN: Somewhat stuffy at present. And I've been to New Delhi many times and September in New Delhi can be very hot. Since my open heart surgery, I find heat a little trying. Also this is one of those several areas in which I've paid my dues. So I'm not going to do that again.

VDT: You were saying more on what the Chinese work was and what's now going to be interrupted by this turmoil in China.

FREEDMAN: What the Rockefeller Foundation had been discussing with the State Family Planning Commission was the idea of doing some studies that would involve benchmark surveys and observations on the introduction of new contraceptives. What happens to the women who adopt the contraceptives, that is, what is their subsequent fertility and family history, what complaints do they have, what are the continuation rates, how do they perceive the services they receive, how is what they are doing affected by the government program. That's from the side of the women. And at the same time, there was going to be a study of the providers: how skilled are they, to what extent do they know about the side effects, to what extent are they providing a quality service.

We were going about three weeks ago to meet with our Chinese colleagues at the State Family Planning Commission to discuss study design, how to study these things. We called that off in view of the disorder and concern about other issues of more pressing immediate importance.

VDT: Did you say there was a Michigan graduate who was going to be put in charge of the program?

FREEDMAN: No, that was another program. The Luce Foundation has given the Michigan group in our sociology department, under the leadership of Martin White and Barbara Anderson, a grant for a three-year program in which our group, together with the sociology department of Beijing University, would cooperatively run a program modeled after our Detroit Area Study in the city of Baoding, in which graduate students from Beijing University would receive formal training in survey research methodology and then participate in interviewing and coding on two studies, to begin with, one with respect to the changing family and one with respect to aging patterns. We had been given some assurances that the Chinese side would be led by Dr. Wang Feng, who got his degree with us about 18 months ago. It now appears that he won't be going back for a while. In any case, this is not a propitious time for doing work of this sort, sending students out to interview peasants. The time will come. We'll have to wait. That will be another chapter to come.

VDT: It seems a pity. This seemed to culminate your work--the networking and influence in Asia. Let's hope it's only temporarily interrupted.

FREEDMAN: I haven't told you about all the problems we had. We had many, many problems. It's not unusual if you're working in Third World countries that you run into difficulties.

I won't go into any detail now, but as we were about to go into the field after testing the protocols and questionnaires and so forth for the Taichung study, the whole thing was about to be called off. I went through a rather difficult month of negotiation before it got back on track. That's not unusual.

And when I told you about Malaysia, I didn't tell you about all the problems we had. There's a whole series of things. It's difficult to work in Third World countries. Afterwards you think about the pleasant part of it and what you accomplished and so forth. But . . .

VDT: Yet you persevered and got a tremendous amount done.

FREEDMAN: Oh, you have to be persistent.

VDT: You made an interesting remark about Nathan Keyfitz, who is now in Jakarta, Indonesia.

FREEDMAN: I simply said that I think Nathan has the capacity of taking almost any subject that he's involved with, demography or other aspects of social science, and putting a new light on things by looking at them from a different angle of vision than most people have. I shared that view most recently with my friend Leslie Kish, who had just been reading something of Nathan's, and other people, I think, have this same kind of view. He's a remarkable man.

I have to say also that a large number of the people you are interviewing, present company excepted, are very interesting people. We've had some really outstanding people in our population community, extraordinary people. Quite different. Dorothy Thomas is one kind and Irene Taeuber is another and Norm Ryder has got a different personality and a different set of interests. A lot of creative, dedicated, hardworking, interesting people.

VDT: Present company totally included!

FREEDMAN: It's been fascinating to have these encounters. And I think that the younger generation is having the same experience, with people like Ron Rindfuss, Larry Bumpass, and all these other young stars.

VDT: That's right. May I ask you frankly--I asked this recently of one of the other interviewees--who among the younger generation do you think might assume your mantle, your broad vision of what the field of demography encompasses?

FREEDMAN: Oh, I think the names I've mentioned are all extraordinary people. Larry has a very broad view. I'm very impressed with Ron Rindfuss of North Carolina.

In an intermediate stage, we've got a whole . . . I'm a member of the older generation, but there are people who are senior in the field who are still very young who are really quite extraordinary. I think right now there are three people who I think are very good in this respect. First of all, Sam Preston, who always comes up with interesting work, is a modest fellow but does extraordinary things. Jane Menken, a dear friend, who is very impressive.

VDT: I just heard that she was just elected to the National Academy of Sciences. Are you a member too?

FREEDMAN: Yes, I'm a member.

VDT: Who's the third one you're impressed with?

FREEDMAN: The third one I've forgotten, but there are others. Ron Lesthaeghe, a Belgian, and his wife Hilary Page are a terrific team in what I think of as a younger category.

VDT: I told Ron that often people in these interviews have come up with interesting afterthoughts and I've discovered several times that people, these demographers, have been musicians. And he said--well, tell us what you do.

FREEDMAN: I said I'm not really a musician, but as a boy I learned how to chant at the synagogue; my father was in a choir. And each year on Rosh Hashanah, which is the Jewish New Year, I chant the evening service on the first night. The last time I did that was the 50th time I've done that in Ann Arbor. I began to do this when I was here as an undergraduate student.

PAUL C. GLICK

PAA Secretary-Treasurer in 1962-65 (No. 11) and President in 1966-67 (No. 30). Interview with Jean van der Tak at Dr. Glick's home in Phoenix, Arizona, May 9, 1989.

CAREER HIGHLIGHTS: Paul Glick was born and grew up in Columbus, Indiana. He received all his degrees in sociology: the B.A. from De Pauw University in 1933 and the M.A. and Ph.D. from the University of Wisconsin in 1935 and 1938. His career of more than 40 years at the Census Bureau, 1939 to 1981 (with 21 months out during World War II), included posts as Chief of the Social Statistics Branch (1949-62), Assistant Chief of the Population Division (1962-72), and Senior Demographer (1972-81). Since retirement from the Census Bureau, he has continued his research and publishing and served as Adjunct Professor Sociology at Arizona State University in Tempe.

As a pioneering U.S. demographic authority on marriage, divorce, and the family, Dr. Glick has been called the "father of family demography." Besides dozens of anonymous Census Bureau reports, his publications include the 1950 census monograph, American Families (1957), two editions coauthored with Hugh Carter of Marriage and Divorce: A Social and Economic Study (1970 and 1976), a coauthored monograph on The Population of the United States for the 1974 World Population Conference, and numerous book chapters and articles in the professional and popular press. Among his professional honors, he has been president of the National Council on Family Relations and received the American Sociological Association Award for Outstanding Career in the Practice of Sociology.

GLICK [from the biographical introduction]: I was born in Columbus, Indiana, about two counties south of Indianapolis. That's where I went to high school and went on from there to De Pauw, like an older and a younger brother, all three of us having scholarships to go there, and all three of us eventually wound up with Ph.D.s.

VDT: Marvelous--of that wonderful family of eight children.

GLICK: Four are still living, two boys and the two girls of the family. It was quite a different family context from my own. My wife and I had only two sons, just enough to make a bridge foursome--something that we didn't do in the Bible Belt of Indiana where I grew up. We didn't bridge; we didn't dance; we didn't do a lot of things.

VDT: What led to your interest in the general field of demography? Did it start at De Pauw?

GLICK: At De Pauw, I was a sociology major, with a minor in psychology and a minor in zoology. I was interested in the biological aspect of human behavior as well as the intellectual and psychological aspect. My main interest, though, as an undergraduate and as a graduate student at Wisconsin was social psychology--what causes people to behave the way they do. How do motivations, attitudes, habits develop; what relation are they to body chemistry, social relations, and so on. I majored in that through the first two years of graduate school and a master's degree. Even in the fall of my second year, I took my final written examinations for the Ph.D. majoring in social psychology.

However, it happened that about that time, my statistics teacher, Sam Stouffer, moved from Wisconsin to Chicago and talked me into spending the summer quarter of 1935 at the University of Chicago, where--among other things--I took courses with Professors William Ogburn, Ferriss, and

Leon Thurstone, who taught a course in multiple factor analysis. I had in mind using that technique when I went back to Wisconsin to study liberal and conservative attitudes of people. We had a data set, 1,200 students, with lots of detail, so one could study attitudes toward religion, which was the original purpose of the study, and toward economics and the political system. I hoped to get a range of high to low attitudes and see how these items correlated among themselves and with other information--the students' IQs, grade point averages, and personality tests.

I found that there was no faculty member in the University of Wisconsin sociology department who knew what multiple factor analysis was. So, about that time, Professor Thomas McCormick, who had taken Professor Stouffer's place teaching statistics, had received the mammoth grant of \$1,200 from the university graduate school to make a study of birth rates in Wisconsin, with particular reference to how they were affected by the Depression. He said to me, "If you will be my assistant, you can get a dissertation from this study." I checked it out with my social psychology professor, Kimball Young, the grandson of Brigham Young, A Jack Mormon, and he said, "It's okay with me if it's all right with you." So that's the way I got to study the trend in the birth rate in Wisconsin from 1920 to 1935, with a big dip during the Depression years and upturn. One purpose of the study was to find out what kinds of groups were most affected by the Depression. To oversimplify, the groups that had the highest fertility changed the most. They were the Catholics, poor people, people on relief--things that we could measure.

VDT: How did you get the background data for the births?

GLICK: We had birth statistics from every county of Wisconsin for all those years. It was a time when birth registration was not very complete, so McCormick and I figured out a way for me to spend hours upon hours in the basement of the state capitol building with these birth records, testing the completeness of birth registration, based on periods of time when both births and deaths of babies and children were reported, on the assumption that deaths were more completely reported than births, which is a reasonable assumption. We used that information to calculate a correction factor.

VDT: But how did you get the socioeconomic data for those births, or did you have that just by county?

GLICK: We didn't have that birth by birth; it was only by inference. That is, the counties with the highest economic level, the oldest settlement, the most Catholic counties, that kind of indirect evidence.

VDT: And that became your dissertation?

GLICK: That's the way I got into demography. While I was still a graduate student, many of us, including my colleague Henry Shryock who was eventually one of my closest colleagues at the Census Bureau, took a social science analyst examination for federal employment. When we were taking this examination, I remember one of our colleagues a bit ahead of us, Calvert Dedrick, who got his Ph.D. at Wisconsin before us [and was already at the Census Bureau], told us, "When you make these applications, make them just as strong as you can, because other people are going to do the same thing. But just be sure you can verify everything you say." We made our applications and from that source, Shryock and I, among others, were selected to work on the 1940 census, beginning in 1939. I went there on July 31st, 1939.

VDT: You had received your Ph.D. by that time, in 1938?

GLICK: That's right, and I had taught two years, the first at De Pauw and the second at a small school, Whitman College, in Walla Walla, Washington. Along in the early spring [of 1939], I got a letter from Philip Hauser at the Census Bureau, asking if I wanted a position with the Census for two and a half years, during what they called a decennial census period. Interestingly, I got another letter from Frank Notestein at Princeton University, asking if I wanted to come there to work on the Indianapolis study of fertility and its social and psychological correlates.

Well, Henry Shryock had finished graduate school a year ahead of me and he was at Princeton, where he was doing such things as proofreading the Population Index. He also got an offer to go to the Census Bureau and I could have gone to Princeton but I was concerned I might end up doing what Henry was doing, so I just decided to go to the Census Bureau.

VDT: You thought that was the better position?

GLICK: If it was Henry's better choice, I felt it should be my better choice. So that's the way I got there.

VDT: You must have already had a reputation in the field--the fact that Frank Notestein asked you to join the Indianapolis survey.

GLICK: Well, mainly because of Henry Shryock knowing me at Wisconsin and recommending me. Notestein probably didn't know too much about me; he took Henry's word.

VDT: You went to the Census Bureau with the famous "class of 1940."

GLICK: That's right. Many of us were former students in statistics of Sam Stouffer, either at Wisconsin or at the University of Chicago. Many of the rest were bright young people freshly though the bachelor's degree at New York University: Ed Goldfield, David Kaplan, Meyer Zitter, Abe Jaffe--well, of course, Jaffe was one of Sam's students at Chicago.

VDT: Jay Siegel has told me there were two "classes of 1940." He considers it two classes--that New York group you mention and he does concede there were the others who had just gotten PhDs, like you and Henry Shryock, and who were some of the others? Con Taeuber, of course, was not yet at the Census Bureau.

GLICK: Con came in 1948.

VDT: And Phil Hauser, of course, was already there, on his way to becoming the assistant director.

GLICK: I can almost count on one hand the professionals who were there before me. There was Leon Truesdell, chief of the population division, Phil Hauser, assistant chief, a fellow named James McPherson, who was a machine tabulation expert, Dick Lang, one of Sam's students from Chicago, Felix Moore, and that's about it. Dick Lang was my first supervisor, in the field of the family.

VDT: You were taken on as a family analyst?

GLICK: I was asked, "Would you rather be in charge of fertility or in charge of family statistics?" I said, "I guess I'll take fertility, because that's what I did my dissertation in." They said, "Well, Abe Jaffe is coming and he wants to be chief of the fertility branch." "Okay," I said, "then I'll go with Dick Lang in family statistics." And that's why I got into family statistics.

VDT: It wasn't really your deliberate choice.

GLICK: It wasn't my choice to get into demography; it was to be in social psychology. It wasn't my choice to be in family; it was to be fertility. But in the end, all these areas of specialization worked into my career. I worked quite a lot with fertility and people there who were eventually in charge of that. Family statistics was kind of a new field; family demography was considered a kind of off-to-one-side field. It's kind of hard to study, because family life changes in its life course. You have people in and out of different relationships. It's not like studying a woman and her fertility from beginning to end. But eventually, a lot of family statistics got analyzed in terms of, say, the head of the family or the family composition and social changes.

VDT: About your early Census Bureau colleagues. Did you work with Wilson Grabill right at the beginning?

GLICK: Initially, yes. He was deaf, as you know. He came to the Census Bureau before I did, in 1937 I believe, when Phil Hauser was conducting an unemployment survey through the Census Bureau. He moved from that survey into the population division, in the 1940 census. Grabill was a whiz at the calculating machine and was supervising people working with the old Marchand and Monroe calculating machines. Then he took a master's degree at American University in sociology and demography and worked in to be Abe Jaffe's assistant in fertility. When Abe left and my superior Dick Lang went to the War Department during World War II, Grabill and I moved up into their slots. That's how Grabill moved into fertility and he stayed in that area until he retired many years later.

VDT: What did you do on the 1940 census?

GLICK: The 1940 census produced about ten reports on families in three areas, family composition, which is my main area, and also family economics and housing. I was responsible primarily for four reports which had to do with things like types of family, size of family, and age of head. There was a special report on the population of military age and a report on institutional population. All those were kinds of reports that became basic and we had planned to continue them.

VDT: They were new with the 1940 census?

GLICK: There was one 1930 report called Families. It was about an inch thick and old Dr. Truesdell had planned it as one of his special reports on the 1930 census. There was also a heap of tabulations for additional reports that never got published. I analyzed some of those and put out some small reports on them.

One of my first jobs at the Census Bureau was to help change the designation of Mexican from "other race" into white. When we showed comparisons of the 1940 census with 1930, we added the Mexicans into whites for 1930 so it would be comparable to 1940. The Mexican people had been greatly upset that Dr. Truesdell had regarded the Mexicans as not white, and in fact most of them were white.

Another thing I did when I first went there was to make a study going back to the 1910 census to get material for fertility. I went to New York City and set up a group of WPA people to copy off a sample of women and their fertility . . .

VDT: Children ever born?

GLICK: Children ever born and whether they'd been married more than once, age at marriage, and their personal characteristics. That's where the 1910 census material came from that appeared in the 1940 census reports. It had never been published before.

VDT: Were you able to get these 1940 reports done before you left for 21 months during the war?

GLICK: Most of them were done. We had a string of, I think, seven fertility reports and Grabill was working on those. When he saw the rest of us being taken off into the service, he wanted to be in the service and he volunteered! They wouldn't take him because he was deaf.

VDT: During your wartime service, did you work on the Stouffer sociological study which became The American Soldier?

GLICK: I was one of those people who moved from the Census Bureau to the army during World World II, working with Sam Stouffer.

VDT: The program set up by Frederick Osborn?

GLICK: General Osborn was head of our Information and Education Division of the army.

VDT: Sometimes called the morale division.

GLICK: We were the information branch, otherwise called the morale branch. There was an education branch, which took a good deal of information that we collected from surveys among the soldiers, to find out what the soldiers thought--about their equipment, their relations with officers, their facilities, and their attitudes relating to the war. I remember about that time, John Lewis, who was head of the mine operators in this country, put on a strike, while the rest of us were in the army, to increase the income for the miners, who were not in the army. So we asked, "What do you think of John L. Lewis?; what do you think of Mr. Hitler?" They hated John L. Lewis more than they hated Mr. Hitler!

VDT: That affected their paycheck.

GLICK: That same group of people were responsible for the point system used to let people out of the service. Questionnaires were given to the GIs to see who should be let out first: according to length of service; if they had families; if they had been wounded, etcetera. A point system was worked up and it worked very smoothly. There were never any objections that I heard of about the way people got out, based on that.

One of my first assignments was to study people who were in a field hospital, not very far from the front line. They were wounded or became ill and were treated in a tent out in a field. I had a corporal--I was a sergeant by that time--driving me out to investigate this. We came to a Y in the road and an MP was standing there and said, "Where do you want to go?" We said, "We want to go down this way to that field hospital." "Well," he said, "They're fighting three miles away."

VDT: Oh, you were overseas?

GLICK: This was in Italy. He said, "Put the windshield of your jeep down so the sun doesn't reflect

on it." I said, "Maybe that hospital is really back this way," and we turned around fast. It was back that way about a mile, over a hill, so just the top of the tent was visible.

That was the closest I came to being in battle. Otherwise I was in the rear echelon, making studies of one kind or another.

VDT: That was amazing that they had such sociological studies going on during wartime.

GLICK: Later while I was in Italy, Arnold Rose, eventually president of the American Sociological Association, and I were buddies in one of these up-and-down bunk beds. Of evenings, we sat together and worked on projects. Arnold would make notes about ideas he might want to pursue after he got out. I made notes too and some resulted in an article called "The Family Cycle" [American Sociological Review, 1947]. That's the way I got started with that idea. I thought, "We have data from vital statistics and the Census Bureau with which we could trace people when they get married, when they have their children, and so forth." That was where I got the idea for it.

VDT: I was going to ask you later about your research innovations and number one on my list was the family life cycle. You thought of that in a bunk under or above Arnold Rose in Italy!

GLICK: I gave a paper on it at the American Sociological Society meetings. It was the American Sociological Society then. They changed the name because of the initials.

VDT: ASS! I had thought it was because . . . Well, I've just interviewed Dudley Duncan and he said that a society means that you are the pure scientific approach. When you change into an association you've become big business.

GLICK: That was not why it was done.

VDT: You've cleared up that point. Not many people are able to use their professional expertise during wartime to serve the service. When did you get back to the Census Bureau?

GLICK: Right after September 21st, 1945. I'd gone in January 4th, 1944. I well remember, because I got out just the day before my birthday.

Soon after we were back, there was a rumor around Congress that maybe we had a few subversives in the government. This was maybe 1946 or 1947, well before the time of McCarthy. They said, "Social science analysts; sounds like socialism." So we got our perfectly good title changed to "statistician" and we were all statisticians until I became Senior Demographer.

VDT: That was the first time you had "demographer" in your title?

GLICK: They had statistician dash [-] demographer somewhere along the line, but it was never just plain-out demographer. It was never plain-out sociologist, which any of us would have preferred. Personally, I liked social science analyst. I thought that was one of the best titles you could possibly have.

VDT: Did you begin then working on the 1950 census?

GLICK: A pretest for the 1950 census. I remember that we went back to the same families in the pretest to ask them how they would report on the same items, to see to what extent they gave the same answers.

VDT: Was that the first time that had been done on the pretest?

GLICK: There had been some pretests for the 1940 census, but that was pretty much done before I got there. I remember in our 1950 pretest we found only about half of one percent or maybe two percent of the women reported themselves as head of the household. So we said, "That's such a trifling amount, we'll just make all the husbands the head of the household." And nobody said boo about it until the feminists came along.

When we asked marital status for the second time, some people who had said they were separated now said they were married. They said, "Well, we thought we were separated, but now we've gone back together, so we're married." I got the sense that whether you're married or separated is an attitude question. It's just not hard demography if it's not a clear status.

In that vein, the 1940 census showed no woman with children who was never married. The clerks, I heard, cleaned it up. They made all those women married so they could be a family head and "married with spouse absent." There was no such term as "separated" in the 1940 census. All the separated people were reported as married. If they had not become divorced, they were still married--that one term, "married."

When it came to 1950, I was in charge. Dr. Truesdell was gone. He was responsible for some of these earlier ideas, including that one person living alone was counted as a one-person family. That was gotten rid of too in later statistics. We fixed it in 1948. Government users of census data convinced us that there had to be two related people in order to make a family. We fixed that in the 1950 census and we also had "separated" and women "never-married with children" for the first time.

We had a question in there on duration of current marital status: How many years have you been in your current marital status? That was my idea and I remember Pat Whelpton and some other demographers said that sounded like a good idea. And for people who were married, we found out how long they were married and if they'd been married more than once and were currently married, you'd know how long they were in their second marriage, and so on.

One thing introduced in the 1940 census was what I called "husband-wife" families. They had been called "normal" families. I thought "husband-wife" families was descriptive. That was used in 1940, 50, 60, and 70. But by 1980 we called them "married-couple" families. That gets away from the word husband being used in front of wife. It was part of the movement away from sexism. That is the history of the identification of what was eventually called married-couple families. You'll find the term in the newspapers; they picked it up.

VDT: You were involved in every decennial census from 1940 through 1980 and the Current Population Survey and other Census Bureau surveys. What innovations did you have a hand in? You have listed a few. For example, what about adding the marriage questions to the Current Population Survey? You must have been the person who did that.

VDT: That was what we call marital and family history; it started in 1966. There was also a question on religion; it came back in the 1950s.

VDT: You got that into the 1957 CPS. Then you had proposals for religion questions in the 1960 and 1970 censuses and the 1977 CPS, but that never happened.

GLICK: In 1906 and subsequent years ending in six up to 1936, there had been what was called a religious bodies report. Each church filled out a questionnaire about their membership, Sunday school, finances, and so on. But in 1936 Roosevelt was President and there was quite a big opposition to him

in the conservative South and those churches down there did not cooperate. They said, "That's a New Deal questionnaire." So the statistics were incomplete and they just never financed a Census survey on religion again.

I knew from my own experience that the church is quite an important thing in the social life of people and it would be useful to know how the country was divided among the religious bodies and the social and economic characteristics. So I arranged quietly to have a question--What is your religion? Lutheran, Baptist, Methodist, and so forth; that was the whole question--put into the Current Population Survey. I think it was March 1957. And 99 and a half percent of the people answered the question; at least there was an answer for that many. Maybe some people with German names might be reported as Jewish in New York City without even asking, I don't know. But only half of one percent didn't have an answer on that.

We published one report from it. Wilson Grabill and I put that report out. It just had some elementary age, education, and race information by religion. We had a nice detailed tabulation which showed religious categories--mainly Protestant, Catholic, and Jewish, but the different types of Protestants were also available--by education, occupation, income, that sort of thing. But before we got that second report published, there was a big uprising among people in New York City about this, saying, "You should not publish especially socioeconomic data about the different religious groups." They didn't come right out and say it, but I think they were concerned that certain groups might be shown to be higher status than others and there was a lot of talk about the Jews dominating the markets and so on. Well, I was just making this study from sociological interest, but the Census Bureau flinched.

So that fine report sat in my files until, in the mid-1960s, Congress passed a Freedom of Information Act. Con Taeuber, who was long since my mentor at the Census Bureau, then said, "We'll get this out and if people want it they can buy it for a dollar." It was about 20 pages of xerox copy, just tables, and lots of people bought it. Sidney Goldstein, another PAA president, published a fine article ["Socioeconomic Differentials Among Religious Groups in the United States"] analyzing this material in the American Journal of Sociology in May 1969. I personally could have done it but I didn't and I was pleased to see that Sidney did it. I was busy with other things.

VDT: You were not able ever again to get a religion question into any Census Bureau survey or the census?

GLICK: We came pretty close with the 1960 census. A political scientist named Foster from William and Mary College came up and made a study of how a government office makes a decision and this was the decision: not to ask a question on religion on the decennial census. I have a little monograph he prepared. He interviewed all of us. It got edited a bit along the line but essentially told how this happened.

We came fairly close again in 1977, which would have been 20 years after the 1957 study. Mr. Barabba was director then, a very fine Republican, by the way, who was very cooperative regardless of which party was in power. He was a marketing analyst; he was interested in religion from that point of view. Of course, I was just interested in updating this information. Barabba left right at the time the decision had to be made and the acting director was afraid that maybe whoever the new director was would not approve. He turned out to be Jewish, Plotkin or something like that, and probably would not have approved it anyway. The decision had to be made before he came. I don't ever expect that subject to come up again, but I hope they'll get away from this bugaboo about religion.

VDT: What about the marriage questions that you added to the CPS in 1966 and to the 1967 Survey of Economic Opportunity, which you talked about in your 1967 PAA presidential address ["Permanence

of Marriage," Population Index, October 1967]?

GLICK: In the early 1960s, COPS, the PAA Committee on Population Statistics, had among its members a very fine man by the name of Ronald Freedman. I give him credit for having persuaded us to get into this business of asking more questions about marital and fertility history. In 1966 the Office of Economic Opportunity, the special office to fight poverty, had \$2 million to spend on research and didn't know how to spend it. I said to Herman Miller, who was the chief of our population division at the time, "How about this marital-fertility history? That's related to poverty and you could get that angle into it." So we got the \$2 million to make a study of marital-fertility history for the first time, asking: When did you get married, divorced, remarried, redivorced and remarried?--two different marriages, complete history. And when were each of your children born?--up to five children, or maybe it was four and then the last child, which means if there were five, you could get the continuous data for five children. That was first done in the OEO survey of March-April 1967.

VDT: It was repeated the following year?

GLICK: It wasn't repeated. The CPS didn't yet have a marital-fertility history. They considered it for a much larger sample survey in 1968, but that survey didn't materialize. But we developed some probabilities of marriage and remarriage and the like out of the 1967 survey, which were published. I'm not sure the programmer was completely sure of how he was doing. I remember he said, "Here's a tabulation. See how it looks." We published it, but I don't have too much confidence in it.

After that, we didn't go into that kind of tabulation. We just made some distributions--in 1971, 1975, 1980, 1985, and probably will again in 1990. It's a chain of questions in the June Current Population Survey. On the left side is a page of economic questions about employment and income and on the right are all these questions about marital and fertility history.

We found not only when each child was born but its sex and with whom it was presently living, the mother or father or some other relative or some non-relative. So from those basic data, we were able to develop distributions of age at marriage, divorce, widowhood, redivorce, and intervals between these marital events.

Which reminds me of a time I got into trouble with my family life cycle paper. I had shown the median age at marriage, median age at the birth of the first child, median age at the birth of the last child, so I just subtracted one median from the other and said, "That's the average interval between." A man named Ted Caplow, I think he's currently secretary of the American Sociological Association, was at that time a graduate student at the University of Minnesota and he wrote a scathing letter to the editor saying what poor statisticians we had working in the field, that you can't subtract one average from another to find the average interval. In a later study, I actually measured these and it came out the same bloody way.

VDT: Good for you! Showed it didn't have to be so fancy. So that's the way you do that wonderful family life cycle, which is such an interesting innovation--the median age of women at first marriage, birth of first child, last child, marriage of last child, the empty nest syndrome and so on. It's fascinating and it dates way back to your idea during the war. And the data for that came from the 1967 OEO survey and the 1971, 75 and so on CPS?

GLICK: Yes. We did double-check with the vital statistics. They don't always come out exactly the same way, partly because the CPS reports are retrospective and may not be precise. And the vital statistics are not complete, because not all states have central registration areas from which they can derive this kind of information.

An interesting side story on that subject. When we were making the June supplement to the 1975 CPS, I was observing the interviewing for that survey in Atlantic City.

VDT: In 1975 or 76? These supplements are on the five or six year?

GLICK: They started out every four years--1967 [OEO survey], 1971, 1975. In 1975 they decided it would be nice to make it in the round, every five years.

In 1975 we went into this Negro home and on the wall it said, "God is the head of this household." I said to the interviewer, "You won't have to ask that first question!" That household had an elderly woman, in her eighties. When the interviewer asked, "When did you first get married?" she gave the answer. The interviewer said, "You said you've been married three times. When did the first husband die or did you get a divorce?" She said, "I didn't get a divorce, I just married another man." That made me a bit suspicious about the quality of data on this subject for older people, particularly people with not much education--people who would give answers, but they weren't necessarily correct. So most of our information from then on has been limited to women who were 75 or younger.

VDT: You mean that based on that one field observation you decided the oldest should not be included?

GLICK: Well, you could tell this from other information. Also, above 75 you have a lot of people in institutions and these are not in the survey. I think Dan Levine cut them out of the CPS after the 1970 census. We used to have a half sample in institutions--about half as many people as you would ordinarily ask--but since they didn't have any economic questions, because people in institutions are not in the labor force, then what use was it. You get marital status, maybe, and some soft sociological information but no economic data. So it was discontinued.

The CPS is not good for the older population. It's just in the decennial census. And in the 1980 census, for instance, they've got about an inch and a half thick report on the institutional population with beautiful detail on age, type of institution, and social and economic characteristics. I was very pleased to see that because Mr. Reagan and Company had cut down on the 1980 census badly. We got less than before out of it in terms of detailed reports.

VDT: Besides your family life cycles, there are many other innovations about the family for which you're well known, for instance, projections of the percentage of first marriages likely to end in divorce. Were you the first to do that, which is now hot news whenever there's a new projection?

GLICK: I think the first time I used that was in the 1971 survey. We found about 29 percent were expected to eventually end their first marriage in divorce. The next time we surveyed, 1975, I believe it went up to 34 percent or so and now it's about 50 percent. In fact, 1985 data show that about 55 percent of first marriages are likely to end in divorce.

VDT: Teresa Martin and Larry Bumpass say in their article in the February 1989 Demography ["Recent Trends in Marital Disruption"] that adjusting for underreporting and adding in separation, it's now up to two-thirds of first marriages that will end in divorce or separation.

GLICK: Adding in separation is something they've done through the years and it probably has certain merits, but it does inflate the results somewhat. There's a relatively small percentage of people who become separated and never divorce. Their data may have come from the wonderful new National Survey of Families and Households, which was started in 1987 and finished in 1988, so their

information would be updated from anything published from the 1985 source. [Martin and Bumpass's February 1989 article was based on June 1985 CPS data.]

VDT: Were you the first who did those projections, thought that up?

GLICK: I think I did the first ones on a national basis, based on the 1971 CPS. In 1984, based on the 1980 CPS, I not only projected the likelihood of divorce, but the likelihood of first marriage, of remarriage, redi-orce. I made a whole series of projections assuming that as people at the time of the survey became five years older, they would add the same amount of marital experience as people five years older added during the five previous years. You incrementally add that up. It was a relatively simple way, but I think the results come very close to those obtained by people who use more high-powered statistical methods.

VDT: I love the one where you show proportions of couples who will celebrate different marriage anniversaries, which you've updated from time to time.

GLICK: Conrad Taeuber asked me to put that into the report that you asked me to prepare.

VDT: The Population Reference Bureau Population Bulletin on "Marrying, Divorcing, and Living Together in the U.S. Today" [coauthored with Arthur J. Norton]. That was in October 1977. But you had done that already and updated it for that.

GLICK: No, I hadn't done that before [for both first marriage and remarriage]. Before the CPS eliminated the institutional population, Paul Jacobson at the Metropolitan Life Insurance Company talked me into asking a question about when did you enter your present marriage and to find out whether that was a first marriage or a remarriage. We published a report going all the way up to 85 years old and over. We estimated how many people lived together in a first marriage for ten years, 25 years, and so on up. I don't think the results are all that I would like them to be.

VDT: For instance, you and your wife before her death celebrated your 45th wedding anniversary and according to the table in your 1977 Bulletin, only one out of three couples reach the 45th anniversary of their first marriage.

Then there are the proportions of children living with two parents, both natural parents or not, one parent, and so on. Was that also your innovation--living arrangements of children? It's become a standard and interesting statistic.

GLICK: I think I did something on that subject way back about 1953, when Hugh Carter had his Office of Vital Statistics pay the Census Bureau to ask some questions relating to marital history in the CPS. I think out of that I found out how many children were living with parents still in their first marriage and how many were living with parents in their second marriage. But for some reason we didn't repeat the same kind of analysis until this survey of marital and fertility history came along. Then you could find out when people got married and had their children, so you could determine where the children were living in the first marriage, remarriage, or with divorced parents, whether their parents had remarried or were still unmarried, separated, never married. All the marital and family composition combinations you can think of became possible.

VDT: Those are always such fascinating statistics, and depressing, because the blacks show less than half living with two parents.

GLICK: At the present time, nearly 60 percent of black children live with just one parent, most of the time the mother, but it's about 15 percent for whites. These are children under 18 years old. For all races, 22 percent of children now are born when the mother is not married. This has a lot of implications for children's concepts of fathers, the father's role in the family. That's the kind of thing I might comment on toward the end of the interview: What are some of some of the implications of the trends, changes in family demography?

VDT: I was indeed going to ask you what you think of trends for the future.

Let's talk about cohabitation, unmarried people living together. I always recall the flak over the title of yours and Arthur Norton's Bulletin, published first in 1977. One of my favorite Bulletins, by the way. I reread it coming along through the desert yesterday and it's just great. It's "Marrying, Divorcing, and Living Together" and you had to go up to the highest echelons of the Census Bureau to get permission to use "living together" in a title. Up till then, wasn't it, the Census Bureau called them POSSLQs--persons of the opposite sex sharing living quarters?

GLICK: That leads to a point I was going to make if you wanted some fillers; I have a whole list of humorous things I could talk about. One is--it wasn't humorous at the time--but when Nixon was in his second term, about 1972, the poverty rate--which had gone down, down, year by year--made an upturn. The man in charge of poverty statistics said, "We've got a story for the press." So he rushed out a one-sheet report on the increase in poverty, for the first time since 1959 when such statistics were first reported. And, wow!--the Commerce Department jumped onto this with two feet: "What do you mean? You should have cleared this up through the Commerce Department because it doesn't look good for the Administration. You should have compared it with about ten years earlier; then it was a lot higher than currently."

That was what apparently stimulated them to hire a young fellow who had lost an election in Michigan and made him--well, we called him a spy. He sat in an office right next to me and was supposed to keep track of every publication going out to see if it would make some kind of damage for the Administration. And it came up with me. I had a report that had come out for many years, Marital Status and Family Status. I said "family status" sounds stodgy, let's make it Marital Status and Living Arrangements. This title came across this guy's desk and he blew a fuse: "That sounds mighty sexy." And he went up the line and when he got to Con Taeuber, Con said, "What's wrong with that?" Con backed me up and we've kept it to this day.

VDT: You have indeed. That's interesting.

GLICK: I had another experience, with the 1970 census. Herman Miller, our income expert, was supposed to make a speech downtown to a group of people interested in federal statistics, but he couldn't go so he asked me to do it. I made a speech about census income statistics and trends and so forth. Afterwards, Judith Martin from the Washington Post said, "You have just completed the 1970 census. I understand it's possible you may not have counted everybody." I said, "That's probably true. I've been with the Census Bureau a long time and every census we try harder to get everybody to cooperate and be counted. But there seems to be a growing antipathy to government and people's invasion of privacy. They just don't think it's necessary, all of it." "Well," she said, "how many do you think you missed?" I said, "We don't know; it's entirely too soon to tell. It may take us a couple of years to find out." "Do you have an estimate?" she says. I said, "Let me put it this way. In the previous census, we estimated we missed about 3 percent of the people. Considering everything, we think if we did that well in the 1970 census, it would be doing pretty well."

Next morning, headline in the Post: "Census Official Says 3 Percent of the People Were Missed in the 1970 Census."

VDT: Oh, lordie.

GLICK: And they came clawing into my office! The head of information in the Census Bureau--I don't think he liked the assignment in the least; he was really a pretty nice guy, John Casserly--but he was called from the Secretary of Commerce's office, "This should not have been done." Of course, the whole story was in the article, about it may be two years before we know. This fellow came and said, "Didn't you know better than to say such things?" I said, "I told them we may not know for two years and the only number I gave was for the previous census."

VDT: Headline writers are always impossible!

GLICK: It really made me sore. I said to him, "I've been here for over 30 years and I have never once had anybody speak to me the way you have. I think I have just done my plain duty." Well, he went away and I never had many relations with him after that. By the way, Con Taeuber backed me up 100 percent. He was my mentor.

VDT: Let's talk about some of the people you worked with. I do want to ask about your relationships with the public. You've always stressed the importance of collecting and reporting data to suit the changing needs of the public and policymakers.

But let's talk about some of your colleagues. You say that Con Taeuber came to the Census Bureau in 1948 [actually 1951].

GLICK: He came from the Food and Agriculture Organization, which had its headquarters in Washington. Before that, he'd been in the Department of Agriculture, head of their population research. Margaret Hagood followed him and Calvin Beale after her. FAO was transferred to Rome and he wasn't about to go to Rome. So then he was hired by the Census Bureau to fill in where Phil Hauser had left. Phil had been Assistant Director of the Census Bureau and then he was made Assistant to the Secretary of Commerce, who was none other than Henry Wallace.

VDT: Actually, he claims that he had those positions simultaneously. Con Taeuber moved into what position?

GLICK: He was called Assistant Director. They had other Assistants, who had specialties like machine tabulation. Con was in charge of things like population, housing, and agriculture. Those were things he knew about and he kind of gave the last word in these areas.

VDT: You've spoken of Con a number of times as your mentor. He gave the approval, so everything would be okay?

GLICK: Yes. Like, for instance, in 1970, soon after the census, three feminists barged in on Con and he asked me to sit in with him. They were objecting to the "head of household" concept as sexist. One of them said, "You have this question on relationship to head of household. The first question is, 'Who is the head of the household?' I put myself down as the head and my spouse I just put down as my husband." The second one said, "I put both my husband and myself as heads, joint heads." The third one said, "I left that question blank, because I didn't know what to do with it." And between them they

said, "You have to do something about this. We can't have you following this practice of not letting the wife be the head. We know what you do: you just change all these wives who are heads to make the husband the head. Anyway, we don't like the term 'head of household.' That sounds like the other spouse is some kind of second-class citizen--the wife is just also a member of the household."

So Con said, "Well, we'll do something about it." But we didn't--until 1975, when I said, "Look, we're going to get into the 1980 census and we had better do something about this issue." So I arranged in 1975 to have a test in one of the eight what we call rotation groups in the Current Population Survey. It's divided into eight groups; they change in different cycles. We had a special page of questions asked of every married couple: Who is the head of this household, you--whoever the respondent is--your spouse, both of you, someone else, no one. And the highest count was they liked joint heads of household. Second to that was to let the husband be the head--the usual.

Well, my reaction to this was, you can't have two heads of a household, because the sister of one is a sister-in-law of the other; the father of one is the father-in-law of the other; son, son-in-law. If you want to get into relationships like that--and we do--well! So we said you have to have just one person.

Arthur Norton claims he joined with me in creating that when you first ask for the listing of people in the household, you start with the person, or one of the persons, in whose name the home is owned or rented. If the home is not owned or rented, then start with one adult, any adult. So that was adopted in tests for the 1980 census and in the CPS before the 1980 census. It was accepted and there were no arguments about it.

Actually, before this decision, there were two incidents. Mary Grace Kovar, one of our leading demographers, asked me to come to a meeting at her home when this was boiling. She said, "There will be some women there." I assumed there would be some men, but there was just a roomful of women--with claws. A little exaggeration. "Why do you have to do this? There must be some other way." They did not want the term "head of household." They did not want anybody in the household to be the number one.

Then in a second incident, Barbara Bergmann of the University of Maryland economics department, head of our advisory committee on economic statistics, put this topic on the agenda for one of their meetings and I went and talked it over with them. Barbara said, "If you don't get rid of the term 'head of household', I personally will lead a sabotage of the 1980 census."

VDT: She's now at American University and I've seen her at Harriet Presser's monthly seminars [at the Center on Population, Gender, and Social Inequality, University of Maryland].

GLICK: They were both in this meeting. But when the solution came up, everything was just as quiet as could be. By the way, we called this first person in our report the "householder." That's important. I think I thought that up. Art thinks he joined me in thinking it up, but anyway--between us. No objection to the term "householder." It kind of fits in with "whose place is this?", not "who is the top dog?"

VDT: There was no objection when you finally did use it in the 1980 census?

GLICK: Jim Sweet pointed out that it was hard to relate it to earlier data. He said, "Suppose you have an old widow who can't run things; her son-in-law lives with her and runs it. It may be that previously he would be counted as head of the household. Now you'll make her the householder [owner of the home] and maybe you'll change the concept in that respect."

We agreed, but stuck to whoever is listed first will be left as the householder. So we get about 4 percent in husband-wife households, now called married-couple households, where the wife has

reported herself first. And that's the way the statistics come out. Characteristics of the householder? Okay, she's in there, or he's in there. Actually, there are not an awful lot of significant things where this matters. But once in a while you want to classify households by the age of the householder, so you know about where in the family life cycle they are. For items like family income or number of children in the household, it doesn't make any difference who's the householder. And if you want to identify a household as a married-couple household, you don't even have to inquire as to who's the householder. So we get away as far as we can from the designation.

Now, we had thought that with the regeneration of feminism, there might actually be more households in which the wife would consider herself the number one, the householder, so with the change, we'd be getting in step with the times. But we found that many of the wives who were householders had a husband who was incapacitated. Or he's unemployed; she's employed. She has more income, more education, superior characteristics, and apparently a man who has a superior woman, he's married to her, lives with her, may let her be the dominant person in the household. Actually, in the first relevant analysis I made, back in 1960, I found about 7 percent of women had more income than their husbands.

VDT: That's one of the interesting things you've had in your studies over the years.

GLICK: Now we know about 18 percent are in the same bracket or have more income than their husbands. In fact, it's over 20 percent if you include families in which the husband has no more than one or two thousand dollars of income more than the wife. In such families, the husband has very little, if any, more than the wife. That reflects changes over time in that respect.

VDT: You obviously enjoyed your time at the Census Bureau. It is certainly not a stultifying government agency.

GLICK: No. I was given quite a bit of freedom in the things I did, which apparently made sense to the people up the line who had to review these things. For instance, we started to talk about cohabitation. By the time of the 1950 census, I had a sneaky feeling it would be interesting to know--just out of sociological curiosity--how many men and women lived together without benefit of clergy. So, from the 1950 census, I had one table with a very obscure title like "unrelated individuals living together."

VDT: Wasn't it "sharing living quarters"?

GLICK: That comes later. This was just a table which showed how many men were living with women not related to them. In the box head were the women's characteristics; in the stub were the men's characteristics. That was where there were just two adults in the household. They may or may not have children, but we did show whether there were children. We found out if they were listed as partners or lodgers or employees of the head of the household. Most of them were just the roomer or lodger type and eventually we just lumped them all together, because that's what most of them were. In the Current Population Survey and elsewhere, it's mainly just grouped together; can't tell them apart. We found only about 400,000, which is a very small proportion--about 1 percent--of all couples of man-woman combinations were unmarried to each other. Now it's moved up to 4 percent. It was about 500,000 in 1960 and so on up. The first time we really featured it was in the "Marrying, Divorcing, and Living Together" Bulletin in 1977.

VDT [looking at Bulletin]: Then it was 947,000 unmarried couples living together. It made a

tremendous jump in the early 1970s.

GLICK: And it has jumped on up. Today it's about 2.6 million and that's about 4 percent of the total number of couple households.

VDT: You've always gone back and updated each time the new CPS data come out?

GLICK: Yes. I don't remember all the numbers, but it's kept going up. Actually, it dipped a notch, about 5,000 in absolute numbers, about 1985, but it's going on up now.

VDT: And you were the first to put out these data?

GLICK: First national data. We didn't even feature it in our earlier publications, because it was not regarded as very important. But with the increase in postponement of marriage and the increase in divorce and increase in postponement of remarriage--those are the demographic things that were related to the increase in cohabitation. Of course, all kinds of social and economic changes were going on which are really the fundamental causes.

VDT: As I said before, you've always felt it was important to collect and report data to suit the changing needs of the public and policymakers. It seems to me that you were ahead. You had been collecting those data and when the time came, it was there to be used.

GLICK: Yes. I never did collect data to reform anybody or a situation. It's just my intellectual curiosity, in the context that, "Here is something the public deserves to know about and we're in a position to find out what the answers are." And at least to get some basic information, which other people may want to enlarge upon and scrutinize in greater detail.

VDT: You've also said that there are data that could help, for instance, marriage and family counselors. In your wonderful 1967 PAA presidential address ["Permanence of Marriage"], you said there are data that if used correctly by marriage counselors and for family life education courses in high school and college, they could help people choose the right mate--people who would be most likely to stay together in a marriage.

Is that how you saw your role? Well, you've just explained--"Here are the data which the public deserves to know, which can be of some use."

GLICK: About that paper, the title was "Permanence of Marriage." I asked Ross Eckler to introduce me, who was then director of the Census Bureau. He was one of the "class of 1940." He came in as an economist and was in charge of employment and income data and moved up the line gradually and eventually became the director, when Johnson was President.

I said things like: Wouldn't it be wonderful if we had detailed tabulations of the characteristics of spouses who lived together for a long time and those who lived together for a short time and see what the mix is, so you could have a sort of encyclopedia for the counselor and he can say, "Now, you have these characteristics and she has those characteristics. You have about x percent chance that your marriage is going to last." And others have only y percent, and so on.

VDT: You called that a marriage quotient.

GLICK: I said that for the combination of this particular pair, that is your marriage quotient. Well,

nobody's ever used that concept, including myself. But I introduced the idea that it would be good to have a more scientific selection of marriage partners and have questionnaires sent out to as wide a group as you could gather and assemble them and use the computer to put groups together to see how they match up. In fact, there was a group at the University of Maryland who were already doing this [in 1967] and I reported on what they were doing. Today, that's widely used. I know people who have done very well in that, and you probably do too.

VDT: Using data from the Census Bureau?

GLICK: No. The concept of couples being matched by those commercial people. Men report; women report; they match them up. One of the professors at our own ASU [Arizona State University] sociology department recently used the procedure of advertising in an upbeat newspaper and she's getting married very soon; she's real happy about it. Not that she's probably ever heard about my idea in this context, but it's a development that's come along.

At the end of that paper, I said something like this: "Now, I'm a demographer; I consider myself a scientist. It's my purpose to report facts and inform people. It's not my duty, as a social scientist, to do anything about it. But, if I were to do so" . . . Then I went on to say things like: fix it so that people make a better choice of spouses and make it harder to get married but easier to get divorced, even. And that there should be more positive propaganda or education about the values and virtues of marriage and not so much information that's so degrading about marriage.

Well, that's the closest I came to taking to the stump. But if I were to take to the stump for anything else, it would be in relation to a footnote in that same paper, which had to do with the sex of children. My wife and I wanted a little girl so badly we could taste it, but the second was also a boy. And we quit there. So I put a footnote: If couples can know how to limit the number of children, they should also be able to determine the sex of their children, with a blue pill for a boy, a pink pill for a girl, and a white one if they don't want any.

VDT: You didn't go on and try for a third?

GLICK: No. The doctor said he wouldn't help us get a girl even if he could.

VDT: I did and got a third son--who is an enchantment, so, okay.
Did you get married before you got your Ph.D.?

GLICK: Sam Stouffer told us grad students, "I'm going to kick you in the rear if you get married before you get your Ph.D." So, what did I do? I got the Ph.D. in the morning, got married in the evening, the same day.

VDT: I have a note in my file that I shouldn't say out loud--that you said you were almost 28 when you got married and you were itching! (Laughter)

GLICK: Well, that was the situation.

VDT: Nowadays, things are done very differently. My oldest son is nine years married and struggling to finish his Ph.D. His wife is very much the head of the household and she is earning a handsome income and he's earning none at all.

Do you think of yourself as a demographer or a sociologist? Like others, Kingsley Davis for instance, do you think of sociology as important in revealing the background to demographic behavior?

You have written that Census Bureau survey data and the census can't tell the whole story of marriage. Psychological traits and attitudes are also important in the story of marriage and the family and they cannot be collected by the census. But you started off that way and you've added your bit.

GLICK: That's not to say that the kind of research that I and others like me have done is worthless.

VDT: On the contrary! It just does not tell the whole story, you've said.

GLICK: Again, I go back to something my friend Sam Stouffer once told us graduate students. He said, "I'm in the business of making surveys and that's one of my specialties. But if I could pick half a dozen people who are as smart as Ralph Linton"--who was the number one anthropologist in the country and on the faculty there at the University of Wisconsin--"and send them out to spend several weeks on a given project just to get the sense of what's going on, I think you would get more out of that than you would from some expensive surveys, where you have a set questionnaire and so on." Well, that's another way to do it. It's good to have interviews with not necessarily a closed questionnaire and you get into some of this more subtle information that way. That's part of what I had in mind.

VDT: Which of your publications do you consider the most important and why?

GLICK: I made a list of about 20 items before you came. It's topped with the Population Reference Bureau Bulletin, "Marrying, Divorcing, and Living Together in the U.S. Today." [This list went astray after the interview, but Dr. Glick commented on several of the items, as follows.]

There is my PAA presidential address, "Permanence of Marriage." After that speech, a lady in front of me, Margaret Martin, said, "Do you believe those things you were saying about how to get people to select the right mate?" I answered, "Well, I tried to be interesting." She said, "You definitely were interesting!"

VDT: It was fascinating--the most different PAA presidential address I've read and I've read most of them.

GLICK: On that occasion, the Census Bureau let me hang loose and say what I had in mind. But I would never have said most of those things in a Census publication. Later on, Meyer Zitter especially, when he was the chief of our population division, encouraged us to speculate on what we thought was likely to happen in the future. Leon Bouvier was working with a congressional committee and asked me to make a presentation in front of Representative Scheuer on the future of the American family in 1979. I said to Leon, "I don't know all that much about the future. If I wrote a paper, it would mostly be about past history, with maybe some projection into the future." He said, "That would be fine." And that's what I did.

Once you develop an audience and people call on you, they expect you to have this wisdom that goes beyond what is known. My sense of science is that its main purpose is to give guidance as to what is likely to happen in the future. My experience in testifying before Congress a number of times was that they want to know what the trends have been and where they are going if nothing intervenes. So that has encouraged me in my writing to try to give a bit of historical context, to show at least where it has started somewhere back, and where it's going. The essence of science is the practical side of it.

VDT: Your writing is also so clear and straightforward. You have lots of tables and charts. I

remember when we were working together on the "Marrying, Divorcing" Bulletin, you said you loved to have tables and charts, with simple, non-jargonistic text, and descriptive rather than too analytical, although now, you say, you speculate on what future trends can be. Has that been a deliberate effort--to have a style of that kind?

GLICK: Yes. I had a fine background in English grammar and composition. Got an A in freshman English, which was the only one in the class. I couldn't understand how those other people didn't have as good a high school background as I.

VDT: Are you talking about high school or college?

GLICK: I got an A in college English, primarily because I had an excellent background in junior and senior high school English.

VDT: Why was that--good teachers?

GLICK: Excellent teachers. They had you be careful about things, to write clearly and coherently and with force. If you can't be understood, there's no point in writing. Now, if you took the straight script of this interview, it wouldn't always make sense. That's the way my interviews go. I hope it will be edited. But in writing, you can go over a draft and clarify it and that's what I like to do. There's no point in writing unless you have some purpose and it's stated clearly.

VDT: You have tables and charts--simple, straightforward.

GLICK: Yes, that gives a tangible element to it. I taught for a couple of years, sociology and psychology, at De Pauw and Whitman College. I had the feeling oftentimes that there's a lot of opinion and guesswork about what you were teaching; that if you just had more factual information, you would be more effective. So I always felt, as I was producing statistical compilations and analyses, that I was feeding something into the hands of college teachers--and to the general public. I usually try to make it interesting on something that's of current importance. I just have little interest in people who deal only in abstractions.

VDT: Let's talk about some of your other publications. There was *The American Family*, the 1950 census monograph [1957].

GLICK: It's not "The American Family." It's called American Families, intentionally. On page one, I pointed out that only some 20 percent or so of families have a husband and wife and at least two or three children, which is the conventional notion of a family. And you have all these other families that haven't any children yet or used to have children or have only one parent or goodness knows what other combination. So American Families was chosen as the title. When President Carter had the White House conference in 1980, it was called the White House Conference on Families. It was picking up the same idea. I don't know whether that traces back to me.

VDT: And, of course, there is Marriage and Divorce: A Social and Economic Study, the book you did with Hugh Carter. Why did you have the second edition so soon after the first, just six years apart [1970, 1976]?

GLICK: It got started as one of about sixteen 1960 census monographs that were financed by the

American Public Health Association. Most of them dealt with topics like specific causes of death and all the socioeconomic relations that went with that. There were three other reports [using 1960 census data; published by Harvard University Press]. One by Evelyn Kitagawa and Phil Hauser on socioeconomic characteristics related to mortality [Differential Mortality in the United States: A Study in Socioeconomic Epidemiology, 1973]; one on fertility by Clyde Kiser, Wilson Grabill, and Arthur Campbell [Trends and Variations in Fertility in the United States, 1968]; and then Carter and I had one on marriage and divorce.

We didn't get started on this until about 1965 and by 1968 we had a draft, but it wasn't until 1970 that it got published.

VDT: It was 1960 census data, but it got published in 1970?

GLICK: It had a heck of a lot of Current Population Survey data, updated information, by the time we sent it for publication about 1968.

Well, this book was popular and probably sold more copies than any other in the whole series. It was reprinted a number of times and by 1975, thereabouts, the Harvard University Press said, "How about some kind of an update for this?" They didn't want the whole thing rewritten, so they said, "You have a chapter that is a straight summary of all that went before. Suppose we just drop out that chapter and replace it was an updated chapter." Which was what was done.

In that 1976 chapter, one of the Princeton folks who reviewed it noted that there was quite a difference in tone as compared with the 1970 edition. 1970 had shown kind of a progressive development in the marriage situation. You had the baby boom, you had everybody getting married, the divorce rate hadn't gone up so much. And then the whole sky fell, pretty fast. So in this second edition, the last chapter doesn't jibe with the rest, because they didn't change the first part of the book at all. They didn't sell all that much of the second edition. But Andy Cherlin came along with Marriage, Divorce, Remarriage [1981], which updated it--smaller and less expensive and a very fine book.

VDT: You mentioned already your family cycle article, published in the late 1940s, which must have been one of your outstanding publications, when you think back. What others?

GLICK: A totally unrelated article. John Durand was in the United Nations and had a tie-in with the very fine lady from Sweden, Alva Myrdal, who was in UNESCO. She wrote me--I'm sure John Durand had directed her--asking me to give a paper at the Second World Congress of Sociology in Liege, in 1953. I accepted and, for whatever reason, I chose to do something that had never been done before--it's been done quite a bit since that time--and that was to project the lifetime earnings of people with different amounts of education. Herman Miller, our income expert, saw this paper and said, "I think I could make some modifications of that and we could get a paper out of it." So he figured out how much earnings people lost for the time they went to college and assumed a certain percent, 3 percent increase, in inflation--stuff like that. And we jointly published a paper, I think in 1956, on education and potential income. That paper got more publicity than most anything else I've written, except this "Marrying, Divorcing, and Living Together." I even found a match cover that says something on the inside about the value of a college education!

VDT: That's become such a familiar statistic and you were responsible! I'm glad to know that.

GLICK: Well, Herman promised to update this study periodically. Now this had to do with men and some women would say you should do it for women. And more recently, they have come up with

something for women that's relatively comparable. That's, as I say, one of the papers that got the most publicity, and it had nothing exactly to do with family life, which is my specialty.

Another was a paper which Irene Taeuber asked me to write for the AAAS meeting in 1959. I invited Jack Beresford and David Heer, who were my assistants in family and marriage for the 1960 census, to join me in a paper on family trends and prospects and in that paper appears for the first time the word "marriage squeeze." It was my idea that with the baby boom going on, there are women born in a given year who want to marry men who are older, born in a year when the birth rate hadn't gotten up that high yet. So there's an excess of women in the so-called usual ages to marry, as compared with men in their usual ages to marry. That was the marriage squeeze; it's hard for a women to get one of those men. I said, "Either there's going to be a delay of marriage or women are going to marry men who are passed over"--one or two things like that.

VDT: Marry men younger than themselves.

GLICK: Yes. I don't know if we contemplated then that if this situation ever turned around because of a decline in the birth rate, you would have women who were born when the birth rate's gotten lower wanting to marry men born when the birth rate was higher, so you have the reverse--a squeeze on the other sex. That's what we're in now. It turned over about 1980.

VDT: The peak birth year was 1957.

GLICK: You have to add 20 years or so to adjust for ages at marriage. So at this point, instead of an excess of women, you have an excess of men. The same situation occurred as the birth rate went down in the U.S. until the Depression. The people who were born during the Depression married about 20 years later, in the mid-1950s. The marriage age for women was then the lowest it has ever been and all but 4 percent of women and 5 percent of men married. Now what do you have? Twice to three times that many are projected never to marry--in the neighborhood of 10 percent are likely never to marry. I estimated in my last projections, based on projections of 1980 data, that 88 percent of men and 92 percent of women will marry.

So you have this marriage squeeze. It never received much publicity till somewhere around 1980. My first record of it is in a 1980 Newsweek article, but Art Norton, my former assistant at the Census Bureau, tells me it had come out somewhat earlier than that. In 1980 a newsreporter asked Norton, "What do you call unmarried people who live together?" He said, "Persons of Opposite Sex Sharing Living Quarters." "Oh," the reporter said, "that's POSSLQs." Oh, that has to do with cohabitation. I'm getting off the subject.

VDT: On marriage squeeze. You say it first appeared in an article you did with David Heer and Jack Beresford at the end of the 1950s. Did it get publicity then?

GLICK: It didn't stir up much excitement until around 1980, I don't know exactly when. I was then telling about the delay in marriage and increase in divorce. I said, as long as there's an excess of women, why should men hurry to get married. And if men did get married, why should they stay in this marriage if they don't like it; there are a lot of women out there "in the woods" who are willing to get married--or to cohabit without being married. That's where cohabitation fits into this marriage squeeze. I think there is a connection between the marriage squeeze and cohabitation, delay in marriage, and delay in remarriage.

VDT: And you said that this 1977 Bulletin, of which we had several reprints and an updated reprint, "Marrying, Divorcing, and Living Together in the U.S. Today," which you did with Arthur Norton, had

a lot of press coverage. I certainly remember that. It was lovely.

GLICK: I think it mentions the marriage squeeze in there.

VDT: Right.

GLICK: The attention probably began with this booklet. I said around 1980, but it was showing up in the late 1970s.

VDT: I already knew of your marriage squeeze when I was studying at Georgetown in the late 1960s, so the demographers knew about it.

GLICK: It was there. The trouble is, it depends on some influential journalist that gets hold of it. Going back to Sam Stouffer again. Somebody asked him, "What do you consider a good piece of research?" He said, "One that would get a good lead article in the New York Times." That's something I kind of kept in the back of my mind. I haven't hit the New York Times every year, but from time to time--and lots of other newspapers.

VDT: Written for them? You have written articles for the newspapers.

GLICK: I wrote an article for the New York Times only once. I did try one for the Sunday edition, but it didn't take. It's just that journalists will have a foot-long piece, reporting facts and commenting on something that I've done research on. It's satisfying to see that.

It's also satisfying to go to all these conferences that I go to and hear people say, "You wouldn't know how valuable the things you publish are to us in teaching our classes." And I say, "That helps to keep me going." And I like to go to conferences to hear questions raised about something that hasn't been developed yet. I go back and I do something about it.

I don't know whether it was a result of one of those situations, but there is another article I like very much. It was published not too long ago, in 1981. It was my idea, but between Graham Spanier and me--he and I got five or six articles out of one summer's experience with him under my supervision at the Census Bureau, in 1978. One paper was entitled "Marital Instability in the United States" [Family Relations, July 1981]. In that paper is a table from which I developed evidence that married couples who have sons are more likely to stay together than those who have, quotes, "nothing but girls." A critical statistic in a whole line of statistics was if a woman has two children and they are both sons, she is 18 percent more likely still to be in her first marriage at the time of the survey than a woman who has two children and they're both girls.

VDT: Another famous statistic!

GLICK: Feminist editors had cut that out of my manuscripts twice. I had it in one of my articles that got lots of coverage, called "Children of Divorced Parents in Demographic Perspective," in the Journal of Social Issues in 1979. The female editor said, "I don't think that is relevant," and she cut it out. One other time, I can't remember what the situation was, it was cut out. But I put it in a chapter, with a female editor again--and a male editor--and I was holding my breath, but they didn't cut it out. That book was called Children of Divorce, edited by two psychologists, Sharlene Wolchik and Paul Karoly, here on the ASU campus. I did a chapter on the role of divorce in family structure and I put in this point. That was only published in 1988, but it was supposed to be out in 1986 or 87.

VDT: What accomplishments in your career have given you the most satisfaction? You really have already said quite a bit about that. You just mentioned going to conferences and having people tell you how important your research is in their teaching.

GLICK: And being invited to be on programs and to be able to find an audience for what I've put in lots of time on--not just 40 hours a week, but weekends, holidays. I never did use all my annual leave. I just enjoyed what I was doing and enjoyed what response I got from it. That's the way it was.

VDT: Great! Who have been the leading influences in your career? You have mentioned Sam Stouffer many times and Con Taeuber, as your mentor at the Census Bureau. And Henry Shryock, who was your peer at the Census Bureau. Can you think of others?

GLICK: I can even go back to my family. My mother was a schoolteacher, in a little red schoolhouse. Drove a horse and buggy two or three miles to teach and she had these big boys in her classes. She always encouraged us in our education. My father always supported us in our education.

VDT: You said of the eight children, three went to De Pauw and, you mentioned, on scholarship?

GLICK: On scholarship; paid our whole tuition for four years. It was called a Rector scholarship. It was founded by a wealthy Chicago lawyer who took a liking to De Pauw University. He gave all these millions of dollars. Back in those days when I went to school, in the middle of the Depression, it was only \$200 each semester and for four years it was--\$1,600?--that's what we got for free. And it didn't cost me but \$1,600 besides that to go four years to college. I scrounged and did some summer work and the like to support myself, being the Depression. My father had helped my older brother go through De Pauw six years earlier than I. By that time, he was teaching and he contributed to that \$1,600 for me. Six years, later I was teaching and I gave money to my younger brother to finish his degree, at least undergraduate.

VDT: A strong family.

GLICK: We all three were teachers. My mother's family was full of teachers. I had scholarships for graduate study at the University of Wisconsin. Paid my entire expenses; I didn't ask anybody for a nickel.

VDT: Who were other influences?

GLICK: My major professor at De Pauw, Lester Jones, recommended me to the University of Wisconsin, which helped me to get a scholarship there. Sam Stouffer, who was teaching at Wisconsin, knew my brother at the University of Chicago when they were in graduate school, and through the combination of these contacts, plus my own credentials, I got a fine scholarship at Wisconsin.

Then there was Kimbal Young. I could go on about what all I learned from him in social psychology.

But none of my professors at Wisconsin ever helped me get a job. My first job after I finished my Ph.D. was to go back to De Pauw and teach while one of my former professors was on sabbatical leave. At the end of that year, I got another job teaching at Whitman College--through Professor Ogburn of the University of Chicago, with whom I had taken one course during the summer of 1935. He also knew my brother Clarence, who had gone to the University of Chicago. So I had a university connection in there that helped me along.

Then Phil Hauser--Clarence knew Phil--helped me get to the Census Bureau. See these many connections. So I owe a lot to my brother Clarence. I communicated with him on all my problems as I went through college, graduate school, and I just owe him no end. When I got the American Sociological Association honor last year [Outstanding Career in the Practice of Sociology], the one person I mentioned was my brother Clarence--I knew sociologists knew him, at least a lot of them did--because of his great influence on my getting started in my career.

Another person is Arthur Norton. As I moved up the line at the Census Bureau, he took over the marriage and family statistics branch. Then eventually he became assistant chief of the population division, in charge of demographic and social statistics, just as I had been--the same two spots I had been in. He's still there. We have continued to write articles together. As recently as 1986, we published an article on one-parent families, a very significant development in our demographic situation and a sad one in many respects. On the other hand, hopeful in the sense that it does allow leeway for people who get into impossible marital situations, at least they think they are. So it's sort of a tradeoff between a greater degree of freedom, options, on the one hand and the handicaps, sacrifices, difficulties people get into when they do split up marriages.

VDT: Others at the Census Bureau?

GLICK: Well, Henry Shryock was really effectively the chief of the population division, between you and me. Howard Brunsman for something like 20 years after Dr. Truesdell was the chief of the population division. He was a housing expert. In the 1940 census, he was chief of the housing statistics branch. We had a population and housing division in 1940 and then in 1950, there was a population division and a housing division. I thought that Brunsman would become chief of the housing division and that would let Shryock become chief of the population division. But Howard instead chose to be chief of the population division. So for about 20 years, Henry was the assistant chief. But Brunsman didn't know that much about what was going on among us folks, so Henry was the one who gave final approval on all our papers and reports as they were going up the line. I owe a lot to Henry also because from time to time he'd be on PAA nominating committees and got me nominated for offices in PAA. I've served in all of them.

But I remember that Dudley Kirk was chairman of the nominating committee when I was nominated for president-elect and Ansley Coale was my opponent. Now today, in retrospect, it seems absurd but I won easily. The ballots were counted and I knew about it. I called Ansley--I was secretary-treasurer [1962-65] at the time--and said, "The people have spoken. I don't know that they've spoken wisely, but they chose me." "Oh," he says, "I think they chose wisely." What else could he say!

VDT: And he became president the year after you [Glick, 1966-67; Coale, 1967-68]. I never knew that they have people run immediately again.

GLICK: Well, not often do they do that. I've been on nominating committees a number of times and I would never do that. I don't think it's right for a person to run the possibility of being defeated twice in a row. That's impolite.

VDT: That was a point I was going to bring up about PAA, but we're talking about leading influences. What about Hugh Carter?

GLICK: Hugh and I had many contacts. He was 15 years older than I. Back in the late 1940s, I believe, he had come to what's now the National Center for Health Statistics--it was then the Office of Vital Statistics--in charge of marriage and divorce. [Carter left the Immigration and Naturalization Service for this position in 1952.] I put in a request to civil service for someone to assist me in my

marriage and family activities and Hugh Carter came up on top of the list of candidates. Well, he was 15 years older. He was well known already for his work at the Immigration and Naturalization Service and maybe also with marriage and divorce. But I just passed over; I didn't request anyone until later.

But in the early 1950s, he had me help him make some studies that he wasn't able to get information about, on marriage and divorce, through our Current Population Survey. Then in the early 1960s when the decision was made to have a monograph on marriage and divorce, Hugh, of course, being in charge of marriage and divorce statistics [at NCHS], was a natural. And I was collecting and publishing all these retrospective data about marital trends. So the two of us were chosen to be the authors. It wasn't until the book was ready to go to print that anybody said anything about whose name should go first. And Mort Spiegelman, who was the man in the American Public Health Association who fed the money out to all of us doing the monographs, said, "Well, why don't we just make it alphabetical: Hugh Carter and Paul Glick?" So that's the way it turned out. But everybody--excuse my immodesty--regarded that as my book.

VDT: Yes!

GLICK: A footnote on that. My secretary of 13 years, who worked with me on that, referred to it as "our book." She really got interested and would say, "Do you really want to say it that way?" You don't find secretaries like that much anymore. She was wonderful.

VDT: How did you work--longhand, and your secretary typed it?

GLICK: Oh yes. I wrote everything longhand. My secretaries typed the copies of my papers that went out to reviewers. Another note. On my 1957 monograph American Families, one copy--the last of about seven carbon copies, apparently--went to Fred Stephan at Princeton University, a big name in demography back in the 1950s. He claimed that reading that manuscript gave him a split retina; at least he said that with a straight face.

VDT: Have you ever gotten into typing or word processing?

GLICK: I write in longhand and they word process all my papers at ASU. I refuse to take up wordprocessing. I like the kinetic feel of writing, which I don't get when I type. I learned to type before I went to college and could do the touch method. I could make perfect copies with all my papers. And I typed family letters--six, eight perfect copies. But when the family sort of split up, I got tired of typing and went back to handwriting. I like it. I can cross out. I can make a rough draft, a second draft, a third draft.

VDT: And save them all?

GLICK: No, no. I have all my last copies, but I don't keep all the crap.

VDT: Back to influences. Jay Siegel has told me about the group of sociologists who were gathered in Washington in the 1950s, perhaps already in the 1940s after the war, who used to meet informally at each other's homes to discuss their research. What are your recollections of the group? He said it was called the Sociology Discussion Group and included himself, you, Margaret Hagood, Abe Jaffe, Calvin Beale, Henry Shryock, the Taeubers.

GLICK: That's pretty much it. Those were people who had PhDs in sociology, with one exception,

and that was Jay Siegel [also Calvin Beale]. Jay Siegel took his M.A. at the University of Pennsylvania and I think it was Margaret Hagood's bright idea to give him a dry run at a final Ph.D. examination. So all of us guys just twisted that fellow in one of these meetings. Jay Siegel never did get a Ph.D.

VDT: He's very sensitive to that.

GLICK: I credit some of it to this mock Ph.D. exam. He didn't think it was going to be that tough. Margaret Hagood was in charge and he took the guff. Of course, with all due respect, Jay has published a lot of first-class material, but he's kind of slow on publishing. That may have contributed to his never getting the Ph.D. But I like the idea that maybe there was some contribution also from this rough treatment we gave him.

VDT: He didn't tell me that story! But he told me about the group.

You were also involved with the D.C. Sociological Society and have been president of it, along with many other outstanding demographers, though now they seem to have gotten away from demography. I think it was you who recommended me to be their newsletter editor, which I was, for a couple of years.

Also, I have to put on record the lovely story about the one time I was to give a paper at a professional meeting. It was a D.C. Sociological Society meeting in the early 1970s and Irene Taeuber got the Stuart Rice Award that year. It was a beautiful Saturday at Howard University, a well-attended meeting, and everyone stayed to hear Irene give her speech. My paper, coauthored with Murray Gendell, on illegitimacy in Washington, was to be given in the afternoon. But everybody left--except you, the two Taeubers, and Murray Gendell. I've always been grateful to you for that!

GLICK: Well, that's on record now. (Laughter)

VDT: You were elected to IUSSP [International Union for the Scientific Study of Population] in 1949, one year after it was reconstituted on an individual membership basis and you and Henry Shryock are often there. Have you attended most IUSSP general meetings?

GLICK: No. The first one I attended was in 1954, in Rome. At that meeting, the Russians and the satellites all made the same speech: There is no population growth problem if you have the right political and economic system. It was just tiresome to hear one after the other. When we were out at the airport ready to leave, this one big fellow looked around and said, "When we meet again, we'll have another speech for you"--implying they knew what they were saying, but one does what one must.

At the IUSSP meeting in 1965 [held in conjunction with the UN World Population Conference in Belgrade], Ron Freedman told me that he had met a USSR ethnographer, Kozlov, who had an atlas that he wanted to get into the hands of somebody at the Census Bureau who might make use of it, so Ron had mentioned me. I met this fellow and he had this big book with a brown cover. We went over to one side of the room and we looked at some of the tables and he had little images to show how many ethnic people were in different parts of each country. I said, "Maybe I could send you some of our Census reports in exchange." He said, "I think we have most of those. What I'd like to have is some American science fiction books." His wife was teaching English. They had two sons, and I figured they were trying to get the sons to read more interesting material in English. We sent him some and Toby Bressler gave me some to send. We sent several times.

At a later meeting in London, 1969 I think it was, I didn't know he was there but he came up and gave me a big Russian bearhug and kiss on each cheek--he was about four inches taller than me,

big fellow--and said, "I'd like to talk to you." My wife was with me, so we went to another room and he says, "There is a book by Kingsley Davis that costs \$25. They wouldn't let me bring much money out of the USSR. Could you help me?" I just reached in and gave him a ten-pound note, which at the time was worth \$25. No questions asked.

The next day at a session he caught up with me and gave me all kinds of gifts. He had a set of coins and a set of stamps from the USSR for my sons and beads for my wife and tie clips and cufflinks for me. He was so grateful. Before the day was over, I said, "Let's go around the corner and have something to drink." Which we did. While we talked, he looked around one way and the other and didn't see anybody and he says, "You know, my country does a lot of things that I don't agree with, but it's my country so what can I do?" I never heard of him again after that.

Not too long ago, I told this story to someone else and he said, "You should have said, 'My country does a lot of things that I don't like. What can I do about it; it's my country.'"

VDT: What do you consider leading issues in U.S. demography over the years you've been involved?

GLICK: Well, obviously the baby boom and all of its repercussions. I was asked to testify before Senator Mondale about some legislation proposed by a commission on children. Barabba was the new Census Bureau director then, so when he didn't know the answer, the questions were shunted to me. When the list of questions was finished, Mondale turned to me and asked, "How long have you been at the Census Bureau?" I said, "Over 30 years." He asked, "Tell me in your own words what you consider the main things that have happened in demography during that time." I wasn't prepared, but I did the best I could for a couple of minutes. Obviously, the baby boom and the aftermath of that were high on the list. Also, of course, the delay in marriage, increase in divorce and cohabitation, the delay in remarriage--all these things that are written about extensively were of topical interest, of importance. I felt a duty to write about them in an understanding way. Not that I ever said anything bad, but I like to tell it like it was.

About four years ago, I was asked to make a speech before 250 Catholic bishops in Dallas. They were from the U.S., Canada, and Latin America. They had a couple of nuns in the back of the room translating into Spanish what was going on in English. The man who spoke before me was straight out of the Pope's office, said all the things that the Pope says, that most demographers don't agree with. And I was asked to talk about changes in fertility, in family life, divorce and cohabitation, and the like. Later the bishops told me it was useful to them to have updated, factual information and not just pronouncements from the Pope's office. They asked this Pope's representative questions, "What if a woman has a baby by incest or by rape?" This Catholic spokesperson said, "Every birth is an act of God. Nothing should be done to interfere with life." Anyway, those are some of the kinds of experiences I've had.

VDT: Great. Now let's turn to PAA. Do you remember the first meeting you attended?

GLICK: Yes, 1940. I went to the Census Bureau in July 1939, so I missed the Washington, D.C., meeting [May 1939]. I went in 1940. Dr. Truesdell was the president of the Association that year. That meeting was in Chapel Hill. I've never missed any meetings after that.

VDT: From 1940?!

GLICK: Well, let's see [looking at meeting list]. I never missed a meeting until I came to ASU in 1982. They were having a meeting in San Diego at the same time as I was moving and I couldn't go. All the rest of the meetings I have been to.

VDT: From 1940 you have missed only one! I think you must hold the record.

GLICK: And I have the same record with the American Sociological Association. I missed one in 1986 in New York City that I could have gone to. I had been asked to give a paper at a meeting in New Delhi, India. That's halfway around the world, so I decided I might just as well go right around. I could have arranged to attend the American Sociological Association meeting in New York at the end of my trip, but I thought I would be tired of attending meetings, so decided, "I'm going to spend my last three days in Geneva, Switzerland." And I did. I can't remember any other ASA meeting I've missed.

VDT: Amazing! Can you give me a quick idea of the early PAA meetings? Of course, many people have told me about the small intimate group that usually met at Princeton. The ambiance was very different from what it is now.

GLICK: I remember one time, maybe it was 1941--who was president in 1941? I think it was Pat Whelpton.

VDT: He was president in 1942.

GLICK: Yes. In 1941 he came back from the meeting and my superior Richard Lang asked who was elected president for the next year and Pat said, "I was," in a very casual way. He didn't make a big thing out of it.

Pat was quite a man. Of course, I had a lot of dealings with him through the years. He was at Scripps [Foundation for Research in Population Problems]. He made all these projections of the future U.S. population. It was supposed to start going down about 1960. And as the birth rate began going up, he said, "Well, it's just temporary. It will just be for a little while." He had worked so hard, cross-classifying all the characteristics and projecting them, it just had to be right. That was a sad thing. He finally had to say, "I guess you just can't predict under certain circumstances." At the PAA meeting in Cincinnati in 1953, Phil Hauser made a statement about if you can accurately predict when the peak and the trough of trends are going to happen, you could become a rich man.

The next time the meeting was in Cincinnati, I was president. That was 1967. We could not go into the South because blacks couldn't go into the same hotel as whites and Cincinnati was as close to the South as we could go, so I helped to arrange to have it there. A secondary reason was it was near my home where I grew up, in southern Indiana.

VDT: Tell about your time as secretary-treasurer, 1962-65. You've written a wonderful vignette about the Committee on Organizational Management ["PAA Committee on Organizational Management: 1966-67," *PAA Affairs*, Summer 1982], the big issue that flared up when you were president in 1966-67. But can you recall issues that were important when you were secretary-treasurer? You are, I think, the only one who's been secretary-treasurer and then immediately president. You just said that you ran against Ansley Coale for president.

GLICK: Of course, I had been second vice-president and first vice-president before I was secretary-treasurer. So there was a lot of personal exposure.

I worked hard. I remember Chris Tietze helped in saying at the end of my time as secretary-treasurer that the members were very grateful for my good work during those three years.

VDT: The secretary-treasurer's work was beginning to increase enormously because the membership numbers were increasing with Don Bogue's promotion of Demography [first published in 1964].

GLICK: Another of my bits of humor. All the money from all the dues came into my office and my secretary said she felt it was like her money she was carrying downstairs to the bank in the basement.

Yes, it was getting burdensome. I figured about 20 percent of my secretary's time was going into the work on that. We had to make announcements and programs and so on. I had to arrange for the printing and the like.

Then Andy Lunde was the next one [secretary-treasurer 1965-68] and the last one who had this burden at the expense of a government agency. Finally, they decided to negotiate with the American Statistical Association to take care of such logistics.

VDT: That partly came out of the recommendations of the Committee on Organizational Management that you set up during your presidency. But they went beyond simply recommending that there should be a paid business office. The other proposals, that you wrote about so well in your vignette, included active recruitment to expand the membership, establishing professional standards for PAA membership, including two classes of members, and a small grants research program. And it was all shot down by Ansley Coale [incoming president] and the membership.

GLICK: Yes. Well, I did my duty by getting that committee together to make this appraisal of future directions of the organization. When I went in as secretary-treasurer, there were about 600 members [660 in 1962]. But by the time I was president-elect and president, it was up to about 1,200 [1,283 in 1965; 1,375 in 1966; 1,435 in 1967]. One reason was Don Bogue got in the act and sent out 30,000 announcements of PAA's benefits and he got oodles of people to join the organization during those years.

VDT: So that's how he built up his subscription list to Demography?

GLICK: Yes, the new journal, which Cal Schmid [president 1965-66] and I got money for.

VDT: You got a \$30,000 three-year grant from the National Science Foundation [after the first year of publication].

GLICK: Yes, and we had to work out some figures to show how, when the three years of the grant were over, we could take the load on from there, would jack up the dues a certain amount. Actually, I think they didn't quite use all of it in three years and they stretched it out another year or two and then they phased it out. Lived happily from there on out and haven't had any money problems, I don't think, for many, many years.

VDT: Of course, there was Don Bogue's infamous last issue ["Progress and Problems of Fertility Control Around the World," special issue of Demography, Vol. 5, No. 2, 1968], the big thick white one with the inverted Indian family planning red triangle on it, that was violently objected to. But he paid all the bills for that from his Community and Family Study Center.

GLICK: We had a bit of a problem from time to time with Don Bogue when he was editor. And then our executive committee would meet with him and tell him how we thought things should be done. He would argue some and then he did it the way he wanted to do it. As long as he was paying the bill, what could we say? But he gave us a good start. The first editor after him was Dudley Duncan's wife

Beverly. She was my pick for that. I thought she was a businesslike person and would do a good job. And she did.

VDT: What do you think of Demography as it's developed over the years? It's a very sober and scholarly publication.

GLICK: Well, it's done fine. The only problem is--I haven't published anything in there for a number of years simply because of the kinds of statistical analysis one has to do to get published in it. I remember even back in the time when Charlie Nam was editor [1972-75; following Beverly Duncan, 1969-72], that was when they were beginning to get these high-powered statistical procedures into the articles. He said, "I always put those articles at the end of the issue." I liked that. I guess I'm just too old to start learning all that. I heard somebody make a speech here at ASU the other day, a retiree from the faculty. He said the same thing.

I still don't have too much trouble getting stuff published. I don't publish in Demography or American Sociological Review or in the American Journal of Sociology where I used to. But I publish in the family journals and I have very rarely, if ever, had one rejected by their editors. As a matter of fact, I've had four articles published by them in the last two years and another one coming up. I published one last November called "Fifty Years of Family Demography." 1988 was the 50th year of the National Council on Family Relations and I was asked to be one of four authors of special articles about developments during those 50 years. They have four issues a year and there was one special article in each. Mine was in the November issue.

VDT: What's the name of their journal?

GLICK: Journal of Marriage and the Family. You probably wouldn't know any of the other three journals. They're mostly for family sociologists, home economists, and family counselors.

VDT: Back to PAA. Could you tell me about Kurt Mayer, who was secretary-treasurer just before you [1959-62]? He's one I will not get to interview in this series; he lives in Switzerland [but see Mayer's "self-interview" of April 1990, above].

GLICK: I didn't know too much about him. But I remember when I was about to follow him as secretary-treasurer, I went to a meeting where he was giving his finale and he gave me a check--all the money that belonged to the Population Association--and I carried it in my pocket even to De Pauw, where my son was a student at the time. It wasn't a lot, \$350 or so. He had apparently done a reasonably good job as secretary-treasurer. PAA was in the black, but only three or four hundred dollars.

VDT: And Cal Schmid [president immediately before Glick]. He is again someone whom I will not get to interview.

GLICK: Cal Schmid once said he regarded me as one of his best friends. He was at the University of Washington umpteen years and the father of census tract statistics in that area for many, many years--and in the U.S. too. He would go to Hawaii for a spring holiday. My brother Clarence is out there, professor of sociology at the University of Hawaii. Clarence is retired now. I talked to him on the phone just two or three weeks ago when I came back from Australia and New Zealand. We had a fuel stop in Hawaii.

Cal Schmid attended many meetings of our Census advisory committee on population statistics,

something the population division carried on for years. I was the coordinator for about ten or 15 years before I retired. Cal was well advanced in census tract technology. That's the main thing I can remember about him.

A little footnote. There came a point when Howard Brunsman was offered a candidacy to be president-elect of PAA. Irene Taeuber and I were on the nominating committee and we offered it to him. He said, "I have some other things I want to do" and he turned it down. So we offered it to Calvin Schmid--might have done it anyway, but that's the way it worked out.

Calvin Schmid was president the year before I was and he did something that may make you amused at what I say. My last act as secretary-treasurer was to send out a six-by-eight card to each PAA member with questions about their background and professional experience to be returned when they sent in their dues. I had in the back of my mind that if I ever got to be president, I would like to use that information for my presidential address. But Cal Schmid asked me if he could have that for his presidential address ["Some Remarks Concerning Contemporary American Demographers and Demography," Population Index, October 1966]. And he did use it and he did it very effectively. He also got out a nice little brochure with all the statistics about the membership. He knew I had collected this information. It didn't occur to him that I might want to publish it myself. Anyway, we've been good friends ever since.

VDT: It seems to me that the Census Bureau and other federal government demographers used to be more prominent than they now are in the PAA leadership, especially among the presidents. Now they're almost all academics, though the secretary-treasurers seem to have been mostly non-academics. What do you think accounts for that? Well, you had your little coterie in the Census Bureau when you first went there.

GLICK: You know how many departments of demography there are in the United States? Darn few. I don't know the answer, but I know that there aren't very many and demography is always sort of a tag-on of sociology. That's one thing we found from that study. About 60 some percent in the PAA are sociologists, then some 30 percent or so are economists, and the rest trail off.

VDT: That was in the mid-1960s.

GLICK: It probably isn't far from that today. I remember the Footnotes of the American Sociological Association about four-five years ago published a table showing how many requests they had received for jobs teaching certain subjects and how many were offering to teach certain subjects. The number offering to teach demography was very small, but the number asked to teach demography was at the top of the list! What I'm saying is demography is new, relatively speaking. And now it comes more in the vein of academia than it does in the government and elsewhere.

VDT: What do you think of PAA meetings nowadays? You continue to come--you don't want to break your record. But now we're up to 84 sessions, eight overlapping, Saturday afternoon sessions, all these splitoff workshops, and over 1,200 people at the last meeting in Baltimore. Do you still enjoy them?

GLICK: Just to illustrate the strength of my interest. I was all set to go on this trip to Australia and New Zealand on the third of April, but the PAA meeting in Baltimore ended on April first. I went to the meeting for the first two days and then I came back early. But I wanted to be there for that Wednesday night bash, which is one of the best parts of the whole meeting. And then, of course, all the other chances to see and talk to people. It's always a great treat. There's getting to be a little

skimpier number of people of my vintage who are there. In fact, there are hardly any demographers of my age at the ASA meetings anymore.

VDT: Well, I hope you will go on and on. Our meetings couldn't go on without you.

You've just pointed out that demography is rather a new field. It's still rather elitist. There are just about 2,600 members in PAA [2,679 at the end of 1989]. Of course, 1,200 people--not all members--attend the meetings. The membership has been stuck about that number since the mid-1970s, which is far smaller than the ASA, for instance.

GLICK: I don't see anything wrong with that. When I was president of the National Council of Family Relations in 1979 they had 5,500 members; now they have 3,700 members. It actually declined. It's just that there's been a slacking off. I think the American Sociological Association doesn't grow much anymore. People have other ways to spend their money. They have, goodness knows, a myriad of other associations and demands on their income.

It doesn't bother me if the PAA is in a sort of stagnation. I think that it constitutes a significant body of intellectuals who are interested in an important field. As a matter of fact, you're not going to see the college enrollment go up much anymore because of the decline in the birth rate. So what's wrong with that? I've heard speeches along that line: Don't worry about zero population growth.

VDT: I was going to ask that: What do you see as the outlook for U.S. population trends? You've just said don't worry about zero population growth. Are you still an optimist that marriage is here to stay, despite rising cohabitation, divorce, out-of-wedlock births?

GLICK: I could refer you to an article that was published in the December 1987 issue of the Journal of Family Issues, where the editor asked 16 of us to write what we thought of the trend in family life. He said, "Be brief, off the cuff, informal." I wrote a three-page item, which I entitled "A Demographer Takes Another Look at American Families." The title was going back to a paper I had read at a meeting of the National Council on Family Relations in 1974, entitled "A Demographer Looks at American Families." The journal editor analyzed the small papers. He classified them as either sanguine or concerned about the trends. I was one of three classified as still sanguine.

What do you expect? We're not back in the 1920s. Other things have changed so much, why shouldn't family life change? You may not like some of the consequences, but there are tradeoffs. You have obviously oodles of benefits from being able to control the size of family. And it may not be long until you can control the sex of offspring close to 100 percent. I just think it's a shame that people have to go through life without having the sex of children they like.

I wrote a paper which was presented on Friday the 13th of January, 1989, called "American Families: Where They Are and Were." It was given in Sarasota, Florida, at the first of a series, presumably, of conferences, organized by Evelyn Duvall. She had me give the number one speech. My finale in that paper was something to the effect that there are those who think the trends are just a consequence of social changes and something to live with and others who think it's pretty bad and something important should be done about it. I said I thought there's much to be said on both sides. That's the way I have written from the beginning, a sort of balanced view. I don't take sides on issues. I just try to describe what the facts are on either side, maybe more so than to interpret them in terms of norms. Many people are so critical of the family trends that if you say anything otherwise, you're put in the sanguine pigeonhole.

But I said that if I were to recommend something, it is simply to exert effort toward supporting people in whatever type of family they are, whether they're married, single parent, alone, or whatnot. Just take the situation the way it is and do the best we can with it. And if divorce declines, I'll be

perfectly happy with that. If cohabitation declines, I'll be happy with that. I'm not out to stump to do anything to change that or any other aspect of the situation.

VDT: Great. Obviously, your own work now, continuing to write and publish and be asked to give papers, is all bound up with the question I asked: What do you see as the outlook for U.S. population trends and the family? You've told me you don't plan a new book, but you're still very much in demand for meetings and papers.

GLICK: I may have pretty much reached a turning point. I think that I'll be writing much less from here on out. I find myself frequently these days going to meetings and not even offering to write a paper. But that's okay; I haven't run out of invitations to write papers. I still have a research assistant, who is working on a project which I'm not sure feminists would like but it appeals to me. That is what are the characteristics of women who are employed in so-called men's occupations and what about men employed in so-called women's occupations. My research assistant has done some tabulations that begin to show what the results might be. Primarily with respect to their marital and family characteristics, naturally--that's my pet topic. I'm using the tape from that National Survey of Families and Households, which has a long list of topics that I want to relate and see how chosen characteristics apply to these 8 or 10 percent of men who are in women's occupations and 8 or 10 percent of women who are in men's occupations. Many of the others are in mixed occupations, but the majority of men are employed in traditional men's occupations and the majority of women in traditional women's occupations.

VDT: Still a new question! That's great. You've had a really fine career and life. And you are making some time for travel. Are you going to the IUSSP meeting in New Delhi in September?

GLICK: I should say not! I was at the international sociological meetings in Delhi in 1986 and that is such a bad place to breathe and live that I don't want to go back. It's sultry and uncomfortable, especially in August when I was there. One of the main reasons I accepted was to go and see if the Taj Mahal is as beautiful as it's pictured to be. And it is.

VDT: Have you got some final things that you would like to add? You said, for instance, you had prepared a list of little humorous touches.

GLICK: I have sort of worked them in as I went along. I thought there was no reason an interview like this shouldn't have some interesting spots. Maybe I have my maternal grandfather to thank. He was born in Northern Ireland, a Scotch-Irish man, and the Irish are supposed to have a sense of humor. And I always think of myself as having a sense of humor and I try to work those things in.

VDT: Sense of humor, yes. The Irish are not always noted for their optimism about life, but you certainly have that.

INTERVIEW CONTINUES

VDT: We're just talking about the birth rate in the U.S. It now rounds off to 1.9. It's been 1.8, so it's up a notch.

GLICK: The latest reports from the National Center for Health Statistics show the birth rate up a small amount. But they haven't yet published this information by age and that would show, I hear, that

the people in their thirties are coming along with their delayed births. The 17-, 18-, and 19-year-olds are a small crop these days and they're not producing many births.

VDT: I interviewed Richard Easterlin a few days ago and he still stands by his prediction that because the baby-busters are going to have it much easier than the baby-boomers did when they get to working ages, they will have higher fertility. But I don't think that's what accounting for our little upturn so far.

GLICK: I have said that I think the change in the direction of the marriage squeeze may make for a bit more stability too, because in those times when women are scarce, historically, they've had more family stability. When they've been in the excess, you've had more instability. There's a book called Too Many Women. It goes way back into history and it supports the same general idea.

VDT: And right now we're in a period when because of the decline in the birth rate 20 years ago, the baby bust . . .

GLICK: We have a scarcity of young women now.

VDT: More men than women in the marriageable ages. The men are born a few years ahead of the women.

GLICK: An excess of men, yes. So this should be a good time for women . . .

VDT: To find a man and stick with him. Okay! Thank you.

ANSLEY J. COALE

PAA President in 1967-68 (No. 31). Interview with Jean van der Tak in Dr. Coale's office at the Office of Population Research, Princeton University, May 11, 1988, with excerpts from Dr. Coale's interview with Anders Lunde during the PAA annual meeting in Philadelphia, April 27, 1979.

CAREER HIGHLIGHTS: Ansley Coale was born in Baltimore, Maryland. Except for four years during and after World War II, he has spent all of his career at Princeton University, beginning as an undergraduate. From Princeton he received the B.A. in 1939, the M.A. in 1941, and the Ph.D. in 1947, all in economics. He has also received honorary degrees from the University of Pennsylvania and the Universities of Liege and Louvain in Belgium. From 1947 to 1954, he was Assistant Professor of Economics at Princeton, before returning to demography and the Office of Population Research, where he had been the second Milbank Memorial Fund Fellow and a research assistant before his wartime service. He was the second Director (following Frank Notestein) of the OPR from 1959 to 1975 and Professor of Economics from 1959 until 1986. Since 1986 he has been Professor Emeritus at Princeton, but continues his research and writing at OPR. Among his other posts, he has been U.S. Representative to the United Nations Population Commission (1961-67), President of the International Union for the Scientific Study of Population (1977-81), and Chairman of the Committee on Population and Demography of the National Academy of Sciences (1977-82). He is a member of the National Academy of Sciences and winner of both the PAA Mindel Sheps Award in Mathematical Demography and Demographic Methodology (1974) and the PAA Irene B. Taeuber Award for Excellence in Demographic Research (1989).

Ansley Coale is famous in the field of demography for his innovative work in both the mathematical aspects of population and the social and economic implications of population structure, growth, and change. In addition to many articles, his influential publications include such monographs as Population Growth and Economic Development in Low-Income Countries: A Case Study of India's Prospects (with Edgar M. Hoover, 1958), New Estimates of Population and Births in the United States (with Melvin Zelnik, 1963), Regional Model Life Tables and Stable Populations (with Paul Demeny, 1966), The Growth and Structure of Human Populations (1972), and two monographs in the Princeton European Fertility Project which he initiated in 1963: Human Fertility in Russia Since the Nineteenth Century (with Barbara Anderson and Edith Harm, 1979) and The Decline of Fertility in Europe (edited with Susan Watkins, 1986).

VDT [from biographical introduction]: You retired from the Office of Population Research in 1986, I understand, but here you still are, having just ridden in on your bicycle, which do every day. Do you also play tennis every day? Charles Westoff told me yesterday that he plays tennis every day.

COALE: Well, no more than six or seven days a week. Sometimes it rains or my opponent cancels out.

VDT: That's at lunch time, between riding in and out on your bicycle. How far is it to ride?

COALE: Counting the ride over to the tennis courts and back, I do five miles a day--on an old Sturmey-Archer three-speeder.

VDT: That means more exercise.

COALE: I never got adjusted to the ten-speed.

VDT: When and how did you first become involved in the field of demography?

COALE [This combines Dr. Coale's answers in 1979 and 1988]: I started in population for reasons of what you might call cupidity. I was an undergraduate at Princeton. I had no firm idea of what I wanted to do. I wasn't even sure that I wanted to go to graduate school. I didn't have a job and it was in the Depression. My father was a Presbyterian minister. As I recall, it cost my parents something like a total of \$200 for me to go to college. Things were different in those days. The tuition and fees for the library and so on totaled \$550 for a year. I had a scholarship that was worth \$625, so that paid my tuition and fees, with \$75 left over. My room, which was part of a two-bedroom unit with a living room--you would call it a suite now--cost \$125. So I needed only \$50 more than my scholarship to pay for tuition and room. I waited on tables in the common; 11 meals a week, it turned out, was enough to pay for my board. I was self-supporting from the end of my freshman year. I was not in a position to ask my parents to support me in graduate school.

I went to see the dean of the graduate school at Princeton, who was also chairman of the math department, and said I was toying with the idea of going on to graduate school. When I entered Princeton as a freshman, I had intended to major in mathematics or physics, which both fascinated me. My switch to economics at the end of my sophomore year probably had something to do with the salience of economic problems during the Depression. I told the dean I'd like to go to a place where I could combine mathematics and economics in my graduate work; mathematical economics didn't really exist as a discipline in 1939. He said, "Well, Harvard and Michigan have good mathematics and good economics. But probably the best place would be to continue right on at Princeton."

Not long after that, he called me in and told me that Frank Notestein had become director of the Office of Population Research two years earlier [in 1936] and the funding of the Office included a graduate fellowship provided by the Milbank Memorial Fund for an economics Ph.D. candidate interested in population. It paid tuition and living expenses. I had no money, so the only way I could go to graduate school was with a full ride. I was willing to have an interest in population in order to have a fellowship that would pay my way. In those days, one had to qualify in five different fields within economics for the general exam to qualify for the Ph.D., and demography could be one of those fields. All I had to do to get this fellowship was to agree to make demography one of the fields.

That's how I got started. I became the second Milbank Fellow, after John Durand. John and I overlapped for a while. I did get very interested in demography. I took my general exams for the Ph.D. in economics, including demography, in the spring of 1941.

[On OPR before the war, from the 1979 interview with Anders Lunde]: The OPR when I joined it consisted of Frank Notestein and his secretary, Henry Shryock as research associate, and John Durand as a graduate student, and that was it. Dudley Kirk joined the Office while I was the Milbank Fellow. I left out a crucial person--Irene Taeuber. I didn't think of her because she always worked in Washington. She was editor of Population Index, raising her two children and writing about 20 articles a year. She used to send up slips with the abstracts of the titles that were to appear in Population Index. In those good old days, the Milbank Fellows had to proofread all the copy of Population Index four times a year. So I would sit with John Durand and Dudley Kirk and we would proofread. It went on to the secretaries who typed it for photo offset reproduction. That was how it came out.

Subsequently, while I was there, the Office started to work on a series of books for the League of Nations, starting with The Future Population of Europe and the Soviet Union. Different people came to work on that, including Wilbert Moore. Frank Notestein was not only director of the Office but he also taught the two graduate and undergraduate courses on population. In fact, we continued to

offer only two courses at Princeton up until about 1970.

LUNDE [from the 1979 interview]: Population Index, was that supported at the time from outside?

COALE: It was supported partly by the Milbank Memorial Fund, which also gave the handsome sum of \$10,000 a year for the Office. From the beginning, it was the essential organ of the Population Association of America. Part of the dues of the members went to Population Index.

LUNDE: I'm interested in what you say about Irene. I asked her once, "How in the world did you get all of this done with the kids in the house?" She said, "Well, with card tables in the living room, a card table for each job." And the kids were told, "Stay away from the card tables." She must have been well organized as a person.

COALE: She was extremely productive and scholarly. In addition to raising a family, she prepared the entire bibliography of Population Index for many years, wrote hundreds of articles, and several books, including the encyclopedic Population of Japan [1958].

LUNDE: What was Dudley Kirk like in those days?

COALE: A good friend and productive scholar, who sometimes had trouble meeting deadlines.

LUNDE: Maybe you ought to put into the record your discussion with me a minute ago about what the heck is happening to the money for this year's Irene Taeuber Award [for Excellence in Demographic Research].

COALE: Well, it's true that Dudley Kirk upbraided me. He said, "Did you bring a check for \$1000?" The Irene Taeuber Award was funded by contributions that went to Princeton, which agreed to hold the money and provide from it \$1000 for the award every two years. Dudley has been chairman of the committee deciding who should receive it [in 1979]. So he asked me, "Do you have the check for \$1000?" I thought, my God! Am I supposed to have it? Dudley then told me who the recipient is.

This morning, after feeling guilty all night, I realized that I couldn't have had a check for \$1000, since Dudley had not told me the name of the winner! So we'll have to give the winner a promise to send him the check later on.

VDT [continuing 1988 interview]: When you were starting graduate work, did you get your master's as a Milbank Fellow?

COALE: Well, Princeton has a snobbish attitude about advanced degrees. In the liberal arts and sciences, they will not accept a candidate for a master's degree. They don't have master's degree programs, except in the professions like engineering and architecture. What happens is that you complete the course requirements for the Ph.D., passing the examinations in five fields, plus language and mathematics and things of that sort. Then you are qualified to write a dissertation. If the department decides that you've barely passed the general examination and they don't think you're very promising, they will give you--as a booby prize, so to speak--a master's degree. You get it en route to the Ph.D. anyway. I remember that they charged \$35 for the M.A. diploma and I didn't want to take it; I figured I'd get my Ph.D. My mother said, "That's ridiculous." She came up with the \$35--and kept the diploma! There's no thesis for a master's degree. It is just a degree awarded to someone who has

fulfilled all the qualifications for a Ph.D. except the dissertation.

VDT: And you'd managed to do that within two years?

COALE: Yes, 1939 to 1941. That was fairly standard in those days, and I still think it's the right thing. I think that in two years you can do enough work in a field to master the basics of it. I sort of regret the four years of course work and two more years on a thesis common today. I don't think the students are coming out any better qualified after spending five or six years than they used to in two, plus one for the thesis.

VDT: You think three years beyond the undergraduate degree is enough for a Ph.D.?

COALE: The best students that I've trained, who got Ph.D.s under my supervision, have done it in three years from the B.A. People like Sam Preston.

Alvaro Lopez, from Colombia, had gotten his degree in some kind of engineering from the University of the Andes and then went to the Sorbonne and studied mathematics. While there, he got in some kind of exchange with Alfred Sauvy. When he was in Washington at the Colombian embassy, Frank Lorimer encountered him. They discussed his interest in population and Frank said he ought to apply to Princeton. He came here as a visiting student and decided to become a degree candidate in economics. He had never had a course in economics before in his life. He took his general examinations in mid-year of his second year. He never had a course in which he didn't get the top grade. Between the first of February and the end of May in his second year, he finished his dissertation, so he received his degree in two academic years. The dissertation was published by September, by the second anniversary of his arrival. He was the most brilliant student we've ever had.

VDT: His dissertation--remind me--was seminal in your work?

COALE: That dissertation [Problems in Stable Population Theory, 1961] was a proof of a conjecture that I'd made to the effect that all populations, not just stable populations, have an age composition that is independent of the remote past. It depends only on the fertility and mortality in the recent past, say, the past century or so. Two centuries are enough to wipe out the effects of the past. In explaining it to students, I say that the age distribution of France is no longer much affected by the Napoleonic wars. Lopez devised an extremely subtle proof and explored other implications in addition to proving the proposition in his dissertation. He did his degree in two years altogether because he was a genius.

Sam Preston spent two years before his exams and one more year for his thesis.

VDT: You could tell right away that he was also exceptional?

COALE: Exactly. I shouldn't say it, but of the 50 presidents of PAA [through 1987], 16 have been either students or staff at the OPR. Many of these students spent no more than three years on their Ph.D. More of them finished in three years than took more than that.

VDT: Of course, your own Ph.D. completion was interrupted by the war. In the two years from 1939 to 1941, you did not go beyond your generals.

COALE: The generals were in 1941, so the period between my completing the generals and going into the Navy was just between June of 1941 and the first of February, 1942. I had a low draft number and I knew I was going into the service one way or another. So I thought there was no point in trying to start a dissertation, because it was sure to be interrupted by my going into the service. So, instead of

that, I worked as a research assistant at OPR.

VDT: You were working on projections of European populations?

COALE: Yes; in fact, I designed them. During that time, the Economic, Financial and Transit Section of the League of Nations left Geneva and came to Princeton; they were given headquarters at the Institute of Advanced Study. They negotiated an agreement with Frank Notestein that the Office would undertake studies of the population of Europe, trying to create a picture of what the populations of the different countries were likely to be at the end of the war, so when they had the next postwar treaties, they would at least have a pretty good idea of what populations they were working with.

The Office undertook several books. The first was The Future Population of Europe and the Soviet Union [by Notestein, Irene Taeuber, Dudley Kirk, Coale, and Louise Kiser, 1944]. Another was The Population of Europe in the Interwar Years [by Dudley Kirk, 1945] and the third was The Economic Demography of Eastern and Southern Europe [by Wilbert Moore, 1945]. The fourth was by Frank Lorimer, The Population of the Soviet Union [1946], who wrote it in Washington but for the Office.

The first book was organized around population projections through 1970 for every country of Europe. My contribution was to work out a general method for projecting each one of the European populations and to supervise the actual calculation of the projections. I did that part of the work and drafted part of a technical appendix describing the projections and had nothing to do with the rest of the book.

VDT: Those projections turned out to be somewhat wide of the mark, or low, because--as you pointed out in the paper ["A Reassessment of World Population Trends"] that you wrote recently for the United Nations Population Bulletin [No. 14, 1982]--you didn't anticipate the rapid fall in mortality, even in the developed countries, in Europe.

COALE: Right, indeed I did not. There was a discontinuity in mortality, and also there was the baby boom in Western Europe and the United States. However, the fertility and mortality that we projected for 1970 is not so far off the mark, because the fertility decline has resumed. So it sort of went back onto what we were projecting.

VDT: Then during the war you had a very interesting project. You taught electronics at MIT?

COALE: It was radar. What happened was that in 1941 there was a roster of scientific and specialized personnel that had been circulated to universities by the National Research Council. I didn't realize then that this was the operating arm of the National Academy of Sciences. As a graduate student, I'd filled this thing out and listed the substantial amount of statistics I'd taken as my technical background. After Pearl Harbor, I knew I was going to be called into the service right away. I went down to Washington to see the National Research Council, which, as I say, was the NAS. The man whose office I entered turned out to be a former professor of physics at Princeton. I said I thought I would be more useful in the war effort doing something that capitalized on my training rather than serving in the infantry. I explained that I had passed my preliminary exams for the Ph.D. in economics and had a lot of statistics. He said, "We don't know anything about the social sciences here; this is a natural sciences organization. Do you know anything about physics?" I said, "Yes, as a matter of fact I started out to major in physics and I continued to take courses in physics as electives." I told him the courses I'd had; I realized by then he'd been at Princeton so he knew what they meant. He then asked me if I knew anything about radio and I said, "When I was in high school I built several radios as a

hobby." He said, "Do you think you could handle a course in radio engineering?" I said, "I'm sure I can." I'd had such courses as the functions of complex variables and other subjects that are directly pertinent. He picked up the phone and called across the street, from the National Academy of Sciences to the Navy Department, and said, "I've got a candidate here who ought to fit into the radar program." He sent me over to the Navy commander who was in charge of that program, who said they would give me a commission as Navy ensign, as an officer specialized in electronics. I would go to Bowdoin to study radio engineering and then on to MIT to study radar. These were programs that the Navy was supporting.

I was sped up. Instead of going to Bowdoin on April 1st, I went to Harvard on March 1st for a pre-radar radio engineering program for three months. It was very intensive. In three months of six days a week of only one subject with labs and so on, you learn a lot--equal at least to a full academic year. Then on to MIT. MIT was training all of the Army and Navy officers who were going to be in charge of radar all over the world--maintenance, installation, operations. It was a combined Army-Navy school. In addition to the civilian staff, they took qualified people from the services to teach. I went through the three months of training and did well. They kept me on as an instructor and thus I ended up teaching radar.

VDT: You took the training course at Harvard and then switched to MIT?

COALE: It was a sequence. Harvard gave pre-radar radio engineering, without security clearance. They were just teaching the fundamentals of radio engineering. MIT was teaching the operation and maintenance of radar sets, which were just being designed. As I stayed there for three years, the composition of the course changed altogether, because new sets kept coming in. We had to master the sets from the instruction books and teach it to these officers who were responsible at Navy yards and on big ships and so on for the operation and maintenance of the radar sets for the whole fleet, including airborne radar. I actually wrote part of a book called Principles of Radar. I really did learn a lot about electronics.

In the last year of the war, I went to the Radiation Laboratory, which was also under MIT administration. It was the big civilian laboratory that worked on radar. It was full of people who won the Nobel prize--Rabi and others of that caliber. It was an extraordinary place. I was assigned to the Navy liaison office, providing liaison between the laboratory and the Navy. Thinking back on it, I was really an emissary from the Radiation Lab to the Navy and not the other way around. My sympathies were all with those scientists, not with the somewhat unimaginative people in the Navy Department.

At the end of the war, I had an offer from Bell Labs to become an electronics engineer there. I decided that having once switched from physics into economics, I wasn't going to go back. So I decided to complete my degree and go back into the social sciences.

VDT: How was it that you were appointed to this committee that led to your writing of the book, Problems of Reducing Vulnerability to Atomic Bombs, that became your dissertation?

COALE: What happened was that, in an extraordinarily generous program, the Social Science Research Council gave a limited number of what were called demobilization awards. I was actually a sort of trial candidate for this. The idea was to prevent the loss from the social sciences of people like myself who'd gone into the service. I had a wife and two children; I was a full lieutenant in the Navy, drawing pay about equal to an assistant professor. It would be very difficult for me to go back to grad school. So they offered me this demobilization award, which gave to a person who needed to write a dissertation one year of full support at the same stipend that he'd been earning in the services.

I said, "Splendid." I was still living in the Cambridge area and started going to the Harvard

library to look around for a thesis topic. Then I got a call from the president of the SSRC, saying they were establishing a committee on the social implications of atomic energy and they'd like to have me be the secretary, on salary. It was going to last a year. I said I really wanted to get my thesis done. He said, "Come down and talk about it." I went down to New York and he said they would hold the demobilization award, that I would spend a year as secretary and then I could do the thesis. Professor Rabi was a member of this committee, a very distinguished group of both social and physical scientists. The bomb had just gone off; I had known some of the people involved at Los Alamos and was very fascinated by the project. I had read the Smyth report with special interest when it came out; Professor Smyth had taught me freshman physics here at Princeton. So I agreed to be the professional secretary on this committee.

The committee commissioned several studies by distinguished people on such topics as the problems of establishing international agreements to control weapons and the economics of the production of electricity from nuclear fission. Then they said, "There's another problem. If these agreements don't work, other countries will develop arsenals of nuclear weapons and all the advanced countries will live in fear of being decimated by these terrible weapons. What's that going to do to society?" People were talking about having linear instead of circular cities--a city strung out from Pittsburgh to Philadelphia so an atomic bomb wouldn't do too much to it. The committee said they didn't know what they ought to do about it, so they said, "Okay, you're the secretary, you decide what kind of action the committee might take in this field." So I sat down to do an outline that kept expanding and it turned out to be a book. It was called The Problem of Reducing Vulnerability to Atomic Bombs. When I finished a draft, the committee liked it and recommended that it be published, which Princeton University Press agreed to do. Then the committee was dissolved.

I got the idea of saying to the economics department at Princeton that one was supposed to write a Ph.D. dissertation on a social science subject and one requirement was that it was supposed to be publishable. I asked, "How about one that's already published?" They generously agreed to accept it if I wrote an additional chapter with some more economics.

VDT: Did you then have to have it redone in proper dissertation style, retyped and on the special paper? I have a son who's just finished a dissertation and we talked about this acid-free, archivable paper.

COALE: The extra chapter was on regular typescript. I had an idea. I went to the university store and bought two copies of the book. I took a papercutter and cut the spines off. I had to have two copies because the pages were printed on both sides. Then I sat pasting these to the center of pages.

VDT: It was literally just one additional chapter!

COALE: One of my graduate student colleagues came into the seminar room where I was pasting pages and said, "Writing your thesis, I see."

VDT: You were back teaching in the economics department by that time?

COALE: No, my appointment to the faculty at Princeton as an assistant professor didn't come until after the thesis was finished and handed in. It started in September 1947.

VDT: When I read about this being your thesis topic, in your 1979 interview with Andy Lunde, it seemed far removed from your subsequent work. But you brought it up again in an article you did for Population and Development Review in 1985 ["Nuclear War and Demographers' Projections," PDR, September 1985]. You were reminding demographers that their projections always assume that there

will be no nuclear holocaust, but actually the chance of nuclear holocaust is not zero. Why did you write that at that time, in 1985?

COALE: The reason I did it was that the address I gave to the IUSSP meeting in Manila in 1981, when I was outgoing president, was on world population trends. I mentioned a lot of ways of trying to decipher what the prospects were in fertility and mortality. In the closing section of that speech, I said that having written this book, I was aware of the fact that there was something in the human scene that wasn't encompassed by assuming a steady progression in fertility and mortality. The weapons that had been developed and were stored in the arsenals of the world were sufficient to kill a quarter of the world's population if there were unremitting war. More than a billion people might be killed.

Then scientists had come up with the idea of nuclear winter. The climate of the world might be changed and radioactivity of an unknown degree might jeopardize the remaining people. That was a discontinuity that wasn't in our normal thinking. I thought some acknowledgement of this possibility should always be made. The probability of war had, in my mind, been greatly reduced by the existence of these weapons, so that, for example, at an interval very much longer than that between the two world wars we had not had a real confrontation between the two major powers. Many occasions might have provoked it in a different weapon situation, but, given the existence of these terrible weapons, there was a restraint shown that had prevented a confrontation, because both sides realized they might be ruined by it. So the probability of a major war, I thought, was reduced. Not minor wars; there were plenty of them. The probability of a major war had not been reduced to zero.

One of the things you learn from statistics is that something that has a non-zero probability becomes inevitable if you wait long enough. Even a one percent chance that a war might break out in each year would make it inevitable within a couple of centuries. And I thought there was at least a one percent chance, given the possibility of a Colonel Quadafi, Hitler, or perhaps a Richard Nixon. There could be an irrational person in a responsible position who could trigger the catastrophe. Given that situation, I felt we had to acknowledge it--I didn't know what to do with it beyond that--the possibility that population projections would be totally out of line because of the occurrence of something of that sort.

It grew out of the fact that I had continued to think about it, having written that book.

VDT: Are you glad that you're not going to live through the next two centuries?

COALE: Oh, certainly, I'm glad for that.

VDT: You explained in the Lunde interview that from 1947, when you were appointed to the economics department at Princeton, to 1954 when you rejoined OPR, you did "other things." What brought you back to OPR?

COALE: It was sort of economics again--cupidity. Not quite, it wasn't so directly financial. The situation was that as a result of having written this book, I was in demand for the kind of thinking that social scientists and economists do about military or quasi-military things. I spent a brief time as a consultant to the Pentagon. Then the Rand Corporation was started. It was originally a project within the Douglas Aircraft Company and was moved out and made independent. It was an organization that then, and still, works on social science questions relevant to the military. It was supported by the Air Force--very imaginatively. At their organizing meeting, they said that part of their concern was how you prevent a war as well as other factors of that sort. I was made a consultant because I'd written this book. I spent the summers of 1951 and 1953 out there.

In the meantime, 1948 to 1950, I spent two years at the Institute of Advanced Studies here at

Princeton. The Social Science Research Council, jointly with the National Research Council, had offered a fellowship to allow social scientists to study the natural sciences, or vice versa. I applied, saying that I already had a fairly firm background in both areas and that I would like to study the economic implications of technological change, because I had professional training in both fields. That was a great mistake on my part. I tell my students not to do what I did. I was calculating that I had a comparative advantage, as we say in economics. There weren't many people who knew both technology and social sciences at a professional level. It was an opportunity for me to study the interaction. I was trying to capitalize on the circumstances, rather than having a brilliant idea that I couldn't resist following up. I wrote many drafts, but it never led to much. Also, I had a diversion in the work for the Rand Corporation.

I taught some courses here in the economics department satisfactorily, but I had published only a very few things when my six years were up. As a rule at the end of six years, if you're not promoted you're terminated. This is designed to prevent the exploitation of junior faculty, keeping a person on as assistant professor forever, paying a low salary for routine teaching. To prevent that possibility, you are either promoted or terminated. The economics department recommended that I be promoted to associate professor. The all-faculty committee, with the president and deans and so on on it, which makes the final decision, rejected that recommendation and said, "He has one more year"--a so-called terminal year.

That summer was my second at the Rand Corporation. I had a standing offer for a job at Rand at twice what I'd been making at the university. Charlie Hitch, who was then head of the economics division at Rand and subsequently president of the whole University of California system, offered me a job right then at Rand. I told him I was very attracted, but I was determined to go back and use my terminal year to return to the Office of Population Research doing demography, which I was pretty sure I could still do, to establish a firm professional base in academia. I had no intention of making a permanent career at Rand.

That's why I say that my return to OPR was kind of self-serving. I asked Frank Notestein if I could spend my terminal year at the Office. I did several good things during that year. I wrote an article estimating the undercount in the 1950 census by demographic analytical techniques ["The Population of the United States in 1950 Classified by Age, Sex, and Color--A Revision of the Census Figures," Journal of the American Statistical Association, 1955]. [From the 1979 interview]: When I did this work, it had an implication for the publications of the Division of Vital Statistics of the Census Bureau. It was this way. The vital statistics authorities had run a test of completeness of birth registration in 1940 and 1950 and had determined the extent of underregistration as well. By allowing for the degree of underregistration for different categories of births, they were able to estimate the changing deficit and give, for every year, a corrected number of births, from which they calculated the birth rates. What they realized from my work was that by making adjustment for 2 or 3 percent underregistration and then dividing through by a population figure based on the census, they were overstating the birth rate, because they were adjusting only the numerator and not the denominator of the rate. [Continuing with 1988 interview]: I developed an outline for a study of population growth and economic development in low-income countries. I had a couple of other things well on the way. So Frank went to the other members of the economics department, where he was as a professor, and proposed that they reopen the issue. This time my tenure appointment went through.

VDT: Thank goodness. You were saved for Princeton and demography.

COALE: When I came back to Princeton in 1947, I had said instead of going into demography, I was going to do other things. Notestein said there was a tendency for his students to do that. Demography isn't a department; you're a student in sociology or economics. It's like choosing labor economics

instead of international trade or something; it's a field, not a department. What he said was that a lot of his good former students declared independence by entering one of the other social science fields instead of demography. I had done that.

The particular mistake that I warn my students against was trying to capitalize on certain skills I had, rather than being enthralled by an idea that I couldn't resist following up. The latter is the way to do good research. If you find something of that nature, never mind what field it's in, pursue it. Demography has features that suit my temperament and I just failed to understand that. I still regret it. I think those were seven years when I could have done a lot. That's the age when you're normally most productive.

LUNDE [from the 1979 interview]: After the war when you wound up again at Princeton, how did you find the Office of Population Research then?

COALE: When I first came back I had only a kind of social connection with it, because for the first seven years I turned to other fields to investigate. Of course, Frank Notestein had always been one of my best friends. I visited the Office. It had grown and Kingsley Davis and Wilbert Moore had joined the staff. They became the nucleus of the department of sociology. First there was a program within the Department of Economics and Social Institutions. During the 1950s, this program broke off and formed a department. The Office of Population Research had brought these very distinguished personalities to Princeton and they became the nucleus of the Department of Sociology.

Kingsley came to Princeton in 1946 or 47 [in 1942-44 as a visiting research associate and 1944-48 on the faculty]. It was at Princeton that he wrote his monumental book on India [The Population of India and Pakistan, 1951]. It's been reprinted and it's been a continued seller as a reference work on the population of India. It's quite different from the kind of work one would ordinarily associate with Kingsley. He's an original and insightful social theorist. This book does not lack insights, but it is conspicuously factual and historical--a monument indeed.

The Office of Population Research remained small, moved around into various rooms on the campus, and finally in the late 1950s, it moved into 5 Ivy Lane, where it remained for about 20 years. The building was an eating club that had gone bankrupt and the university had taken it over. It was the first time the Office had ample space under one roof. It just gradually grew, taking on more research projects and staff to carry them out.

[On his work after rejoining OPR, from the 1979 interview]: I think probably the most influential writing with which I was associated was the book that Ed Hoover and I wrote on Population Growth and Economic Development in Low-Income Countries [1958]. That's been nearly a bestseller and I think it had some influence.

That was what I started to work on when I went back to the population field in 1954. I worked on the study of the undercount in the U.S. census. Then the International Bank [World Bank] said they wanted a kind of pamphlet. Eugene Black was president of the Bank at that time and he was concerned about population. He wanted to go to an African country which asked for a loan and say, "If you want a loan, you ought to be concerned about your birth rate." He asked Frank Notestein to write a pamphlet he could hand out. Frank told him there wasn't enough known in this field to make an authoritative statement. So the Bank offered to fund a research project to look into it. Frank hired Ed Hoover to be the Washington economist and I was the Princeton demographer. We went to India and made a study of the Indian situation and then to Mexico. It was a kind of economic-demographic model. My personal regret is that I don't keep up with this area of research. I think we raised a lot of questions, but I haven't seen any new cogent formulations and they are overdue [but see below].

VDT [from the 1988 interview]: You certainly made up for lost time once you were back [in

population]. You and Edgar Hoover produced the very important book, Population Growth and Economic Development in Low-Income Countries, for the World Bank, pointing out the great advantages there would be for India and also Mexico and similar countries if they reduced their fertility by 50 percent in 30 years. You went back to that in the paper you did for the American Academy session in 1986 ["Population Trends and Economic Development," in Jane Menken, ed., World Population and U.S. Policy: The Choices Ahead, 1986], which looked again at population growth and economic development in light of the revisionist, turnaround, stance of the U.S. delegation to the 1984 Mexico population conference. I'd had a question to ask you before I read that: Were you discouraged about how things turned out, because of course India's population growth was close to your high variant in the 1958 book?

COALE: National product under the high variant was also well predicted. We made an estimate using the parameters that Ed Hoover dug out of what was happening to economic growth in all fields--agricultural productivity and so on. He projected the growth of the GNP in India under the assumption of no change in fertility. Using the population projection with a 50 percent reduction in fertility, he made another economic projection. The first one, with no change in fertility, is within 2 or 3 percent of what has actually happened. So it's not true that we foresaw a catastrophe; people imply we're Malthusians or something. We foresaw that India was going to do quite well and just said that they would do still better if they reduced their fertility.

What I'm sorry about is that we used "population growth" in the title. That really isn't the theme of the book at all; growth is peripheral. For example, I realized in going back and looking at it that of the two countries we analyzed, India and Mexico, Mexico had much the more rapid rate of population growth and also a much more rapid rate of economic development. And we didn't see that as contradictory. We were simply saying that in those two countries, under very different conditions, both would profit from a substantial reduction in fertility as compared to not changing.

I went back and looked at Mexico 20 years later in a lecture I gave in Mexico in 1977 and said that Mexico had in fact just about doubled their per capita income during those 20 years. At the same time, their population had doubled, school enrollment had multiplied by three and a half times, life expectancy had risen, and so on. Didn't that contradict what Hoover and I had said? Not at all. What we had contended, and what I still thought was true in 1977, was that Mexico would have been still better off had they reduced the birth rate. In spite of having three-and-a-half times the number of kids in school, there were more kids not in school than there had been 20 years before, because the population grew so rapidly. If they'd brought the birth rate down, they could have had them all in school. So things had gone very well, but they could have gone still better. That was our theme. I still think it's correct--revisionism to the contrary.

VDT: I hope you'll keep harping on that theme. I think this is a very uncomfortable period for demography, this revisionist theory of population growth, the Julian Simon theme, the turnaround in the American long-term support of family planning programs.

COALE: Well, you have some guy like Ed Meese deciding what's going to happen! In this [Reagan] administration, we can expect to have ignorance and ideology predominate everything. Excuse me if I'm treading on your toes, but I really think that's the case.

Star Wars is an example. There isn't a reputable scientist--except Teller, who's sort of nutty--who thinks it has any possibility of working. And we're committed to it, pulling our best brains into this will-of-the-wisp. So I don't expect to get sensible population policy out of that crowd.

VDT: Well, let's hold our breath till [the elections] next November.

As you said in your 1979 interview with Andy Lunde, you've always had two streams of

interest in your career. One is abstract mathematical demography, stable populations, that began already with your work on the projections of European population in 1941. The other is in social problems, of which you've just given an example--your concern is not just about the recent lack of U.S. support of family planning programs in developing countries, but also backing of Star Wars. The mathematical interest obviously grew out of your first interest--as an undergraduate you started out to major in mathematics and physics--and that stream in demography began with your work on those projections.

COALE: That's true. I started out to major in math and physics and switched at the end of my sophomore year, when at Princeton you had to make the choice of what department you were going into. Your major doesn't start in the first two years. I chose economics rather than math and physics, but I did continue to have an interest in math and physics.

It's true--I say this to my students--that what I find appealing about demography is that there is a mathematical side to it that is not imposed on it as to some extent it is in economics. Mathematical economists talk about indifference curves, possibility curves, and tangents to these curves. Those are all constructions in their heads. You don't actually observe them, you construct them and use them to explain the way complicated situations develop. To a certain extent, it's a chess game. But in demography there are complicated and sophisticated and interesting mathematical relationships that arise fundamentally from the fact that we are a collection of objects which all get one year older every year. There are several effects that come out of that. The difference between demography and mathematics in, say, sociology or economics is that people really do get one year older--not approximately. Age is a continuous function. We can record the date of birth and the current date and determine age with precision. A person is either there or not; we count people. The same is true with births and deaths. So we're dealing with observables that can be given numerical reference with exactitude. And the relations among them lead to some complicated and interesting theorems. There's a genuine suitability of mathematics applied to demography, which is less true of other social sciences. At the same time, the implications of, for example, the changing age structure of a population, constitutes a very interesting social problem as well.

I left physics back when I was an undergraduate, because in the Depression I was concerned about economic problems and economics seemed more relevant to issues that I was aware of. In demography I could continue to do the kind of things I might have done in physics and math and still maintain a direct connection with social problems.

[On mathematical work, from the 1979 interview]: When I worked on population growth and economic development in India and Mexico, I became further interested in how the age composition of a population is determined. I used stable population analysis to estimate fertility and mortality and the true age distribution of India. At the same time I was working on the India book, I wrote a paper for the Milbank Memorial Fund Quarterly on stable population analysis. Later at a conference on biology, I presented a piece called "How the Age Distribution of a Human Population is Determined." In it I had the insight that not only does the population become stable if fertility and mortality rates are fixed--which means the age distribution becomes independent of where it started--and is determined by the fixed vital rates, but by analogy, even if the vital rates are varying, as they always are, it's still true that the age distribution gets determined by the recent past of fertility and mortality. That got to be known as "weak ergodicity" and was proven correct by Alvaro Lopez.

I also like to work on social problems. The attractive feature of demography is that it addresses social problems, but also has a rigorous and subtle mathematical and analytical side, involving real numbers.

[Continuing from 1979 interview]: There was The Demography of Tropical Africa [1968]. When Frank Notestein left the Office to become president of the Population Council and I became

director of the Office [1959], Frank wrote to me to say that Frank Lorimer wanted to start a project on Africa and the Carnegie foundation was interested in funding it. So I wrote a proposal and Frank Lorimer came to the Office and we assembled a staff including Bill Brass, Etienne van de Walle, Paul Demeny, Anatole Romaniuk, Karol Krotki, and Don Heisel. We had it going on for several years. That was typical of OPR work. It had international scope, but it also involved the development and application of a lot of analytical techniques for abstracting valid information from bad data. That in turn led to the work that Paul Demeny did for the United Nations on methods for extracting information from faulty data.

Parallel to that, I was working on model life tables. When on leave of absence in Rome, I carried with me a lot of material on model life tables. I had the idea for model life tables when making projections for the population of Europe, back in 1941. And the UN then came out with a full set of model life tables. But I thought they had certain weaknesses and I was trying to find another way to prepare the tables. I brought preliminary results back with me and played around and finally Demeny and I brought Regional Model Life Tables and Stable Population to a publisher [1966].

The interest in census undercount that started in 1953 led me to suggest a thesis topic to Melvin Zelnik on trying to estimate the completeness of birth registration by projecting censuses backward and then projecting forward again and getting estimates of undercount. He and I produced a book on the history of fertility of the white population in the U.S. [New Estimates of Fertility and Population in the United States, 1963]. I followed that up a few years later with a parallel study of the black population, with William Rives ["A Statistical Reconstruction of the Black Population of the United States, 1880-1970," Population Index, 1973].

The thread of interest in analytical and mathematical demography that was always there culminated in two ways. One was a couple of articles involving a mathematical investigation of an extraordinary regularity in the age patterns of marriage. I wrote a paper for the 20th anniversary of Population Studies on "Age Patterns of Marriage" [1971]. Then, working with Don McNeil, a very able statistician, I found a very pleasing mathematical explanation of why there was a regularity in the age of marriage of Americans. With T. James Trussell, I worked out a combination of age at marriage and marital fertility to formulate a set of model fertility schedules.

During those years, I had different ideas about stable populations. In the paper I wrote in 1957, I had the idea that age distributions are independent of the remote past, whether stable or not. Alvaro Lopez picked that idea up and made it the subject of his Ph.D. thesis and proved the existence of "weak ergodicity." This range of ideas are incorporated in my book on The Growth and Structure of Human Populations [1972].

I have gone back and forth between the mathematical aspects of population and the social and economic implications of population structure, growth, and change.

VDT [1988 interview continued]: One of your works that nicely combined your interests in both the mathematical aspects and social problems aspects of population was your part in The Demography of Tropical Africa. There were so few data that you had to apply yours and Brass's methods.

COALE: Brass's methods. Brass is the real designer of indirect methods. Can I tell you a story? We had a bright student, an economics major, who wanted to come into demography and he hadn't taken the first semester course. I allowed him to take the second semester course without the first; he had a lot of mathematical background and seemed very bright. At our picnic that year--we always have a picnic about now, in May--as is not unusual, it rained. We had some kind of shelter and we all hastened for it. He said, "We should use the Brass method for keeping off raindrops." There's a Brass method for everything.

VDT: But you and Paul Demeny did your Regional Model Life Tables.

COALE: Sure, I've done some work in analytical demography.

VDT: I've heard a story that William Brass when he first presented this method, at a seminar in Africa, said, "Of course, these things are no substitute for good data," and eventually the aim should be that Africa get some good vital registration. But now, I understand, he rather defends them; obviously they are splendid methods. Have they held back the collection of good data?

COALE: I don't think so and I don't think Brass has changed his position on that. It's going to be decades before you can imagine having a full and accurate registration system in many of those African countries. You must deal with the fact that people don't know their ages; it's a very difficult situation. It takes a high degree of effective central control to have a registration system that works. So I don't think Brass would be in the camp of saying, don't bother with registration, you can use these techniques. There are many things identifying the individual circumstances--births, for example--that don't come out of these retrospective data.

I have just heard from a student of Brass something he said that I'm going to quote very frequently. He said, "All data are guilty until proved innocent."

VDT: What do you regard as your leading work--the project that has given you the most satisfaction?

COALE: The individual piece of research that has given me more satisfaction than any other was devising an age schedule of entry into first marriage that seems to be almost universal--in developed countries, in developing countries, everywhere. It is universal in the same sense that the so-called normal distribution or the Gaussian distribution, the bell-shaped distribution, can be applied to the weight of newborn chicks or the test scores of army recruits. It's the universal curve that expresses what happens when the factors affecting the variate are random. The marriage curve is similar; it says what proportion of a given collection of woman in a population get married at individual ages. Like the normal curve, it has a different mean and a different spread in different populations, and even a different area, because there's a different proportion that doesn't marry at all. But if you specify the mean and the standard deviation, the same curve will fit in all those different populations.

I just found out that it fits in the People's Republic of China, for individual cohorts of woman, even though the mean age at first marriage was changing under the rapidly changing social situation. Each individual cohort has a remarkable degree of fit. There must be some adjustment for a catastrophic year in which people don't get married; that affects them all. But then they go back on the curve.

What was satisfying about the standard curve of marriage was that we stumbled on this uniformity while looking for something simple to make it possible to estimate the distribution of European populations by age and marital status for our European project [Princeton's European Fertility Project]. Some 19th century censuses had a distribution of women by age and a distribution of women by marital status but no cross-tabulations. We needed to know what proportion were married at every age and I thought if I could find some kind of uniform curve of proportion ever-married by age, I could slide it along the age axes. I set out to look for it and it turned out to exist.

It was especially satisfying that when working with Don McNeil, through different forms of mathematical analysis, we stumbled on a behavioral explanation of why the curve was uniform. It's because there are several constituent steps in entering marriage. The first is becoming marriageable according to social norms--reaching the age of menarche, in many populations. Age of becoming marriageable has a normal distribution. Once marriageable, there must be a search for the ultimate

spouse. Once the spouse is located, there is a period between locating the spouse and getting engaged, and then between getting engaged and marriage. All of those constituent stages have their own distributions. When you put them together, they form the universal curve.

What happens in Asia or Africa is that the whole process goes very fast, because the marriages are arranged. As soon as a girl reaches menarche, the family searches around to find her husband. After the husband is found, there's a contract written, equivalent to engagement, and then a period before the marriage. All of it happens very fast. In Sweden or Ireland or the United States, the bride and groom have to find each other congenial and suitable and the search takes a long time. Also becoming marriageable occurs later--the age at which steady dating is accepted as okay. The whole process is much slower. But the shape of the curve is just the same; it's just later and more spread out.

VDT: Marvelous! And you discovered this in the process of working on the European Fertility Project. For which you also devised your ingenious indexes of overall and marital fertility. I used those for a Georgetown term paper comparing historical trends in Quebec and Ontario fertility, which I'm still excited about. Then you and Trussell went on to devise the little m's and so on.

COALE [On the European Fertility Project, from the 1979 interview]: My mentor Frank Notestain had made a very cogent statement of the theory of the demographic transition. I've always been interested in factors that contributed to major changes in fertility and mortality in the developed countries. There were a couple of students at Princeton who wrote doctoral dissertations synthesizing Notestein's work who found that the decline of fertility in England and Hungary began about the same time and proceeded at about the same pace. England was the leader in industrialization and had an almost fully literate population by 1880. Hungary was mostly agricultural, mostly illiterate, and still had high infant mortality. The two very different countries started their fertility decline at the same time. That had been overlooked because of the higher proportion married in Hungary. The crude birth rate was always higher than in England, so in any cross-sectional study, England and Hungary didn't look alike.

This anomaly led me to think the demographic transition theory had been based on a kind of impressionistic look at the statistics of Sweden. So, back in 1963, I thought that we ought to collect all the data on the fertility of individual provinces that had experienced the decline and try to find what the circumstances were.

I probably wouldn't have started the project had I known how large a job it would prove to be. The statistics for different countries have surprising gaps; the data need a lot of adjustment before you can compare them. Each country has particular peculiarities; some don't have rural-urban classifications, for example. And then the factors that influence fertility are not the same. One needs to look at the culture of each country and become a social historian. So each person drawn into this project has ended up spending five or six years accumulating enormous files of computer output. But it may produce a better understanding, or at least an appreciation, of the complexity of historical demography.

VDT [back to 1988 interview]: That European Fertility Project has been praised to the skies by Richard Easterlin in an article in Sociological Forum ["Toward the Cumulation of Demographic Knowledge," Sociological Forum, Special Issue: Demography as an Interdiscipline, Fall 1987]. He called it "the most important and successful demographic research undertaking of recent times." And the great thing about it was it didn't cost too much for the amount of output it had. The World Fertility Survey, he said, was also a great project but it cost a lot. You must be very satisfied with that.

COALE: Well, yes. It's different from other kinds of projects I've been involved in. It was an

inordinately ambitious scheme. I wouldn't have started it if I'd known it was going to take 20 years. Each of the people who got involved and wrote a book on a European country found that they had to spend four or five years on it, because one is never satisfied; you have to know everything about a country before you can begin to write its fertility history. You start off with an idea and it leads to something else. It has taken an inordinate amount of work; it's been slow at getting to the stage of completion that it's reached.

Early in the project, I said at a conference of our staff that as a minimum we would document the decline of fertility in Europe and the circumstances under which it took place and as a maximum we would find out some kind of generalization about what had caused it. The minimum would probably be achievable. I remember saying it would probably be Kingsley Davis who would come out with the maximum.

VDT: He's always the one who wants the overall context.

COALE: We did not produce simple new generalizations. In terms of the demographic transition, I think the project has done more to qualify or destroy explanations people had already made than it did to create overall generalizations. Thus it's not always true that infant mortality goes down before fertility; very often fertility goes down before infant mortality. There were a lot of ideas that had been in the standard explanations of the demographic transition that didn't hold generally. There have been, I think, some quite good new ideas that came out of it. It think it means that from now on research on the decline in fertility will have to be reconciled with that body of information. Perhaps we will find out that it's different in different times and places. That may be the answer.

Actually, my personal disappointment is that my biggest individual contribution was to be the principal author of a study of the decline of fertility in Russia [Coale, Barbara Anderson, and Edith Harm, Human Fertility in Russia Since the Nineteenth Century, 1979]. Every now and then I look at that and I think that is as good as anything I've ever done. And it attracted very little attention.

VDT: I have to admit it must have, because I came across it in Easterlin's article and I did not know it. I knew from working on a Population Bulletin by Murray Feshbach on the Soviet Union that the Central Asians did not reduce their fertility as you would expect under the demographic transition theory. You're right; it should have had more attention.

COALE: I just went back to it. It really does illustrate the effect of cultural factors in fertility decline. It's a marvelous laboratory for doing that. The book just got lost; never got reviewed.

VDT: That's too bad. It's sort of common knowledge in the demographic world--and I'm sure you're the one that's brought it out--that those Central Asian republics are a peculiar anomaly; they will not bring down their TFR, despite modernization in all other respects.

COALE: There are other instances of the same thing, not as extreme as that. That's the kind of cultural context Ron Lesthaeghe found among the Belgians. The French-speaking areas reduced their fertility and Flemish-speaking areas did not. There were two villages ten miles apart with very similar socioeconomic conditions. The French-speaking village reduced its fertility a long time ago and nothing had happened in the Flemish-speaking one. In that particular case, as in the French-speaking and English-speaking populations in Canada, the laggards finally caught up, and then some. Fertility is now lower in the Flemish-speaking areas than in the French-speaking, just as the French-speaking population in Canada now has lower fertility than the English-speaking.

I wouldn't jump to the conclusion from that that they are going to have a big reduction in

fertility in the Central Asian republics. I think that the strongly self-identifying Islamic cultural areas have attitudes toward women and children that are very slow to change, education or not. Like Irish Catholics.

VDT: That may retard the possibility of bringing down world population growth as rapidly as hoped.

Let's talk about your connections with the IUSSP. It was reestablished after World War II with individual members. There were 29 Americans among the 147 demographers on the first list. When did you get elected to IUSSP?

COALE: I would guess in the late 1950s [1956].

VDT: Have you attended most of their meetings?

COALE: I must have been elected by 1957, because I went to a meeting in Stockholm that year; wrote a paper for it. It was every two years in those days and I went to Vienna in 1959, New York in 1961.

I'll tell you a funny story. In the fall of 1972, I had a leave of absence and was spending it in Florence. I've spent three leaves of absence in Italy. I did the work on model life tables in Rome; that was the first one. I went to Italy on leave of absence at the advice of a hyper rational colleague in the economics department. At Princeton, you don't get an automatic sabbatical leave every seven years. Instead, each department can have one seventh of its members on leave at any given time. You have to justify a leave; apply for it. If you haven't had one in a long time, the justification doesn't have to be too strong. I didn't realize all of this. I hadn't had a leave for many years and was thinking of asking for one and talked to Will Baumol about it. I said I was thinking about going to INED or the LSE, where they had strong demography. He said, "Don't do that; they'll ask you to give lectures, supervise students. Get your research project to the point where all you need to do is to write the text and then go to Majorca." I followed his advice, but for me Majorca was Rome.

On a later leave in 1972, I was in Florence, my third leave in Italy because I love Italy. I was not studying the population of Italy but working on a continuation of my projects at Princeton. Massimo Livi-Bacci, who was then secretary-treasurer of the IUSSP, asked me if I'd be willing to be a candidate for vice-president of the IUSSP; the succession from vice-president to president is automatic. I said yes, under the belief that there was enough anti-Americanism in the membership that I wouldn't get elected. I went to the Liege meeting in 1973. I didn't attend the business meeting because of some sort of conflicting activity. Someone came out of the meeting and said, "Congratulations, you've been elected vice-president of the Union."

VDT: Elections were held on the spot? I thought they were by ballot.

COALE: They were on the spot then. The nominating committee came up with the nominees at the meeting and then the election was held on the spot. I didn't know that.

I was already suffering from being the director of the OPR, with a lot of administrative responsibilities. I knew the vice-president spent four years going to the Council meetings and then four more years being president. I was very depressed. I went back to our hotel room and told my wife I was facing eight years of going constantly to meetings and doing administrative work and she said, "Maybe you won't live that long."

VDT: One of the aims of IUSSP has been to increase the proportion of people from less developed countries, but I think we need to have some Americans in there still. And you're right, there's anti-

Americanism, as in almost any international organization. So I'm glad you were there.

COALE: Actually, I enjoyed it. It was a lot of work, but I enjoyed it. I like dealing with people, I guess.

VDT: The IUSSP is important in providing many ways for demographers from the two parts of the world to get together, although here at OPR, you have so many demographers from less developed countries, many of whom go back and become well known. Perhaps that connection is enough.

COALE: Oh, no. I like to see them again at the IUSSP meetings. We have trained many LDC demographers who are good. I kept count of OPR alumni at a couple of IUSSP meetings. They listed the participants by country. The number present ran: U.S. members first, then Indian members and French members, in that order, as I remember. OPR alumni were more numerous than participants from any country except those three.

VDT: Whom do you consider your outstanding students, those in whose subsequent careers you've taken particular pleasure?

Ansley is showing me a whole section in his library that has the theses of his students. Here are Pravin Visaria, Barbara Anderson, Jeremiah Sullivan, Linda Martin, Jacki Forrest, Howard Goldberg, Mary Breckenridge, Jane Menken of course, Miroslav Macura, David Bloom, Neil Bennett, who raised such a flap, unwittingly, over the poor marriage market for educated women in the U.S., and so on. That's a tremendous array. It seems like most of the younger people in U.S. demography and elsewhere have been your students.

COALE: There are others. Al Hermalin, Sam Preston and Alvaro Lopez whom I mentioned earlier, Pravin Visaria's wife Leela, and many others. I have a terrible memory for names and I hesitated to try to list them, because I would leave out some favorite who would then be offended. I think that's the most rewarding aspect of being an academic, having such an outstanding and congenial group of students.

VDT: Many of those students must of course have been attracted to Princeton, to demography, because of you. Did they usually deliberately ask to work with you or were they those who were more quantitatively oriented?

COALE: Actually, during a long period after Frank Notestein left [1959], I supervised almost all the theses. Less than half of the students, I think, come to Princeton to study demography. Sam Preston was an example of someone who did not; David Bloom was a more recent one. The long list of students we've had, especially of economists, have come here because we have an outstanding economics department and they've come to study economics. Those who plan to specialize in labor economics may hear that it is a good idea to study demography, because it's very pertinent to labor economics, and so they turn up to take the two one-semester standard courses in demography that have been given for many years, and some of them love it, decide to write a demographic thesis, and they become demographers.

When I've been boasting about the quality of these students and the fact that 16 of the presidents of PAA have been here one way or another, it's only fair to say that that isn't primarily due to the quality of the demographic training. It is the selection of the students. The mean score on the Graduate Record Exam of students we admit in economics is in the 95th percentile. Sam Preston isn't a great demographer because he studied with me. He's a great demographer because of his ability.

And it's very, very nice to be teaching in a place that selects its graduate students so carefully.

I've left out people like Etienne van de Walle and Francine, and Susan Watkins, Doug Massey, Jim McCarthy, John Knodel, and Doug Ewbank. I recently went down and gave a lecture at Penn. A very funny thing happened. When I was introduced, they said I was the mentor of Sam Preston and Susan Watkins and the two van de Walles and others and one of the students came up to one of these people and said, "Does that make him our grand-mentor?"

VDT: It's a wonderful line of heredity that you've spawned, now into the second generation.

COALE: I left out Paul Demeny.

VDT: Who was a student of yours as well as a collaborator. That's a great attribute, that many of your students have gone on to work with you in projects as well. You'll be sorry to lose Jane Menken [leaving Princeton for the University of Pennsylvania at this time]. Tell me about Jane, as a student and colleague.

COALE: I did an unintentionally mean thing to Jane. I was asked to introduce three successive presidents of PAA [as they gave their addresses at annual meetings]: Sam [1984], Jane [1985], and Paul [1986]. As I was thinking what I was going to say about Jane, I remembered a funny feature of her career. She had gotten a master's in biostatistics at Harvard and had then been employed at NIH [and Columbia] before we recruited her to be a junior research staff member here at the Office. When she got here, she decided that she needed a Ph.D. We didn't have one in demography so she opted to get a Ph.D. in sociology. The reason that she hadn't gotten a Ph.D. at Harvard was that she was so pathologically shy that she didn't want to follow a program that required her to take an oral exam. When she was here, she was asked to give an informal Friday afternoon seminar and she couldn't sleep before. So when I was introducing her as president, I tell this story--and it brought back her nervousness! She could hardly talk when she started.

VDT: I don't remember that; I heard that address.

COALE: The nervousness was not apparent, but she told me afterward that she really had trouble getting started. Then her confidence reasserted itself as she went on.

Jane has been a delight. She would have done well anywhere. She was a brilliant staff member and a first-rate student, of course. She had a readymade thesis as a result of her work with Mindel Sheps. She has been an ornament to the field. While she was still finishing her Ph.D., she spoke from the platform at a PAA meeting. I turned to the person next to me and said, "That's a typical graduate student at Princeton!"

I was asked by the IUSSP to be chairman of a committee on some aspect of fertility. IUSSP committees design a research program and hold seminars and so on. I said I was already swamped with administrative responsibilities. I knew from experience that an IUSSP committee chairman has a lot of work, involving a lot of correspondence. I said I would be willing to do it if they would appoint Jane as a paid secretary. That launched her on her international career. It was during the period when I was vice-president, I think. We had the first committee meeting in Florence. It was Jane's first trip abroad and she just had a ball. She flourished in this position, was really good at it, ran the thing extremely well, and then went on. It was part of her belated maturation, getting over her shyness, and having her administrative talents come to the fore.

VDT: And Paul Demeny came here as a student?

COALE: Yes. What happened was he left after the 1956 revolution in Hungary. He had been very disaffected by the government before that as well, but the situation was extremely uncomfortable after that uprising. He was a professional employee of the government and he went as a statistician to a meeting in Geneva and asked for asylum when he got there. [Not quite how it happened; see Demeny's interview below.] He was a stateless person in Geneva, which is very uncomfortable; the Swiss allow people asylum but they don't give refugees any kind of citizenship. Dudley Kirk was in Geneva. He was then head of the Demographic Division of the Population Council. He ran into Paul [was directed to him by Frank Lorimer] and suggested that he apply for a Population Council fellowship. Paul did and came to Princeton. He got his Ph.D. in economics and wrote a demographic dissertation. He worked on The Demography of Tropical Africa, model life tables, and Manual IV of the UN Population Division.

VDT: You've had a lot of outstanding students.

Ansley has just marked off on an OPR alumni list ["OPR Alumni List," OPR Newsletter, Spring 1987] all the students who have done their dissertation under him and they range, column after column, from Barbara Anderson, the first one on the list, to Hania Zlotnik--those two in themselves are well known. So indeed you can be called Father, Grandfather, Progenitor of the U.S. demographic scene. That must indeed be one of your great satisfactions.

COALE: It is.

VDT: I'd like to ask you to repeat a story you told in your 1979 interview about Frank Lorimer [PAA president 1934-39], who wanted you to come back when you were on your honeymoon, so you must have known Frank quite well. Tell me about him. I regret I didn't get to New Zealand in time to interview him before his death.

COALE: Frank was a free spirit. Here is that story. Frank was working on The Population of the Soviet Union [1946], part of the OPR work on Europe for the League of Nations. I had worked out a way of correcting the understated mortality rates at older ages in Eastern Europe. The rates were clearly understated in Russia as well. He wanted to adapt the system that I had used for the Balkan countries and apply it to Russia. I had corresponded with him about it. He asked me to come down to Washington and speak before his graduate seminar at American University on this subject. I wrote back saying I'd love to come but the time he mentioned was right after our scheduled honeymoon and I wasn't sure I would be ready to come. He sent me this telegram: "Please come and bring the new Coale to our castle."

Later on, Frank and his second wife had a problem about living in a hippie commune. Earlier he was traveling in Africa and, typical of Frank, he was staying in African hotels, deliberately staying away from European hotels. Staying at such a hotel was a nurse who had the same ideology, determined not to be identified as a European and to share the African life. They met and fell in love. This was Petra, many years his junior. Later they were back in the United States and Frank was always a rebel, so with Petra he moved into a commune in Connecticut, with a lot of hippie types. Frank had to leave, because the commune wouldn't tolerate dissent. If you did not follow their precise hippie line, they didn't want you there. Seventy-year-old Frank couldn't stand the hippies' intolerance! Actually, he was in his sixties then. His baby was born when he was 69, I remember that. Surpassed by Kingsley David, whose [last] baby was born when he was 79. I think it's lovely that this man in his sixties couldn't tolerate the intolerance of young radicals.

VDT: Another person I wanted to ask you about whom I regret we didn't get in this series of interviews is Robert Lapham [PAA secretary-treasurer 1984-87]. You must have known him when you were chairman of the National Academy of Sciences Committee on Population and Demography and he was study director.

COALE: I hired him for that committee. On a leave of absence, one of the few that I didn't spend in Italy, I was at the East-West Center in Honolulu. I got a phone call from the NAS, asking me if I'd be willing to be chairman of a committee on population and demography that they were founding. The formation of the committee was requested by AID to make estimates of fertility trends in major LDCs. AID was in trouble with Congress because Ravenholt was making claims about reductions in fertility that were not credible. The AID administration wanted independent estimates. I gave preliminary agreement to serve and went to Washington to plan the membership of the committee and the staff. Lapham was then at the Population Council. I knew him slightly. He was very highly recommended by Parker Mauldin, so I suggested that he was ideally suited to be head of the staff. He had the background and administrative experience. Ken Hill, a Brass product at LSE, was a technical staff member and Hania Zlotnik, a Princeton Ph.D., was another. I worked for five years with Bob Lapham as the senior employee of the committee. He was a remarkable man.

VDT: He did so much. He overlapped that job with the Demographic and Health Surveys and with being secretary-treasurer of the PAA. Amazing how much he got done, and he must have been ill too. [Robert Lapham died of cancer on February 20, 2988.]

I'd like to touch on your time with the UN Population Commission, as the third U.S. Representative [1961-67], following Phil Hauser and Kingsley Davis, and before General Draper. Does that time stand out in your memory?

COALE: Oh sure. It was a very interesting experience. Let me give you an idea of what was interesting and kind of frustrating about it. The Commission was like a legislative committee. That is, it was an organization that made recommendations about what the actions of the UN should be. Its report went to the Social and Economic Council; the Commission couldn't act itself. When I first went on it, there was a coalition against any kind of action, at least, having to do with human fertility--a coalition consisting of devout Catholic developed countries, the Soviet bloc, and, to a certain extent, other Catholic or Marxist countries from Latin America or wherever. The Russians were the most inflexible. When I was first there, the Russian delegate would object any time there was a mention of the word fertility in the Commission report. We'd have to think of some euphemistic way of saying the same thing. They would make speeches about how concern about lowering fertility in the Third World was capitalistic, imperialistic, and cannibalistic. Marx had denounced Malthus, hence they had to say the problem of poverty came from capitalism or colonialism and not from the high birth rate. It was a little wearing.

Then also there was a very inflexible Stalinist who was their representative, a fellow named Podyatchikh, who'd been the census commissioner. He was absolutely inflexible and also was obviously operating under instructions. I remember vividly that in the late 1960s there was an item on the agenda about the 1970 round of censuses. That was officially the business of the Commission on Statistics, rather than the Population Commission; nevertheless, we were asked for our recommendations. Various recommendations were made and then Podyatchikh got up and talked for half an hour about how the 1959 census in Russia had shown that everything good in Russia had quadrupled, they were the best country in the world, and so on. He went on and on. Members turned off their translators and read their newspapers. It was not very effective to give such a long propagandistic speech. I think he may have felt uncomfortable, but he was under instructions.

On the last day and a half of its meeting, the Commission devoted itself to accepting the report. A rapporteur took notes, drafted the report, and then read it. The members of the Commission reacted to it. They started off objecting to everything. Then on the last day they began to accept it all, because shopping time was coming to an end and they were going to leave. At the end, everyone said what a great job the translators, the rapporteur, and the chairman had done.

After we had adjourned, I met Bourgeois Pichat and I said, "Jean, this is the only report I know that has more authors than readers." And he said, "No, no, the same number." It's true. I never read the report when I wasn't on the Commission. It would be published, but no one read it.

It was an interesting opportunity for interchange with other people and it performed a certain useful function, that I tried my best to foster, of constructing the best agenda for the Population Division between the meetings of the Commission. The Division had to pay attention to it. We tried to include the best technical material and members like the UK representative, Bourgeois Pichat and a few others, who were themselves professional demographers, would be able to select items of the agenda to suggest something sensible. That made the Commission worthwhile. The long political discussions were interesting but frustrating.

VDT: Do you think it regrettable that the U.S. does not now have professional demographers as representatives?

COALE: Well, I think it's regrettable, but the U.S. participated in the further politicization--if that's the word--of the Commission. Frank Notestein actually founded it. He was the first director of the Population Division and had a lot to do with forming the nature of the Commission. And the Commission was initially listed as a technical body that would give advice to the population activities in the UN. Very soon--the way things happen--the countries nominated their representatives. In fact, most of the smaller countries on the Commission just sent people from their New York staffs who didn't know anything about population. They spoke up to make sure that their individual Marxist or Roman Catholic or whatever view their government represented was spoken for. The appointment of General Draper brought a political point of view to the U.S. representative. He was very effective in doing what he thought was right. He succeeded in pushing the Commission to advocate stronger birth control policies. He was a skilled political operator rather than a technical person. And that contributed further to the change in the nature of the Commission.

VDT: Do you think the Population Division is still the progenitor of the best international demographic statistics that are available? After all, they all have to accept what the countries report.

COALE: Well, sure. Actually, the Demographic Yearbook is produced by the Statistical Office rather than the Population Division, but a lot of the work is done in the Population Division. These periodic assessments of world trends are quite useful documents.

I'll give you an example of what I think is their greatest handicap. At a certain predictable time, they didn't publish any more statistics from Taiwan. When China was admitted to the UN and then to the Security Council, the UN took the official position that Taiwan was a province of China and they no longer published any data--perhaps the most valuable data from East Asia! I think it's too bad that the UN follows such a political line. They have to give priority to the political reality.

I think my relatively warm impression as a member of the Commission was a result of the fact that I wasn't speaking for the U.S.; I was speaking for the field. In fact, when I was asked to be the U.S. representative, I said I would feel very uncomfortable getting up and making speeches about the U.S. position on population. I said I knew Frank Notestein had been instrumental in establishing the Commission and had wanted it to be a technical body. The Under-Secretary of State who was

negotiating with me to accept the position said, "I want to make it clear to you that you are appointed by the U.S. but you will speak as an individual professional demographer. Should something come up, such as the acceptance of Mainland China on the Commission, where the U.S. has an official position that comes from the Secretary of State, you will have to speak and you are authorized to say, 'I am instructed to say.' Anytime you feel uncomfortable--and it will almost never happen--but if something that reflects an important U.S. policy position comes up, you are entitled to present it with that introduction--'I am instructed to say'--that is, you will separate yourself from the political aspect of it." I felt very pleased about that. I never had to say it. There was never any pressure put on me.

VDT: Now on PAA. You and Andy Lunde talked in your 1979 interview about the flap that occurred when you were president-elect [1966-67] over the recommendations of the Forrest Linder Committee on Organizational Management [including a paid business manager, "positive membership recruitment activities," the establishment of "professional standards for membership qualifications" including two classes of members, and a small-grants research program, from PAA history vignette on this committee by Paul Glick, PAA Affairs, Summer 1982]. You agreed that a business arrangement should be made, but objected to other recommendations.

COALE [from the 1979 interview]: Up until that time [1966-67], the secretary-treasurer of the Association maintained all the business and correspondence of the Association, but he did that through his employer. Paul Glick [secretary-treasurer 1962-65] worked at the Census Bureau. A substantial amount of his secretary's time was devoted to PAA affairs. When his term was completed, I think it was he who said no government office or university could be asked to devote that much resources to the PAA. We decided that we'd have to find someone who would do this and get paid for it. We would have to increase the dues to make it possible. That decision was made and I believe you [Anders Lunde, secretary-treasurer 1965-68] were asked--or volunteered--to see if we could find an arrangement. That's how it all came about [PAA business affairs were taken over by Ed Bisgyer and his staff at the American Statistical Association; see Lunde interview above]. I think it's been a marvelous thing.

[On other recommendations of the Linder committee]: When I was president-elect, there was a proposal circulated that the Association should undertake a number of additional functions and that the budget should be increased by a large amount and a manager be hired. For example, the Association should solicit funds that would be used for grants for population research.

It was my feeling at the time that the function of the Association was to provide a place for interchange of ideas, to make it possible to conduct meetings, and to sponsor publications. These other diverse activities are not effectively carried out by an association such as the PAA. To administer funds would require a big staff. Since the officers of the Association are elected and change all the time, it is not an organization that could allocate research grants as NIH and private foundations can.

When the proposal came out, when I was president-elect, I wrote Paul [then president, 1966-67] stating my reasons for opposing it. At the business meeting that year [during 1967 annual meeting in Cincinnati], there was a discussion of the proposal and it was voted down. I still feel it was the right decision.

VDT [continuing 1988 interview]: The Linder committee proposed two classes of membership and broadening the membership greatly and you felt it could get a bit too broad. Do you think that demography and the PAA should really stick with those whose real work and interest is demography? What do you think about the applied demographers, for example?

COALE: I said at the time, and stated in my Lunde interview, that I was not in favor of restricting the

membership. That wasn't my point. What I was in favor of was restricting the activities of the PAA to the development of the professional field. Applied demography is certainly part of that. What I was opposed to was taking resolutions in favor of zero population growth or pro- or anti-abortion or on any other issue of that sort. I thought the resolutions that the Association should take should be in favor of some aspect of census-taking or having accurate vital statistics or something else that was a professional, technical matter, which the PAA, as a professional society, could take a stand on--not political issues. I still feel that was correct. I think the Association would be weakened by taking professional stands.

I didn't like the idea of two classes of members. I wanted everybody interested in population in the PAA, with no restrictions. But just never let the society get into being an advocate of a particular political point of view.

VDT: Can you remember your first PAA meeting? It must have been at Princeton.

COALE: Not really; I remember its happening.

VDT: Here's a list of all the meetings and where they were held.

COALE: The PAA met in Princeton in 1936, 1938 and in 1941. I certainly would have been at the 1941 meeting; that was before I passed my generals. There wasn't any meeting in 1937, so two consecutive meetings were in Princeton, 1936 and 1938, and then another one three years later in 1941. And then again in 1946 and 1947, 1949, 1950, and then in 1952 and 1955. Note the registrations at the early meetings: 20 [1934], 38 [organization meeting, 1931], 17 [1933], 67 [1932].

They used to meet here in Princeton and the members would be put up in the Princeton Inn. It was a small group of people who all knew each other quite well. That was my memory of the early meetings. I'm not sure I was present at the one in 1941. I remember the flavor of it, but not any more.

VDT: Do you regret the relatively enormous growth of the meetings? At the one we've just been to in New Orleans [April 21-23, 1988], there were over 1,100 people [1,115], 87 sessions, seven and eight overlapping at a time.

COALE: I think that's inevitable. The PAA meeting is not as big as the Economic Association meeting. Too many sessions can be frustrating, because you can't be at more than one at a time. In the interview I had with Andy, I explained that I tried to increase the size of the program when I was first vice-president [1964-65; responsible for program of 1965 meeting in Chicago]. The first vice-president in those days had the responsibility for the program. I heard complaints--not said to me directly--about an old boys' network. The program always consisted of well-known people and the youngsters didn't have a chance. I thought to an extent that was true and unfortunate.

So I instituted a reform. I sent out a notice to all members that any member was entitled to give a paper. He or she could send in something like 20 copies of a paper to the program chairman--that was me. I took the responsibility of organizing sessions out of the different papers and finding chairmen for each session. We got a dozen or more sessions for those submitted papers. I felt that the average quality was just as good as for the invited papers.

The system did not last. Within the next year or two as the program chairman tried to follow this scheme, hundreds of papers came in. Members would get a free trip to the meeting from their universities if they were giving a paper. Self-selection of authors did not work. The idea of giving everyone a chance is good, I think, as long as you don't compromise the quality. Part of the function of the meeting is to give everyone a chance to get an audience for whatever idea they're working on.

With a large membership that means a lot of sessions.

VDT: That's very important indeed, even though there might be just two or three left to hear you on Saturday afternoon at the last session. You can put it on your resume that you gave that paper.

Actually, demography has remained quite a small discipline. You mentioned the Economics Association meetings, which I know are huge. The membership of the PAA is just around 2,600 [2,679 at the end of 1989] and has been for years; we seem to lose some and gain some. That's pretty small as professional organizations go.

Your presidential address [in 1968] was on "Should the United States Start a Campaign for Fewer Births?" [Population Index, October/December, 1968]. That sort of ties in with what you say about not wanting PAA to become an advocacy organization. Of course, it was at the height of the Paul Ehrlich "population bomb" scare of the late 1960s. You concluded in that speech that there was no need of a policy to bring down fertility in the U.S. It would come down of its own accord.

COALE: It was in the middle of doing so. I said then and feel now that was like the generals who always want to fight the last war. There was alarm about a high birth rate when it was on its way down. It was really almost down to replacement at the time. I'm not one who says that giving people freedom of choice necessarily leads to an optimum outcome. I would not be surprised if in the next generation we will come to feel the need for policies aimed at raising the birth rate.

VDT: What would you suggest could be such a policy?

COALE: Well, in general the kind of policy that appeals to me, as a staunch libertarian, is having maximum freedom of choice. I would like to have everyone informed about the possibilities of controlling fertility. The means of effective control should be made available to those who might not otherwise have them. Then policies could be aimed at influencing decisions at the margin. Gunnar Myrdal in the 1930s said it was wrong to give people a cash bonus to raise the birth rate. That might induce the wrong people to have more children. It was better to try to offset the disadvantage that people see in having more children. A policy that I liked was making university education free, with admission by competitive exam. People need not refuse to have children because they feel they won't be able to educate them. The aim is to affect decisions at the margin, to reduce the marginal cost of having an additional child. Then let those who want to have four or five children do so. Fine. With several siblings, children grow up with a minimum of rivalry. The people who love children are the ones that have them. I don't like the idea of "Stop at two" or any other uniform rate. When the birth rate's too high, one should try to raise the marginal cost of a child. When fertility's too low, you try to lower the marginal cost.

VDT: Now you're saying something might be done about it, but you didn't . . .

COALE: I said so then. At the end of my address, I said I would like to see fertility influenced in the tradition of the Chicago economists--making childbearing a free choice, but trying to influence it at the margin, not trying to propagandize.

VDT: Do you think there will have to be a stronger push for reducing rapid population growth in developing countries? The latest Population Reference Bureau World Population Data Sheet and the press release that went out with it show that world population growth seems to be stalled at 1.8 percent and the trend is getting closer to the UN high variant than it is to the medium variant.

COALE: I think that it is advantageous for countries that have high fertility to reduce it. I do not

concur with the Chinese one-child policy, including aborting women who get pregnant with a second child or sterilizing women after a second delivery without a free choice. A very effective way of controlling fertility, but I don't like it. I don't like interference with individual freedom. The situation in China is not nearly as desperate as it is represented. The total fertility rate is down to less than 2.5. That's splendid. Let's use moderate policies; give people as much choice as you can; try, as I say, to influence at the margin. More work points for people with fewer children is the kind of thing I think is not too bad. I do not want to see coercive measures introduced into Bangladesh or anywhere else. I remember Notestein saying when coercion was being proposed in India, "Coercive measures to force contraceptive practice are more likely to bring down the government than the birth rate." That's what happened in India. I'm not especially optimistic about what is going to happen. I view high birth rates in poor countries as one of the many problems in the world. Agricultural productivity is not going up as fast as it might with better organization and agricultural policies. There are irrational policies of subsidizing low agricultural prices for the benefit of people in the cities who are the ones that have political clout. That ruins the farmers' profit and keeps agricultural output from going up.

Those things need to be changed. How? I don't know. Basic social changes are needed in many countries before the birth rate will come down. Nothing will happen easily. Bangladesh will double its population in a short time. It will be very impoverished. Population growth is not going to lead to a famine or the world coming to an end, but it certainly will make a lot of problems worse.

When I see a situation that's bad, I'm always interested in trying to make it better. There's a side of me that says lots of situations aren't going to get much better. We do our best, but we have to avoid getting into extremist positions, like the book Famine 1975. Most famines since World War II are the result of bad policy, not a growing population. Fewer people die now as the result of malnutrition than used to. In fact, per capita output of food has risen.

VDT: Not in Africa.

COALE: Not in Africa, but that's not because of population growth primarily. It's because they have the most incompetent agricultural policies in the world. And it's much more important to reform those policies than it is to work on the population growth. Not that population doesn't need attention. Population growth is not the source of the problem; it is the incredibly inept agricultural policy. And they need to work on it.

I agree with the National Academy report [Population Growth and Economic Development: Policy Questions, 1986] on that part of it. Where I disagree with the report is the view that population isn't important. It is. It's not the primary cause of the things that people like Ehrlich blame it for. I've been advocating this since 1955. But Hoover and I did not say that there was going to be a disaster. In fact, our projections for the economies of Mexico and India with no decline in fertility implied a substantial improvement, as indeed did happen. It just would have been a lot better--and still would be--if they would reduce fertility. And if the United States had not gone to Mexico City [International Population Conference 1984] and said that population doesn't matter.

VDT: What are you doing now?

COALE: Right now I'm finishing up a paper for a conference on mortality in Asia that's going to be held in Beijing in August. I've written a paper showing that even in China, where ages are reported with extreme precision, at the very oldest ages their mortality rates are not reliable, because of a minority--less than 1 percent of the population--that misstates its ages terribly. Xinjiang province in the west has 50 percent Islamic minorities. It has 1.3 percent of the population of China, but 47 percent of the males aged 95 to 99. Of course, that's not true; the true proportion is no more than about

1 percent. Half of the mortality rate for China at 95-99 is based on the terribly understated mortality rate for these non-existent people. Even in China, the death rate at the very highest ages is contaminated by misstatement of age. If you leave that province out, the death rate at 95-99 is acceptable.

VDT: Carrying on from a paper of yours I reread while preparing for this interview about the apparent crossover of mortality rates at the oldest ages [rates for blacks become lower than for whites] having much to do with this misstatement of age.

COALE: That's right. Even in China, where for the most part the ages are incredibly precise, there are minorities that don't share that precision in reporting age and it contaminates the rates.

VDT: You're doing quite a bit of work on China after your excellent piece on the 1982 retrospective fertility survey.

COALE: I've published a thick volume at the East-West Center on fertility rates by age of women and by duration of marriage for all 28 provinces [Coale and Chen Sheng Li, Basic Data on Fertility in the Provinces of China, 1940-82, Papers of the East-West Population Institute, No. 104, East-West Center, 1987]. It's based on the survey. It goes back historically. Also based on the survey, I have analyzed the distribution of intervals between births.

I am scheduled to go to London and give a paper at Brass's retirement in late June, a paper on the use of models in demography. Then what I want to do next year is write a book with Sam Preston on the so-called "variable r" analysis, an extension of stable population analysis to any population. The rate of increase of the population is a function of age, instead of being constant as it is in the stable population. Using the variable rate of increase, one can generalize the stable population relations between fertility and mortality and the age distribution to any situation. Variable r extends the understanding of basic relations in demography and their use in estimation. Sam and I had an incomprehensible, too dense, article in Population Index four or five years ago setting this idea forth, with too many illustrations ["Age Structure, Growth Rates, Attrition, and Accretion: A New Synthesis," Population Index, Summer 1982]. It was just too constipated to read. We've had our minds set on writing a book. We've each drafted some chapters. Both of us have had to set it aside. I've told Sam that next year I'm going to do that.

VDT: Well, you continue to be a font of ideas. And now you're going to go play your tennis that gives you extra energy. Are these pictures of your grandchildren?

COALE: Yes, those are grandchildren.

VDT: How many children do you have?

COALE: I have just two boys. The younger has two daughters and those are them. Our older boy doesn't have any children.

[In response to Lunde's question in the 1979 interview, "Where did you first meet your wife?"]: I was a senior at Princeton. When I was an undergraduate, my family was poor; my father was a Presbyterian minister. I took the train to Princeton to enter my freshman year and never took the train again. I hitchhiked about 45,000 miles. I used to hitchhike home; I lived in Annapolis. I went through Baltimore all the time. One of my best friends in college had a first cousin who went to Goucher. He said I should meet her. So when I was hitchhiking through Baltimore, I looked her up. Her name was

Campbell. It was the best stopover of my life.

[Back to 1988 interview]: That's a joke [photo of a painting of a sailing ship]. There are terrible jokes in our family about the old song "Red Sails in the Sunset." For example: Why did the newly rich Indian oil millionaire want to join the local yacht club? Because he'd always wanted to see his red sons in the sail set. When we were sitting looking out over the sea in Honolulu, I would say, "Sue, look at what is over there." And she would ask, "What?" And I say, "It's jet trails in the sunset." She bought me that picture as a memento of these terrible jokes.

VDT: I heard a bit about that from Charlie Westoff yesterday. He said you're full of bad jokes. But they're cute jokes.

Are you ever going to write your own autobiography?

COALE: I don't have any plans to do that. It might be fun. Who knows?

VDT: You could talk into a tape recorder.

COALE: Well, I'll tell you what I'm going to do this summer, in addition to starting on this book with Sam. I'm going to learn word processing.

VDT: Oh, you're just now learning it. I'm impressed--Word Perfect [Coale showing book].

COALE: I haven't yet done a thing with it. I just got that book out. I've gotten fairly adept at APL and I do my own programming, but I haven't learned any word processing. I realize that although the Office is very nice about it, it really is not fully correct for me to continue to get the same level of secretarial service I got before I retired.

VDT: Senior corporate executives are supposed to be able to use their own computers now.

COALE: That's what I want to do. I want to learn to type. I can't type at all.

VDT: You never have? You've done everything by longhand?

COALE: And dictating. And all of my colleagues who've gotten into Word Perfect say that it is really so much more efficient than writing by hand, because you can edit it so easily.

VDT: Once you've learned word processing, what are you going to do with it?

COALE: Do all my own correspondence and my own manuscripts. I have a terrible story and probably shouldn't put it on tape. It is the Ron and Nancy typewriter, which has no colon, no period, and no memory.

VDT: I think we'd better stop at this point!

Additional note on Ansley's year ahead, in 1988.

COALE: After going to LSE for Bill Brass's retirement in June, my wife has persuaded me to go on a theater trip organized by the local university theater, where an expert in London lines up a bunch of

plays for us to go to. I did that once several years ago. Then in August, I go to Beijing for the IUSSP meeting on mortality in Asia. And then I just got a phone call from Massimo Livi-Bacci a couple of days ago, asking me to attend a conference in Firenze in December. I said, "Massimo, I can't write another paper." And he said, "No, no, we just want you to be chairman, get your expenses paid, and you come and chair."

VDT: What explains your particular love of Italy?

COALE: I stopped there my first trip abroad, on our way to India in 1955. My brother had been in Europe for several years and I was asking him where I should stop. He said, "Don't try to go to London, Paris, Rome, Athens, and so on. If you have only a week, spend it in one place. I would recommend Italy." We did as he suggested. We had to stay overnight in Paris and then went to Rome and spent four days in Rome and four days in Florence and just loved it. So when I got this advice about when you take a leave, get everything ready and go to Majorca, I said Rome was my idea of Majorca. I took my research with me and my wife and I studied Italian the year before we went. So I went back there.

VDT: Lovely. Great country.

OTIS DUDLEY DUNCAN

PAA President in 1968-69 (No. 32). Interview with Jean van der Tak at Dr. Duncan's home in Goleta, California, May 3, 1989.

CAREER HIGHLIGHTS: Dudley Duncan was born in Texas and grew up there and in Stillwater, Oklahoma, where his father, sociologist Otis Durant Duncan, was on the faculty of what became Oklahoma State University. He received all his degrees in sociology: the B.A. from Louisiana State University in 1941; M.A. from the University of Minnesota in 1942; and Ph.D. from the University of Chicago in 1949. He was Assistant Professor of Sociology at Pennsylvania State University in 1948-50, at the University of Wisconsin in 1950-51, and at the University of Chicago in 1951-56. At Chicago, where he remained until 1962, he was also Professor of Human Ecology and Associate Director of the Population Research and Training Center. He and his wife Beverly, whom he first met when she was his student at Penn State, were married in 1954. Beginning work together at the University of Chicago, they became one of the outstanding research teams in U.S. demography. From 1962 to 1973, they were at the University of Michigan, where Dudley Duncan was Professor of Sociology and Associate Director of the Population Studies Center. He was Professor of Sociology during their ten years, 1973-83, at the University of Arizona in Tucson, and again from 1984 to 1987 and then Professor Emeritus at the University of California, Santa Barbara. Beverly Duncan died in 1988.

Among Dudley Duncan's many other posts relevant to demography, he has been chairman of the Committee on Social Indicators of the Social Science Research Council, served on the Census Bureau's Advisory Committee on Population Statistics, and was a member of the Commission on Population Growth and the American Future. His honors include election to the National Academy of Sciences (1973) and receipt of the Samuel A. Stouffer Award in Methodology from the American Sociological Association (1977) and of the Irene B. Taeuber Award for Excellence in Demographic Research from the Population Association of America (1991). His long list of influential publications include some 20 books, among which are The Negro Population of Chicago (with Beverly Duncan, 1957), The Study of Population (coedited with Philip Hauser, 1959), Metropolis and Region (with Beverly Duncan and others, 1960), Statistical Geography (with Ray P. Cuzzort and Beverly Duncan, 1961), The American Occupational Structure (with Peter Blau, 1967), Socioeconomic Background and Achievement (with David Featherman and Beverly Duncan, 1972), Introduction to Structural Equation Models (1975), and Notes on Social Measurement (1984).

VDT: How and when did you become interested in the general field of demography?

DUNCAN: I was first exposed to that in an undergraduate course at Louisiana State University, taught by T. Lynn Smith. He later wrote his material up in his book, Population.

VDT: Your father was a professor of sociology at Louisiana State, is that it?

DUNCAN: Well, the year I was a senior there [1940-41], he was there completing his Ph.D. He spent just one year in residence at Louisiana to get his doctorate. He had done most of the work at Minnesota and failed his prelims. Then he went back and passed his prelims but couldn't get a dissertation accepted, so he had kind of given up and Smith invited to come to Louisiana and spend a year there and Louisiana gave him the degree. It happened to be my senior year at college, so I went

with him.

VDT: He'd had that long gap between his graduate work and finishing his doctorate?

DUNCAN: He had many gaps in his career. He was a country boy and had to walk four miles to go to high school. He got into college because World War I made it possible to finance his start at college. Then he was a school teacher for a number of years before he graduated from college. He finally got his master's degree at Texas A. & M., but his graduate work came to a crashing halt two years later when he failed to pass his doctorate prelims at Minnesota. He took a one-year job at Louisiana at that time and then moved to Oklahoma A. & M. College--later Oklahoma State University--which is where he stayed the rest of his career, and life.

VDT: So you had taken a course with T. Lynn Smith?

DUNCAN: Yes. I went to Minnesota the next year, 1941-42. That was the fall that the Japanese bombed Pearl Harbor. I was then almost 20.

VDT: You finished your undergraduate degree very early.

DUNCAN: Yes, I finished at age 19. So I was foreseeing being drafted and I had only the one year. In the spring, my Dad gave me some tables that he'd compiled down in Oklahoma on birth registration statistics. He had age at first birth and he'd classified the women according to where they lived, in urban areas, open country, or rural farm areas. He did that work with the Oklahoma vital registration people and then gave me the tables to use for a thesis. That was sort of a demographic thesis. It wasn't very good, but I published a little paper on it in Rural Sociology, the first paper I ever did. It came out after I was in service ["Rural-Urban Variations in the Age of Parents at the Birth of the First Child," Rural Sociology, March 1943].

VDT: How did you happen to choose to go to the University of Minnesota?

DUNCAN: I got an assistantship there and Bill [William H.] Sewell was a great influence on me. He was my father's colleague at Stillwater [Oklahoma], but my Dad had met him in 1935 when he was back at Minnesota to take his prelims and try to get started again on a degree there. He hired Bill for Stillwater in 1937. The Sewells were very close friends of the family. I did babysitting and housecleaning for them and Bill would talk to me about his work. I took my first course in sociology with him, a course on rural sociology.

Then in my senior year, I went off to Louisiana with Dad. He urged me to come back to Oklahoma A. & M. College, where he was head of the department of sociology, to do a master's degree, but Bill advised me that that would not be a good idea and I should look around for other opportunities. I'm sure he recommended me in a warm way to Minnesota, so I got a nice assistantship there. It paid \$600 and tuition; that was ample to live on for the year.

VDT: You managed to finish a thesis, get a paper out of it, do all the coursework, in two semesters?

DUNCAN: It was three quarters. I wrote the thesis mainly in June and July and had to go back in August and take one additional course and have my oral exam. I went into the service in October 1942 and was in the service until January 1946.

VDT: A.U.S., it says in Who's Who.

DUNCAN: Army of the United States. That's the official name of the World War II army to distinguish it from the regular army. I had various assignments, training programs and so on. I never went overseas or had any combat or significant military experience, to tell the truth. They passed the GI bill and after you came out you could go wherever you wanted to, so I went to Chicago.

VDT: Why did you choose Chicago?

DUNCAN: It's hard to say. I've written a bit about that in my autobiography [Autobiographical Statement, prepared for the National Academy of Sciences, January 1974, on being elected a member of NAS; with addendum, August 16, 1983]. I was interested in the sociology of knowledge [Louis Wirth] and various things. It wasn't particularly demography that I went there for. I took my major in social psychology and methods and theory in the Chicago Ph.D. program. After I'd finished my undergraduate degree and my master's with that exposure to demography, I still was not committed to demography at all. I had other interests, and still do. But I studied with W.F. Ogburn and found him the most congenial professor I had. So I ended up doing a dissertation with him. He had a project and was able to hire me for the year that I used to write my dissertation. That was on a topic designated for his project.

VDT: Was that An Examination of the Problem of Optimum City-Size, published as a book in 1949?

DUNCAN: It was actually published only in 1980 by Arno Press in one of those projects to reprint dissertations. It's just a photocopy of the typed dissertation.

VDT: Was William Ogburn's project, which allowed you to do your dissertation, connected with the Chicago Community Inventory?

DUNCAN: No, Ogburn had no connection with the Community Inventory; that was Louis Wirth and Phil Hauser. Ogburn's was Carnegie money for social effects of technology and he got interested in the possibility that if we dispersed cities they would be less vulnerable to atomic bombs--sort of a naive idea. He thought one angle of that was to see what would be lost if you broke up large cities, made them smaller somehow. So he asked me to tabulate characteristics of cities according to size. I looked up a lot of fugitive tables; I didn't do much original compilation myself. I just assembled all the material, made it into a dozen chapters or so, and called it a dissertation. It was a descriptive thesis, but it still has some interest today. Julian Simon wrote me recently, saying he's going to write on correlates of population density and he thought my material would still be useful to him. So that was a sort of demographic thesis, though not entirely demographic. But I still was not committed to demography. I did not have any firm intention of specializing in that.

When I got on the job market, I had various offers, but by far the best was from Penn State. They said, "What could you teach?" I listed several things and finally said, "Well, population." They said, "Oh, that's something we need," so I got designated to teach population. I began offering a standard undergraduate course in population problems and a graduate course in methods of population research. I was at Penn State for two years, 1948 to 1950, then I had one year at Wisconsin, 1950-51, and then I went back to Chicago on the faculty in the summer of 1951.

VDT: Was that where you always wanted to be?

DUNCAN: I did not have any such aspiration. I thought Penn State was a very fine job and would

have been prepared to stay there happily the rest of my life. I had a part-time research job at the Agricultural Experiment Station which I thought was very satisfactory. Bill Sewell had such a job at the beginning of his career. That was demographic research. I had an unpublished study on differential fertility and a small published article on fertility of the village population ["Fertility of the Village Population in Pennsylvania, 1940," Social Forces, March 1950], which was patterned after a paper that Lynn Smith had published, showing how you can use census tables to get child-woman ratios for the village population.

VDT: Down to that low a level--census tracts?

DUNCAN: No, it wasn't census tracts in those days. The census showed small incorporated places. You could get tables on age-sex distributions for very small places and classify them by size and their characteristics to infer fertility.

VDT: Why did you leave Penn State if you enjoyed it?

DUNCAN: Because Bill Sewell was at Wisconsin and he got a better job for me. A fine hand; he influenced most aspects of my career. He went off to the Navy during the war and when it was over, he had an offer from Wisconsin and never came back to Stillwater.

VDT: I certainly think of him as connected with Wisconsin.

DUNCAN: That's been his lifelong location. The only other job he had was at Stillwater.

I was in Wisconsin for a year and W.F. Ogburn wrote and asked if I would like a job at Chicago. I was disinclined to take it. I thought Chicago was a very rough place and I didn't particularly want to go there.

VDT: The city?

DUNCAN: The city and the university. In terms of the competition in the faculty and the scholarship standards and so on, it scared me. But I talked to Bill Sewell and he said, "I wish you would stay here, but I think you should go for the sake of your career." So I went. And as Robert Frost said, "That made all the difference." [Laughter] I'm sure, though, you can't prove that.

VDT: You came to Chicago in 1951 and you and Beverly were married in 1954. Did your book with Beverly, The Negro Population of Chicago [1957], and Social Characteristics of Urban and Rural Communities, 1950 [with Albert J. Reiss, Jr., 1956] grow out of the Chicago Community Inventory, that became the Population Research and Training Center?

DUNCAN: Social Characteristics of Urban and Rural Communities, 1950 was a 1950 census monograph, sponsored by the Census Bureau and financed in part by the Russell Sage Foundation, I think.

VDT: You were young to be picked to write that.

DUNCAN: That was based partly on my doctoral dissertation. In 1950 they put out a special report on urbanized areas, classified by size. I guess they thought that since that was a new topic they wanted a monograph to report those data and I guess they knew about my dissertation and asked me to do that.

I asked my friend and classmate Al Reiss to work with me on it. That had nothing to do with the Community Inventory, except that I did the work there and used their facilities.

The Negro Population of Chicago was a project done at the Community Inventory, which was established early in the postwar period by Wirth and then when Hauser came back he sort of took it over.

VDT: He was asked to come back and be the head of it [see Hauser interview, above].

DUNCAN: Yes. He was on the faculty as a professor, of course. When I went to Chicago, he invited me to work on a research project for which he'd gotten funds from the Air Force. That lasted two years and that was sort of what cast the die, which turned me into a human ecologist and then demographer, for a decade or so.

VDT: That project was working for the Human Resources Research Institute of the Air Force. Phil Hauser [in his interview] said you did the methodological work for that and helped develop segregation and concentration indexes.

DUNCAN: Beverly and I did practically all the work, if you want to know the truth of the matter. There were a few other things done that were okay, but we were the backbone of that project. Phil brought in the resources and oversaw the thing, but he was not active in the research itself. He never published anything on it, except as coauthor of a short summary report. We published some things on segregation indexes, a variety of articles that were related to that.

Then the Negro Population was a bit later. After the Air Force project was over, the Community Inventory didn't have any significant financing. So Phil went to the City of Chicago--I can't remember which agency--and a guy there whom he knew agreed to put up some money to keep the Community Inventory in business. We did little reports; that was the formula. Beverly did the bulk of that work. One of the things they were interested in was the changing distribution of the Negro population. We did one short report and then extended that into the complete book that was published.

VDT: Phil Hauser in the interview I had with him last October said that book was a trailblazer. It indicated that Chicago was probably the most segregated large city in the northern U.S.

DUNCAN: We did not make any comparisons of cities in that book. That was done later by Alma and Karl Taeuber, who did the comparative study of segregation indexes and so on.

What we showed was not so much the degree of segregation as the consequences of segregation when the Negro population is increasing rapidly--what happened when the blacks flooded into the city but the residential areas did not expand to admit them. We showed that in many census tracts there was what we called "piling up." You had increasing population over the 1940-50 decade without any increase in the housing stock, just more people going into the same space. And that was in effect heightening segregation; the fact that you had intensification of the black occupation of their part of the city as compared to the rest of the city. We explored those processes in minute detail by comparing the 1950 and 1940 census-tract characteristics, tract by tract, using regression lines to describe the average changes in groups of census tracts according to the percentages of black population at the beginning and end of the decade. We worked out a typology of tracts that we called "piling-up tracts," "consolidation tracts," and "invasion tracts"--something like that.

VDT: Phil Hauser said Beverly had been his student and she was a superb student and researcher. He said, "She and Dudley were one of the best research teams that ever existed in the U.S. or, for that matter, elsewhere. They did beautiful work."

DUNCAN: Well, that's true. I'm not embarrassed to say that.

VDT: Great! Now about The Study of Population [1959], which I think of as the bible of demography, in which you described demography as a social science for the National Science Foundation. You and Hauser wrote the first five chapters on "Demography as a Science" and you wrote the chapter on human ecology. What do you think/remember about your part in it?

DUNCAN: You said most of it. Harry Alpert wanted a review of a discipline that would show that there is science in social science. He went to the National Science Foundation as a kind of consultant and was asked to look at the social sciences, which had been deliberately excluded from the National Science Foundation when it was established. Actually, I looked into that a bit and wrote an analysis of the way it worked; that was probably about 1947. Harry was there and despite the fact that the social sciences weren't supposed to be covered by the program, he was investigating the possibility of gradually introducing social science work. He decided to sponsor a survey that would show there was some branch of social science that was worthy of attention as scientific. He thought of demography and asked Phil and me to look into that. I don't remember the details of how the project got started. But he put up money which enabled us to give honoraria and pay for translations and other expenses of preparing a large collaborative book of that kind. So we thought of names and wrote to people and put the whole thing together. It turned out to gargantuan and uneven in quality. Then Phil and I had to write some kind of introduction, so we wrote "Demography as a Science."

VDT: And you wrote the chapter on human ecology. By that time you were a human ecologist?

DUNCAN: Yes. Well, I started teaching that course; that was one of the teaching duties I took on about 1952. Amos Hawley's book had come out about that time [Human Ecology: A Theory of Community Structure, 1950]. It seemed to put a lot of things in perspective that I hadn't been able to assimilate before, so I was very enthusiastic. I worked very hard and developed a good course. And we thought of the work at the Community Inventory as work in human ecology, in the old Chicago tradition, the Park tradition, both in terms of methodology and statistical techniques and in terms of the idea of ecology itself.

VDT: The old Chicago tradition of . . .?

DUNCAN: Robert E. Park--and Ernest W. Burgess, of course. They had talked about human ecology from the 1920s and that was thought of as one branch of the Chicago tradition.

VDT: Let's talk a bit about your colleagues of Chicago. You've talked about Hauser; you gave a few insights. Many people have said Hauser was so good at getting money and putting people and projects together--an entrepreneur, in a sense, of the field. Do you feel that?

DUNCAN: Well, he was very good at administration. He got other people to do the work. That was his philosophy of administration, he liked to say, "to give his assistant a headache." [Laughter]

VDT: He said he's had his name on 32 books, chapters in 50-60 other books, and over 500 articles. But I guess they were mostly collaborative efforts?

DUNCAN: Not all of them, but the things he did with us were collaborative. He himself never did a

research project, in the focused sense of that term, at the Chicago Community Inventory or the Population Center. He did a lot of the kinds of volumes that he's famous for, conference proceedings and symposia, things of that kind. He was good at organizing those, recruiting talent, involving people, editing the stuff. He's a very fluent writer and speaker, of course. Brilliant guy. If he hadn't been so good at that public relations stuff he could have been a great researcher. I don't think he's a great researcher; I think he's a very good synthesist and analyst, but largely using results worked out by other people. He'd started that as an official of the Census Bureau. He was in the demographic program [population division] for a while, but he quickly moved up the echelons where he was no longer directly responsible for the production work, but for administering and planning. He got into the fairly high-level policy advisory positions before he was done, working with Henry Wallace and so on.

He came back to Chicago while I was still a student, about 1947, and I sat in his course on population. That's something I didn't mention before. That was an influence at Chicago, though I still didn't see it as a specialty. I thought since he was new on the scene and I was tired of those other guys, I would take a course with him. It was not a great course. He used his old course notes that were heavy on the history of population controversies and doctrines and spent about ten weeks on Malthus, Marx, and Myrdal, all that stuff, and then about the last two weeks, we started learning about net reproduction rates, techniques, and so on, which was the part I was interested in.

He was a dynamic, very charismatic fellow. He was attracted to me when I was a student and he was very supportive and once I was back on the faculty, he turned into a great friend. The Hausers were very close friends of the Duncans for that entire period. We were in their house not less than once a week. We enjoyed them so much.

VDT: Who were other close colleagues at Chicago?

DUNCAN: At the [Population Research and Training] Center, Evelyn [Kitagawa] was the other mainstay, along with Beverly. The two of them did not have faculty positions. Phil and I were teaching, so we were only part-time at the Center, but Evelyn and Beverly were there full-time. And Don Bogue, of course, joined us as a member of the faculty.

VDT: Tell me about Evelyn; she is one I have yet to interview.

DUNCAN: She's a very competent, level-headed person, a very good friend. Her husband is a minister and he married Beverly and me and Evelyn had a reception for us after the wedding, so that shows you that they were very good friends.

She had worked in the War Relocation Authority during the war and then come to Chicago and done her doctoral dissertation. She finished a bit after I did, so I didn't know her as a student. But when I came back on the faculty three years later, she was involved in the Community Inventory and working full-time. She worked at that time on the labor mobility study and Al Reiss was working with her on that. I guess that kept up for three years and then she went off into other kinds of study. We never collaborated directly; we were just associates in the running of the Center and so on. We saw each other twice a day at coffee breaks. I have enormous respect and affection for Evelyn. She was really one of the very few friends that Beverly ever had. Beverly didn't make friends very promiscuously, and Evelyn was one.

VDT: You were on the dissertation committee of Nathan Keyfitz. He had been at Chicago as a student only briefly and came back in 1952 to defend his thesis. He didn't come to the faculty till 1963, when you had gone to Michigan.

DUNCAN: That was the only time I intersected with him, except on visits. We were never in the same location as coworkers.

VDT: Repeat that nice story you told about his dissertation defense, when he was so much older than most students.

DUNCAN: I don't recall whether I was on his dissertation committee, but I was part of the examining committee. They pressed people into service for the oral exam. I'm sure I must have read some of the dissertation--and the abstract, of course--and I was perplexed by the methods, which were a bit fancier on the side of design and analysis than I had learned. I'm sure I struggled to ask questions that would not be too stupid, but I was really adrift. After the examination, Everett Hughes was telling somebody how it had gone and he said, "Nathan was very kind; he didn't flunk the committee."

VDT: Somebody else has told me that the students were all saying, "Who was examining whom?"

DUNCAN: Nathan was not presumptuous in that way. He was very respectful of all of us, even the least qualified. He knew we were all out of our depth. He's a very fine gentleman; he would never be presumptuous in a situation like that.

VDT: Were you and Beverly and Evelyn more or less running the Population Research and Training Center while Phil Hauser was out doing other things?

DUNCAN: On the day-to-day basis, we were there. He would come in from time to time; he would be in Washington or Tokyo or any place around the world. He was very faithful about meeting his classes, but that still wasn't at the Center. When he had a moment, he'd drop by and sign letters and kid the girls. And if money was needed, he would get busy and do something. And he would talk with us about what we were up to. But for the most part, he wasn't doing the day-to-day work. I guess he saw his job as keeping the organization in business and getting some good people to do the work.

VDT: Did Third World students, like Mercedes Concepcion, Visid Prachuabmoh from Thailand, and Iskander from Indonesia, start to come in the 1950s while you were there?

DUNCAN: Yes, they were one of Phil's specialties. He'd make these contacts with the statistical offices and universities in these countries and then write and ask them to send students. I was never particularly involved in that. I can't remember having any of those students as someone who worked closely with me. That was more or less Phil's baby, and later on, Don Bogue.

VDT: Don Bogue was still technically on your Center staff at that time, even though he was branching off with his Community and Family Study Center too. Isn't that correct?

DUNCAN: He was at Scripps Foundation in Ohio, with Whelpton and Thompson, and he was sort of commuting for a while. I think he came once as a visiting professor for one quarter, something like that, to fill in.

You see, Chicago's department of sociology kind of fell apart. Wirth died; Blumer left for California; Ogburn and Burgess died. That left Hauser, Hughes, and Lloyd Warner as the full professors and then some other people. They were floundering and trying to get courses taught. Joe Spengler came at that time; that's how I happen to know him. But he just came as a visitor to fill in by teaching. And Don was doing that. After that was over--maybe it was partly at my urging--we

recruited him to be a regular member of the faculty. But he always wanted to do his thing and he never cared to involve himself directly in the immediate affairs of the Community Inventory and the Population Center. So after a while, he ended up setting up his own organization, the Community and Family Study Center. I can't tell you the details of that.

VDT: I know there were some difficult relations.

DUNCAN: Well, there were some. We tried two or three times to have a cordial working relationship, but somehow it didn't work. You had people with strong egos, his and mine, strong wills, and so on. We didn't hit it off the way Phil and I did. But I still had a great friendship for Don. He and Betty were friends of Beverly and me; we'd go out with them to shows.

VDT: I'd like to talk about the development of your different research interests. Here's another quote from Hauser. He said: "Duncan developed into--and I think still is, although he's retired now--one of the best scientists in sociology, including demography. I say that with conviction. He's an absolutely superb researcher and methodologist and much of it through dint of his own effort and concentration and personal development, more so than through formal training, though he had enough formal training on which to build." You've already given a little insight into that.

Also, Nathan Keyfitz in the interview he did for this series said that "Duncan's work always inspired me--his ability to learn a brand new field, his ability to get at the empirical aspect of a subject, and his ability to cut through nonsense." I'd like to ask how you got into model-building. Keyfitz mentioned that you had more influence than any other person in developing the use of models in demographic research.

I could go on and give one of your own quotes.

DUNCAN: Go ahead.

VDT: For example, your basic model, set out in The American Occupational Structure [with Peter Blau, 1967], for the study of intergenerational occupational mobility--the idea that the education and occupation of the head of the family of origin, and also the family size of the family of origin, influence the son's education and in turn the son's occupation. That's all laid out neatly in--well, it's in several places. You wrote, the three of you [Duncan, David Featherman, and Beverly Duncan] wrote in Socioeconomic Background and Achievement [1972], which was the sequel to The American Occupational Structure, this quote, which I like very much. I thought perhaps this is your philosophy of model-building: "A good model serves not only to rationalize and interpret a pattern of empirical relationships but also to raise questions whose answers require further empirical inquiry and/or modifications of the model. Thus the long-run course of research in an area of inquiry may be guided, more or less explicitly, by an incremental strategy of model-building" [page 9]. Sounds like you?

DUNCAN: I wrote that and I recognize those words. That's not necessarily the way I'd want to put it now, so I'm not clear whether you want me to think about how it was then or how I think about things now.

VDT: That's interesting that you say you've changed. However, then.

DUNCAN: Well, it was a long story. I got back into the stratification area by a kind of accident.

VDT: Why do you say "back"?

DUNCAN: I'd done a little study of that at Penn State, in a rural community, which the head of the department of rural sociology asked me to do. He'd started the study and the student who was going to run it had to leave town for health reasons. So, on a week's notice, I was asked to take charge of that study, and I did it with my assistant, Jay Artis. That was published in an Experiment Station bulletin and an article or two. But I didn't have any way of pursuing that interest in stratification for a number of years; I crossed over into the human ecology area.

But along in the mid-1950s, [Iwao] Moriyama and [Lillian] Guralnick at the National Office of Vital Statistics, as it was called then, asked me to develop a classification of occupations that they could use to code death certificates, which they thought would be useful in studying socioeconomic differentials in mortality. So I took on that project; I forget how we got funding for it. My friend Al Reiss was simultaneously working on the completion of a study of occupational prestige that had been started at NORC in 1947 under the leadership of Paul Hatt, who had died. It occurred to me at some point that I could relate those two things by using the socioeconomic census data to predict the prestige scores of occupations. I worked that out and that was included in Reiss's book ["A Socio-Economic Index for All Occupations," Chapters 6-7, in Albert J. Reiss, Jr., and collaborators, Occupations and Social Status, 1961].

VDT: You were using 1960 or 1950 data?

DUNCAN: 1950 detailed occupation characteristics. We ransacked that book, made summary measures for every detailed occupation on all the characteristics and then started culling through those data. But when I saw the possibility of relating it to prestige, that changed the emphasis there and dictated the way in which the results were finally reported.

Once I had that, I realized that if you had a set of numbers for scoring occupations, you could use the scores for correlational analysis, the way Ogburn had taught me to do it. Kitagawa, as I mentioned, had been working on the labor mobility study and they had mobility tables that they had put together. So I got Bill Hodge to work with me and we used their mobility tables. We coded the son's occupation and the father's occupation and calculated the correlation coefficient between the two. We weren't the first to do that, but almost the first. Then we thought about introducing other characteristics, which were available on the labor mobility schedules: the education of the guy, whether he was a veteran, and age, to get a cohort breakdown. We published a little paper called "Education and Occupational Mobility" [American Journal of Sociology, May 1963], which had a path diagram in it, a very rudimentary thing, just a three-variable diagram--father's occupation, son's education, son's occupation--a little triangular pattern. That, I think, was the first honest path diagram in sociology. There was some mention of path analysis by Stuart Dodd in his Dimensions of a Society, but he didn't use it; he just mentioned that it existed.

I had inherited from Ogburn a set of reprints, which included a paper by Sewall Wright, the inventor of path analysis, "Statistical Methods in Biology." I started studying that, but I wasn't making much headway. It was difficult for me. At the same time, [Hubert] Blalock was working on causal inference from non-experimental data, following the lead of Herbert Simon. He was about ready to publish his book on that and I wrote to him that "I think path analysis is doing much the same thing; it's the same general idea." He wrote something to the effect that it was a different approach. But it is the same approach. Simon came at it from econometrics; Wright had come to it out of biometrics. Wright had even done work in econometrics which I don't think Simon knew about. It was a kind of independent, emergent thing. About the time Blalock's book was published, I saw how to put the thing together and wrote the article on path analysis.

In the meantime, Peter Blau had been trying to start a stratification study. He was a member of

the International Sociological Association committee on stratification and they wanted to start studies in every one of the major countries. Blau was to try to get one going in the U.S. So he'd talked with Clyde Hart at NORC about doing a study there and they had tried to design a study and get financing, but were having no luck. Then Phil Hauser said to Peter, "Why don't you get the Census Bureau to do it as a supplement to the CPS [Current Population Survey]?" At that point, Peter figured he would need help to work with demographers, to understand what the CPS was and how to use it. So he asked me if I would like to work with him on that. We had taught a course together; we were young colleagues and we had respect for each other, although our styles of work were quite different. I didn't have anything better to do and said, "Sure." So we went to the National Science Foundation and got a very sizable grant for the time and commissioned the Census Bureau to put a supplement into the CPS of March 1962.

That's how those things were sort of happening in parallel. It's hard to say now what preceded what and how one idea led to the next one.

VDT: You say your study with Peter Blau, The American Occupational Structure [1967], used questions on the March 1962 CPS?

DUNCAN: Yes. They would not give us access to the tape; at that time, it was thought to violate confidentiality procedures. So they said they would tabulate the tables we wanted.

VDT: Could you give me an example of the questions?

DUNCAN: What was your father's education when you were about 16 years old? How many siblings do you have? What was your oldest brother's education? What was your first job? There was a very small number of questions, maybe a dozen.

VDT: This was the only time such questions were asked in the CPS?

DUNCAN: That was the first time. They were subsequently asked in 1973 when the replication was done, but I had nothing to do with that. We started it at the Bureau, so that was an innovation there. And that was at Phil's suggestion.

But I had not done that with the idea of using path analysis; that was a separate thing being pushed simultaneously. At the time we specified the tables, I still did not have the idea of using path analysis. It was sort of by accident that we specified a lot of two-way tables, which could then be used to calculate correlation coefficients, and then we made them into input to path analysis. That was not premeditated; it was an afterthought. I had different ideas, like indirect standardization; I wasn't sure how I was going to do it. But the path analysis came up and Peter was enthusiastic about it, so we decided to invest in that, although we also used other methods--multiple classification and so on--in the end.

So that all kind of came together. Everything in my work from the late 1950s to the mid-1960s, that whole decade, went into stratification primarily. And it happened very fast.

VDT: I have an interesting side comment on that. When I interviewed Charlie Nam for this series, he said that at the Census Bureau about that time, he was also working on developing indexes or data for studying intergenerational social mobility.

DUNCAN: Yes.

VDT: He said he was using only census data. I think he implied that you were using some non-census data, meaning the NORC data, which Reiss had. He gave the impression that you were competitors in a sense, working in the same field. But he was very pleased that you sent him a copy of the Albert Reiss book, Occupation and Social Status of 1961, in which you had two chapters, and inscribed it: "From one SESer to another." He said, "I treasure that." Maybe you didn't get the impression, but he felt you were a bit rivals.

DUNCAN: Well, there were a lot of people doing that. The guy who did it first was the Canadian, Bernard Blishen. He had done it with census data for Canada. Then Don Bogue did it, using census data and some kind of factor multiple techniques to weight education and occupation. August Hollingshead had gotten out his two-factor index of stratification. So everybody was doing it. The novel thing in mine, which sold it I think, was using the census data to predict the prestige score, because the prestige score had a kind of aura of mystique about it.

It ended up that all those indexes were just different versions of the same thing, because they were using the same basic inputs, to wit, the income level and education level of the occupation. I think that's sort of a given; you can summarize it in alternative ways, but it's the same stuff however you summarize it. There are some details about how it's done that make some difference, but it's all variations on a single theme. There were a lot of people and I'm sure they came at it from different points of view and independently, in part. I knew about some of that work before; I think I even mentioned it in the Reiss book. I did not know about Nam's, I think, until his was under way or maybe even done; I'm not sure.

VDT: It came out as a Census Bureau study, I think. He was trying to get the Census Bureau to get more into that field and wasn't getting much encouragement.

DUNCAN: I didn't have anything much to do with that. They had a composite measure of social stratification--that was not the same thing as this occupational measure--which I took strong exception to. I wrote a bitter, devastating--I guess it was devastating--critique of that to the Census Bureau. It died. I don't know whether it was because of my influence, but it didn't come to anything. They were experimenting with a composite measure, which would combine education, occupation, income and so on.

The other thing is occupational status. Did Nam tell you about why they left out the farmers from his publication?

VDT: No. Tell me that story.

DUNCAN: If you look in Nam's first publication, he lists all the occupations except "farmers and other farm occupations". They were simply deleted, because his technique showed their very low status and Conrad Taeuber said, "You can't publish that; the farm congressmen will be on my neck." So they were calculated by the same formula, but not published.

VDT: Interesting! Was that about the time you were on the Census Advisory Committee on Population Statistics?

DUNCAN: 1964 to 1973, it says here [in his curriculum vitae].

VDT: I have a note in my files, which can't be right because your being on that committee came later, that you tried to get a question on religion into the 1960 census. Did I put that in the wrong file?

DUNCAN: We had a COPS, Committee on Population Statistics, in PAA and we were advocating inclusion of that question for the 1960 census. And I think that it was a suggestion of mine that made some religion statistics available. They had done a sample survey of religion in 1957 [March 1957 CPS], but it was suppressed. I think there was a table in the Statistical Abstract, but that was all [Paul Glick and Wilson Grabill put out one Census Bureau report, but a second more-detailed report was suppressed; see Glick interview above]. In a [COPS] meeting one day, I said to Con, "If somebody used the Freedom of Information Act, I wonder if those statistics could be pried out of the Bureau." He said, "Well, I'll have to look into that." And the first thing you know, it was made known, though not widely advertised, that if you would pay for the cost of reproducing the tables, you could have the tables from that 1957 survey. I had them; I no longer have them. I'm sure they exist in various libraries and archives. My suggestion, I think, led him to think of a way in which that could be accomplished without causing too much trouble.

But we went ahead and made a recommendation for inclusion of a religion question in 1960 and there was a lot of serious discussion at the Bureau, but it was very firmly axed in the end.

VDT: By...?

DUNCAN: Well, I can't tell you that.

VDT: The Rightists, the Catholic forces, or what?

DUNCAN: The Catholics, as I understand it, were not opposed. They would like to have that. What they did not want was another Census of Religious Bodies, which would show the value of church property and things of that kind. But they wanted to know about church membership.

VDT: Was that in the 1957 survey?

DUNCAN: No. That was the Census of Religious Bodies. The last one was taken in 1936. That was supposedly on a decennial basis, [1906, 1916], 1926, 1936, and another one was scheduled in 1946, but it was never done.

VDT: Why the Census Bureau?

DUNCAN: That's an aspect of the history of Census operations you might not know about. It was not a demographic survey. It was a questionnaire to the organizations, churches, that the officials of the church were supposed to fill out and return. They asked about membership, but it was not a population survey; it was just their own records. Those data were used in the early days of research on religion and demographic people would compile the memberships by counties and correlate them with various characteristics.

The 1957 [CPS] survey was a different idea. It used the usual sample survey procedure and asked people, "What is your religious preference?"--something like that. That gives you an enumeration not of organizations, churches, but of people according to their preferences. Bogue had previously compiled stuff, using NORC and other private surveys to get essentially the same information. There were no new facts, except more detail and a larger sample in the CPS data.

VDT: But the U.S. Census Bureau has never been persuaded to put religion in the decennial census.

DUNCAN: Well, the Bureau has been persuaded, but not the people who have to put up the money.

I'm not sure whether the Bureau would want to do it now. I'm not sure I would want to do it. I think that was a mistake.

VDT: Why?

DUNCAN: I think the Census has no business asking people things that are matters of their private conscience. My views on that have changed a great deal. I don't trust the Bureau any longer.

VDT: Oh, you're one of those.

DUNCAN: It's politicized. I was naive and I was made more sophisticated by the run of events. I don't think we should use the Bureau for those kinds of things.

VDT: What about the standard questions that are now asked? Well, they will be in 1990--they'll ask whether you're Hispanic or not--on the short questionnaire.

DUNCAN: I don't know about that. Objective characteristics are simply matters of public knowledge anyhow. Your age is a matter of record because your birth certificate's on file, so you go and ask somebody his age, that's just a convenience, ascertaining information that's already there. Your income is something you have to report to the government for tax purposes. Questions that are in interactions with strangers, such as "What's your job? What kind of job do you have?", are not intrusive. But if you ask a person, "What is your religion?", or you ask, "Are you very happy, pretty happy, or not so happy?", you are asking things that the government has no business asking people. People should not be required to report to the government on things like that. The census is, of course, compulsory. And therefore people are being compelled to answer questions that in all conscience they might not find they should be asked.

I no longer have that great thirst for data I had when I was younger, that would lead me to override these kinds of considerations of the ethics of statistics. Quite the contrary.

VDT: Can you think of some data that you were thirsting after, recommending that they be collected by the Census, during the time you were on the Census Advisory Committee? Or any outstanding issues at that time, when you were that closely connected with the Census?

DUNCAN: We wanted them to get Social Security numbers and match records between the census and the Social Security. That, I think, foundered because of the impracticalities of it. It turns out that some people have multiple Social Security numbers and the matching is by no means easy or foolproof; it's a very cumbersome procedure. And that, of course, would have fed into the idea of data banks. Again, that is something I think we have to worry about more than I worried about it at the time. All I was thinking about at the time was how neat it would be if you could get good occupational mobility data with matched records.

VDT: You would have in the Social Security, where and in what jobs you were at different periods and you match it up with the census characteristics of that person?

DUNCAN: It just takes a little imagination to see what fabulous analyses you could do with that. But that presumes, (a) that it works, and (b) that it's politically feasible, and I think neither of those happens to be true.

Of course, the census is on very hard times; you've got a different kind of society now. It's not

at all clear that in 1990 people will submit to being counted. Or if they do have a count, whether it will get large fractions of people who are elusive, like transients, illegal aliens, and people of that kind.

VDT: Well, the 1990 census is a tremendous issue right now, although it's a fait accompli; they're going to still count those who are willing to be counted, whether or not they are illegal.

In your work on intergenerational occupational mobility, one place you summed it up was in your PAA presidential address, "Inequality and Opportunity" [Population Index, October/December 1969]. You said there that your studies had shown that up to that time--through the 1962 data--most occupational mobility in the 20th century had been upward; more than 50 percent had higher prestige occupations than their fathers. But isn't there some downward mobility now, because of the baby boomers, who cannot attain the positions of their parents because there are so many of them?

DUNCAN: I haven't tried to follow those trends since about the time of that presidential address; I turned to other matters. Then my former students, Bob Hauser, the nephew of Phil Hauser, and David Featherman set up this repeat study, which was done in 1973, and they were planning it some years earlier. When they started that, I redoubled my resolution not to have anything further to do with that line of work, so I would not be looking over their shoulder or in any way inhibiting them about having to conform to my pattern or work or anything like that.

VDT: Why do you say "redoubled"?

DUNCAN: I had other things I wanted to do by that time. I felt I was played out on stratification; had done everything I could think of and was sort of at a dead end. And my pattern has always been to work on something very intensively and satisfy my own curiosity or what I think I can do and then move on to something different, perhaps quite different, so as to get a fresh start.

VDT: That feeds into the next question. I wanted to talk about yours and Beverly's time at Michigan, that change. Did that also coincide with your change in research interests?

DUNCAN: The occupation study was started at Chicago, but it really didn't develop until I was at Michigan. The survey was done in the spring of 1962. I left Chicago in the summer of 1962 and the tables didn't come until 1964, something like that, after I was at Michigan, so all the analysis was done at Michigan. That was my main preoccupation, apart from one paper on human ecology, I think, until the late 1960s.

VDT: What had taken you to Michigan, the switch from Chicago to Michigan?

DUNCAN: Al Reiss was at Michigan by that time. He called up and said, "Would you like to come?", and I asked Beverly about it and she said, "Well, let's look into it." She wanted to get out of Chicago, I think--that's the way I remember it; she didn't remember it that way. But she encouraged me to look into it.

VDT: The city or the university?

DUNCAN: Well, the city [Chicago] was a horrible, dirty place. I guess we were just restive and Michigan seemed attractive; they seemed to have a new center being formed and a reason to come onto the scene there and it seemed to be a very lively place and a change in climate. When I went out there, I could see the stars at night. So it was the right place at the right time. And that was a very good period at Michigan; we worked very hard.

VDT: Tell me about your time there. The Freedmans were your colleagues, of course--another famous couple doing demographic research. Were you at all involved in Michigan's research to developing countries, to Taiwan in particular?

DUNCAN: No. We begin with the same story; that was Ron's baby, not mine. I didn't have much to do with the foreign students. He would bring those students in and involve them in his projects, which would often be in their countries. I would work with the American students.

VDT: You apparently were involved with the Detroit Area Study, because you used the data in Socioeconomic Background and Achievement [1972].

DUNCAN: That was the program of the department of sociology. Each year they did a survey on a different topic; they had a different faculty member do that. The topic rotated according to what people were ready to do something and proposed a topic.

I think it was around 1969 that Eleanor Sheldon asked me to write a memo about social indicators and what should be done there. I wrote that one of the things that could be done was to replicate old studies. I illustrated some replications that had been productive and established guidelines for that kind of work; then I surveyed the possibilities for good replications. One was the DAS [Detroit Area Study], which had been going at that time for about 17 years. I said replication of some of the early studies there would give us measures of social change over a period of 12 to 15 years, so that should be done.

I got so enamored of that idea that I followed it up myself and volunteered to be the faculty sponsor and talked to Howard Schuman, who was the DAS director, and he was interested. So we worked closely together on designing that study and he was a participant in that.

VDT: The DAS was phoning back to the same group of people every year?

DUNCAN: No, it was just an annual sample survey of metropolitan Detroit, typically a small sample, usually 600-700 cases, although sometimes larger if they had supplementary funds. The samples were independently drawn. There was no panel feature, no longitudinal thing, unless a particular investigator decided to include that feature.

VDT: So that was the time that you switched to your interest in social indicators, in the late 1960s?

DUNCAN: Yes. Late in the work on occupations, we got into the poverty theme. Pat Moynihan was having a periodic seminar on poverty and we got out the books on poverty research. He was out of his political job with the Johnson administration and found a job at the Harvard/MIT Joint Center for Urban Center. In that interim period, he was interested in poverty policy and formed this seminar and invited people to come. We ended up writing these books.

Related to that, I guess, was the social indicators thing, which had grown up with some people who made a report to the Air Force, of all things--Bauer, Gross, and Biderman--they proposed an initiative on social indicators and a committee was formed by HEW. I was on that advisory committee; it was chaired by Dan Bell. Eleanor Sheldon got into it with Russell Sage. She and Wilbert Moore got out the book on Indicators of Social Change [1968]; Beverly and I each wrote a chapter for that. After that work was done, she raised the question, what's next?, and asked me to write the pamphlet I've just described [Toward Social Reporting: Next Steps, Russell Sage Foundation, 1969]. That led me into the DAS. We were then writing about social indicators and measures of social change and so on.

Then Eleanor Sheldon got to be president of the Social Science Research Council and formed this project to set up a center in Washington [SSRC Center for the Coordination of Research on Social Indicators] and an advisory committee, which I chaired.

VDT: Eleanor Sheldon had been a student at Chicago so you knew her?

DUNCAN: Yes, we were classmates at Chicago. Actually, she had been the first director of the Chicago Community Inventory, before Phil Hauser was on the scene.

VDT: So she put you on the social indicators advisory committee of the Social Science Research Council?

DUNCAN: Yes, that was all schemed up between her and me. She talked with me about that and I agreed to do it on certain conditions, and we kind of did it my way.

VDT: What do you mean by that?

DUNCAN: Well, I said who I was interested in working with and who I was not interested in working with and how it would be set up and I helped her recruit the guy who directed it, Bob Parke. He had been on the staff of the Commission on Population Growth [and the American Future]. By the way, I got on that commission through Moynihan. That was how I got to know Parke and after that was over, I told Eleanor that he was a good man. It happened that she was doing this social indicators thing at that time, so she fitted him into that. I had a lot to do with getting that set up that way and I stayed with it for three years and then moved off.

VDT: Let's backtrack to Michigan, the colleagues you worked closely with--besides the Freedmans, whom you must have seen all the time, even if they were doing their Third World thing and you were doing . . .

DUNCAN: We were just doing things in parallel. We went to brownbags once a week and listened to each other talk, but that was the closest connection I had with the Freedmans, except for one or two papers. I did a paper on fertility, using his and my data together. We did a collaborative study [Duncan with Ronald Freedman, Michael Coble, and Doris Slesinger, "Marital Fertility and Size of Family of Orientation," *Demography*, No. 2, 1965].

VDT: Let's talk about your students at Michigan--and at Chicago. Norman Ryder said that you and Beverly were great mentors to the students at Michigan. And I've heard from others how the students would flock around you at PAA meetings, which meant others could see you were great mentors. Ryder said that the University of Wisconsin, where he started the population center, drew its staff from Michigan graduate students, because they were the best.

DUNCAN: That's true. We had a lot of good students. I hesitate to say who the good ones were, because there were so many of them.

VDT: Who were some that stand out? You mentioned Bob Hauser, for instance.

DUNCAN: Bob Hauser was not my student. He was in a course or two with me, but he was never on my project. The main thing I had to do with his dissertation was putting him in touch with Al Reiss,

who had done a study in Nashville, and I thought if Bob got hold of those data, it could be a beautiful study. And it was. So I helped him get into that and he modeled his work on the Blau-Duncan kind of stuff. But he had not been involved with my research before.

David Featherman, on the other hand, was my assistant, a very good assistant, starting about the time Blau and Duncan [American Occupational Structure, 1967] was finished on the followup project that led to the second book [Socioeconomic Background and Achievement, 1972]. He was the assistant throughout that. He was a social psychologist and I wanted to get more into the social psychological side, which was distinct from the demographic side of that work. We were exploiting opportunities to do that, so he was one of my main assistants there.

But a lot of those others--Jim Sweet, Larry Bumpass, Paul Voss--they are all at Wisconsin now; they were in my courses, but they were not my proteges or assistants.

VDT: Name a protege, other than Featherman--let's say both at Michigan and at Chicago. Of course, there's Beverly, who was your number one protege.

DUNCAN: Yes, she was my best student, sure. Hal Winsborough is another one now at Wisconsin. He saved their neck on their center. They were in real trouble because they had set up this Center for Demography and Ecology but nobody could run it properly, so I told Bill Sewell to hire Hal Winsborough; he was at Duke at that time. He'd been my student at Chicago and had collaborated on the book on metropolitan structure [Metropolis and Region, by Dudley Duncan, Richard Scott, Stanley Lieberman, Beverly Duncan, and Hal Winsborough, 1960].

Stanley Lieberman was a student of mine at Chicago, who's been prominent in later years. Richard Redick was one of the earliest. He worked in mental health statistics, spending his whole career with the government in Washington; very good worker, although not in research particularly. Robert W. Hodge, Bill Hodge, was my assistant on the measurement of occupational stratification, the first paper on path analysis that I mentioned. He died just recently, a very premature death, a great loss. So those were some outstanding people at Chicago.

At Michigan, I think of Hauser and Featherman, Bumpass, Sweet, Voss--I may omit names and I hate to single any out. Another good student there was Mike Coble. He went into computer work and he's still the backbone at the Michigan computer facility. He decided he didn't want to pursue academic sociology--for which I don't blame him--but was very good on the computer. He was my main help when I first went to Michigan--and all the way through, in terms of getting the job done. And Ruthe Sweet, Jim Sweet's wife, worked with me. She had been at Duke with Hal. She didn't stay in sociology either; she became a midwife.

VDT: A midwife! About as far as you can get . . .

DUNCAN: A very interesting person. Well, it's close to some aspects of our interests.

VDT: What is your impression of the problems they had with the two population centers at Michigan, the Population Studies Center and the Center for Population Planning?

DUNCAN: Ron started out to have a relationship with Les Corsa [Center for Population Planning]. They lived in the same building with us, but we kept our distance, and Ron gave up after a while.

VDT: You were connected with which center?

DUNCAN: Population Studies Center. Freedman was the founder and director of that. For a brief

period, I was director [associate director, 1962-73; director, 1967-68]. Then David Goldberg and a succession of other people have been director.

VDT: Who have been leading influences on your career? You've mentioned several, of course--Bill Sewell . . .

DUNCAN: Sewell first; Ogburn second; Phil Hauser third. Those three.

VDT: May I ask you about Ogburn? Phil Hauser said that you had done a series of monographs on him; what was that?

DUNCAN: It's just a collection of his writings, called William F. Ogburn on Culture and Social Change [1964]. It was published in the Heritage of Sociology Series of the University of Chicago Press. That project was run by Morris Janowitz and he asked me to contribute that volume. So I wrote an introduction about Ogburn, his career and accomplishments, and then had a selection of articles and chapter excerpts.

VDT: Was that before or after his death.

DUNCAN: It was after his death. I think he died in 1957; that book was about 1964. I did that after I went to Michigan.

VDT: What about J.J. Spengler, whom you mentioned early on?

DUNCAN: Joe came to Chicago, as I mentioned, as a visiting professor to help do some of the teaching chores. When I got to know him a little better, he said, "Why don't we do a reader?" I guess he met Jerry Kaplan, who had a series of readers in sociology. We talked and decided that to include all the things he was interested in and I was interested it would be more than one book. So we said, "Let's give Kaplan a real scare; let's propose two books." And Kaplan said, "Sure, go ahead." So we did [Duncan and Spengler, Population Theory and Policy: Selected Readings and Demographic Analysis: Selected Readings, both published 1956].

Kaplan was the entrepreneur who founded the Free Press, which later consolidated with Macmillan and he moved to New York. But in those days, he was just a shoestring entrepreneur on his own, developing as a publisher. He used these readers to build a market for his books.

VDT: What are your recollections of the work of the Commission on Population Growth and the American Future? You said that Moynihan was instrumental in getting you on the commission.

DUNCAN: Yes. He'd been taken on as the domestic adviser to Richard Nixon and Congress had passed the law that we'd have a population commission. That was Republican politics--a payoff to John D. Rockefeller, who wanted this. But Nixon wanted it done his way. So he had Moynihan be involved in the selection of people. The legislation said the President would appoint the commission. It was not exactly a presidential commission, where the President forms it and runs the whole show, because it was chartered by the Congress, in legislation prescribing what the commission would do.

VDT: Moynihan got you onto it. What's your impression of it? It was set up in 1970 [chartered earlier], because of the concern then with rapid U.S. population growth.

DUNCAN: Yes.

VDT: But by the time the report came out in 1972, U.S. fertility had dropped to replacement level.

DUNCAN: Yes, but there has never been a commission as successful in getting its recommendations adopted, has there? [Laughter]

VDT: Instantaneously!

DUNCAN: We argued about abortion bitterly. Had a terrible, knock-down, drag-out fight about abortion and then in 1973, the Roe v. Wade decision came out. So all the things that we were interested in seeing happen happened anyhow--but not because of us.

VDT: Of course, the only public attention it ever got was because Nixon attacked it [the report] for those two points--its proposal for legalizing abortion, which happened anyway, and [proposal for providing contraceptives to teenagers].

DUNCAN: You know about the television program? The Commission made a television version of that report that was aired on public television. The networks wouldn't touch it because they wanted their news departments to control the presentation and we refused to do it their way. Public television took it, but they would only take it if it was used as one segment of a program and then followed up by a debate. They brought in the Right-to-Lifers and one Jesse Jackson. And Jesse Jackson and I--on television--I remarked that I thought our last two presidents had been exemplary to the American people in regard to family size; both Nixon and Johnson had had two children. And Jesse said, "Oh, that Nixon--he's too cold." [Laughter] That was about all I said on that program. There was a lady who came in with a pickled fetus; she had a name for this creature and was just insulting to Mr. Rockefeller.

VDT: All of this was live television?

DUNCAN: Yes. There first was a canned program, narrated by Hugh Downs--not too good, but okay. Following that was a live debate. Mr. Rockefeller was there and not all the Commission, but a half dozen of us, and then these people who came in. Ben Wattenburg was one of the critics, the lady from the Right-to-Life group and Jesse Jackson, I remember; I forget who else.

VDT: Well, you really do remember the work of the Commission!

DUNCAN: The Commission was a great experience. Had a very good staff; Charlie Westoff [and Robert Parke] ran the staff.

VDT: The Commission itself met periodically during the two years?

DUNCAN: We met once a month; it was pretty often. We took the work very seriously and we had hearings in three different communities. I was at Little Rock and Los Angeles; sat there and listened to the Right-to-Lifers, the environmentalists, and so on--whoever showed up and wanted to bend our ear.

VDT: There's a great set of research papers, the seven volumes.

DUNCAN: Yes. It's a monumental study and it was right. It was time to slow down growth; it's not slowed enough yet. We didn't have any luck getting through a strong immigration thing. It was Grace Olivaros, a Mexican-American lawyer, who was very keen about the illegal problem. It wasn't looming so large at that time, but she saw the problem and wanted us to make a strong stand on trying to control illegal immigration, but we didn't.

VDT: It was a Mexican-American woman saying this?

DUNCAN: Yes, that was the interesting part of it.

VDT: That certainly has become the issue since then for the U.S.

DUNCAN: Yes. There were quite a few issues that we fingered in there--some of them a bit too delicately, but they're there.

VDT: What do you see as leading issues in demography over the years you've been involved? You got more into social indicators and so on, but think in terms of demography. The issues that the Commission dealt with--U.S. fertility, population growth was considered too high in 1970. Fertility, of course, declined to replacement level in 1972; it's been 1.7, 1.8 since 1976 [2.0 in 1989]. The baby boom and bust--has that been something that concerns you?

DUNCAN: No, it doesn't concern me a bit. [Laughter]

VDT: Why do you say that?

DUNCAN: As soon as I finished my PAA presidential address ["Inequality and Opportunity," 1969], I quit paying any attention to demography. I got into survey analysis, the social psychological stuff, and that's where I've been since 1971.

VDT: Okay, that answers that. You've also made it pretty clear that you've not been concerned with rapid population growth in less developed countries.

DUNCAN: Well, I think it's deplorable and a threat. I've always felt that, but I haven't done anything about it in either a policy or a scholarly fashion.

VDT: But you have been concerned with questions of social policy. The Petersens say that in your entry in their biographies of demographers ["Duncan's work exemplifies a fruitful combination of demography, urban studies, and mathematical techniques, almost always linked to questions of social policy," in William Petersen and Renee Peterson, Dictionary of Demography: Biographies, 1985, p. 285]. You felt it was important for social scientists to talk to policymakers?

DUNCAN: I don't know about that. I did not get into social science to reform the world or to have a role in setting policies. My agenda was to be a scientist and to try to make statements that had a little more authority and basis for them than the usual journalistic social critic might have. From time to time, I wrote sort of essays and studies oriented to policy issues. The last one was on nuclear energy. I wrote quite a good paper on that, which came out of my experience on the energy committee of the National Academy of Sciences.

VDT: Where and when was that paper published?

DUNCAN: That was back in the late 1970s ["Sociologists Should Reconsider Nuclear Energy," Social Forces, September 1978]. I was at Arizona at the time I was serving on that committee. I got discouraged with the way it was going and resigned. But I'd read enough about energy to have some thoughts on it.

I wrote a paper once on air pollution and human ecology. I wrote a paper on riots. I wrote a paper on "Inheritance of Poverty or Inheritance of Race?" [in Moynihan, On Understanding Poverty, 1969]. From time to time, I've written papers that tried to use my research experience to say things I thought I could say with a little more confidence as a consequence of having done the research. Too many social scientists want to put out thoughts on all policy issues, just give their thoughts. It seems to me that's not our role. Our role is to report what we think we've learned and try to help the community to make up its own mind about policy. So my philosophy there is a little bit different from some people, I guess.

VDT: Tell me something about your time at the University of Arizona in Tucson and here at Santa Barbara.

DUNCAN: At Arizona, we got out from under the necessity of running a large research organization. Both Chicago and Michigan were organizations, where you had to be responsible for a lot of other people's work. At Arizona, we just did our own projects. Beverly and I worked together some; we worked separately some. But largely with secondary analysis. We had the Detroit data collected in 1971 and I spent most of the next ten years analyzing those data; most of that was done at Arizona.

VDT: That was one year of the Detroit Area Study and you took a larger sample?

DUNCAN: Yes, we enlarged the sample; got money from the Russell Sage Foundation. They had been involved in getting the thing started. I tried to use that as a demonstration project for how you could create social indicators by replicating surveys and I pushed that idea later in the social indicators committee. We did a bit on that; we had something to do with archiving survey data. They're not very prominent ideas anymore, I guess, but they seemed like good ideas at the time. I wanted to do a thorough analysis of that survey and I kept working on that. I learned new statistical methods. This led into the last phase of my research, which has to do with Rasch measurement models.

Georg Rasch was a Danish mathematician who was asked to do some work in psychometrics and he came up with a new approach that has, in my opinion, profound implications for all social measurement. I was trying to exploit those ideas and get people interested in them. That's why I spent the last seven or eight years of my career largely on that, as well as the book on social measurement [Notes on Social Measurement: Historical and Critical, Russell Sage Foundation, 1984].

VDT: Of which I brought along a copy. I ordered it from the publisher and I hope you'll autograph it before I leave.

DUNCAN: I'll be glad to.

VDT: I also tried to get a copy of The American Occupational Structure but everybody was out of it, including the publisher, the Free Press of Macmillan, but they were expecting new stock this month--I presume of the paperback reprint of 1978.

That leads to another question. Which of your publications do you consider most important, and why?

DUNCAN: Notes on Social Measurement is the only book I care about anymore. The others were all right at the time, but they're obsolete. That one's not obsolete.

VDT: That's an interesting way to put it. Notes on Social Measurement came out in 1984, from Russell Sage. You did a number of books for Russell Sage, didn't you?

DUNCAN: Yes, I was astonished to see how many different things I had done for them.

VDT: And you consider the other books obsolete. Well, I'm not sure they are. The Free Press was out of The American Occupational Structure when I phoned a month ago, but they were expecting new stock, because it's still a standard.

DUNCAN: I can't say why.

VDT: I have to admit I've only heard of it. I didn't come out of sociology. I did demography at Georgetown, but I came out of history, many years ago. So I'm not a sociologist, but of course anyone even peripheral to sociology has heard about that book.

A final question on your career, both in demography--you said that career ended with your PAA presidential address--but also since then: What accomplishments in your career, looking back over it, have given you the most satisfaction?

DUNCAN: I'm not sure that it's given me any satisfaction.

VDT: Why is that?

DUNCAN: Well, it's hard to say. I don't know exactly how I became a social scientist. I think it was kind of following leads of my family. Bill Sewell was my role model and I've mentioned people who encouraged and helped me. I found I could do it. I was successful; it wasn't too hard. But I'm not sure it was worthwhile. I wish I'd done something else.

VDT: Like what?

DUNCAN: Almost anything. [Laughter] I don't know if I could have made a real scientist or not, or a mathematician. I probably couldn't have been as successful; those are harder to do than sociology. I would have liked to have done music, but I wasn't good enough to be a musician.

VDT: You're now trying it. Had you an early interest in music?

DUNCAN: Oh, yes. As a schoolboy, I played violin in orchestras and ensembles. I enjoyed that a great deal. But I didn't have enough motivation to practice carefully and I wasn't cut out to be a professional musician. But I tried composition as a boy.

VDT: On your own?

DUNCAN: Yes, without any instruction, just as an amateurish thing. I've been doing that off and on all my life. But now I don't have to do sociology, so I'm doing it much more.

VDT: You are composing for electronic equipment. It looks very complicated.

DUNCAN: It is.

VDT: We're sitting in front of a keyboard that has an enormous number of notations; a tremendous number of buttons.

DUNCAN: The buttons don't have anything to do with the notation. The notations are fractions that have to do with just intonation, which is now possible. Just intonation is an old theory. It dates back to the Greeks and the Renaissance, but it's been revived in recent years as a consequence of Harry Partch, whose picture is over there. He's the guru of just intonation. We have a small organization, less than two hundred members. It's focused in San Francisco and I've been an enthusiastic member and supporter of that organization for the last two or three years. I do all of my composition in just intonation, which sounds different--sounds better than the conventional equal-tempered music of Western Europe, which is what we use in both our symphonic and pop music. Just intonation is used in other musics around the world, Oriental and African and so on. Those people have better sense than Europeans about what sounds good.

VDT: Do you write this down, does it get recorded?

DUNCAN: It's written into the computer. Just like a word processor, this is a music processor. It has a screen.

VDT: There's a monitor, screen, here, looking like a normal monitor or TV screen.

DUNCAN: A TV screen.

VDT: That's fascinating. I was going to end up with this, but let's jump back to your connections with PAA, which is why this series of interviews began. You said you haven't had much connection with PAA in recent years, but just sort of think back on PAA. Can you remember the first meeting you attended?

DUNCAN: It was at Princeton, probably in 1949, I suspect.

VDT: 1949 was in Princeton, yes.

DUNCAN: I went to Penn State in 1948, teaching population, as I told you. One faculty member gave me his collection of Population Index; he wasn't teaching population anymore. I thought I should subscribe to it. I wrote to the Association or to Princeton and asked to be put on the subscription list and the price of subscription. I got a letter back, I think from Paul Glick, inviting me to be a member.

So I went to Princeton. I went fairly faithfully to those meetings for a number of years. They were often at Princeton; then they started moving to different parts of the country.

VDT: There were meetings at Princeton in 1949 and 1950, and in 1955; that was the last one at Princeton. What stands out in your memory about the early meetings? Few people, of course; everybody went to the same session.

This is a list of meetings that Andy Lunde prepared. He has Princeton in 1949, 1950 . . .

DUNCAN: 1952. You skipped that one. I have special personal reasons for remembering that one,

which we won't go into.

VDT: Beverly . . . And the ambiance at Princeton, which so many people have said was so pleasant.

DUNCAN: Well, you know, Frank Notestein, Frank Lorimer, Warren Thompson, Pat Whelpton, and Fred Osborn would stand up and comment on the papers in a very measured, precise way. It was a good intellectual affair. We had only one session at a time; everybody went to the same session and paid close attention. The discussions were animated and scientific. It was just stunning if you wanted to be a scientist. This was one of the things that consolidated my devotion to population--those meetings.

VDT: The 1969 meeting, when you were president, was at Atlantic City, presumably before the days of the casinos. That wasn't far from Princeton and apparently it was a fairly good place for professional meetings. There were 486 people. At the latest meeting [1989], in Baltimore, there were almost 1,200 [1,193]. I have a nice little story. Lincoln Day told me [in interview, below] that he and Alice Day--they were at Princeton at that time--went down from Princeton with a young woman colleague and introduced her to just a few people and turned her loose. Three days later, on the way home, she couldn't get over what a congenial atmosphere there was, everybody talking to everybody else. And Lincoln said she wasn't so pretty, so that wouldn't have been a reason for anyone to approach her, necessarily. Obviously, PAA meetings still had that flavor somewhat in 1969. But it's huge numbers now.

Aside from your speech, do you remember that?

DUNCAN: I remember that's when we had the beginnings of these caucuses and students going around . . .

VDT: I've heard that.

DUNCAN: Some of that occurred and I had it under control when I presided. I didn't take any guff off of those people. We got into some argument, I forget about what, in the business meeting and Eli Marks kept wanting to talk. He was primarily a survey guy and a statistician, but he was coming to population meetings. He kept standing up, popping up and down like a jack-in-the-box and shooting off his mouth at great length. Finally I just said, "You're out of order." Sat him down.

About the students--I remember some guy from Wisconsin who had red hair which was about a bushel-basket size around his head. That was the beginning of a period of unrest, and the sequel was in the next year or two when Keyfitz was in the chair [president at the time of the 1971 meeting, in Washington DC]. That's when they passed all those outrageous motions. Beverly became very agitated because those young women got up and alleged that the Population Association had not been a place where women could enjoy any success. And we've had more women presidents than any organization that I know of--Irene Taeuber, Margaret Hagood, Dorothy Thomas [to that time; four more since the early 1970s]. It was founded by Margaret Sanger.

Those [presidents] were great people. Not just great women, but great demographers, great scientists. And Beverly had great respect for them. They made it on their merits; it wasn't impossible. So Beverly called those women down, in the meeting itself, and talked with them afterwards, trying to make them understand that people had been there before they showed up.

I was very upset at those meetings, because they were passing resolutions about abortion and so on, which had historically never, never been anything the Association did. We'd always taken the position that ours was not to take policy stands but to provide the scientific basis for discussion of

policy issues, so that people could come together and have different positions. We had Catholic members who would come and members of different persuasions. We'd talk about demography and then they would go back and could write their own policy position papers. But people in the organization, like Chris Tietze, I remember him specifically supporting the abortion resolution. And he knew better than that, because he'd been a part of the organization for a long time and had known this historical thing. He got carried away because he was a partisan on that issue. Well, I was a partisan for it; I was on public record in the report of the Commission on Population Growth. There wasn't any doubt where I stood on the matter, but it was not something I felt the Association should be doing. And that was when I decided it was no longer for me.

VDT: That was in the early 1970s?

DUNCAN: Yes. So we quit going to meetings. I didn't go to another meeting, I think, until Judith Blake asked me to come chair one of those luncheon sessions.

VDT: That would be 1981, when she was the president; it was the 50th anniversary meeting.

DUNCAN: I may have been to a meeting in New Orleans [1973] or some place, too--one or two meetings was all I ever attended after that.

VDT: That was what concerned you--that they were taking policy stands?

DUNCAN: Yes. And the other thing that concerned me was this professionalism business that got under way. It was the [Forrest] Linder committee [on organizational management, 1966-67]. I was [first] vice-president at the time [of 1966 meeting] and Cal Schmid was president and he was scared to chair the meeting, so he asked me to chair it. So I chaired and I ruled with kind of a heavy hand. I remember too that I circulated a memorandum; maybe Ansley did too, I'm not sure.

VDT: This is what happened. Paul Glick wrote a vignette on the committee, in that series for PAA Affairs ["PAA Committee on Organizational Management: 1966-67," PAA Affairs, Summer 1982]. The committee was formed in 1966. It really was formed to decide what should be done about the increased membership; the membership had tripled with Don Bogue's promotion of Demography. They had to have a paid business office, at least that was a consideration. But the committee came up with the idea of active recruitment of many more members, establishing two classes of membership, professional standards for membership qualifications, and a small grants program for research.

Now here was your answer, in a letter to Paul Glick, quoted by him in his vignette. I've got it marked [on this copy].

DUNCAN: "A general hazard in the recommendation to formulate professional standards." He [Duncan] also wrote: "Horror of horrors, that the PAA should commence a small grants program (with whose money?). I am not personally aware of projects so small that any of several present granting agencies would not consider them." [Laughter]

VDT: Simultaneously, Ansley Coale had written--he was the president-elect--and he violently objected to the committee's ideas and said he would not carry them out when he was president. So that whole thing was shot down.

DUNCAN: So for a year or two, we managed to hold the fort. But it came through anyhow, later on.

VDT: What do you mean--professional qualifications for membership?

DUNCAN: Well, the whole bureaucratization of the organization--creation of a newsletter, having professional historians write on the history . . . [Laughter]

VDT: I'm not a professional historian, and it's all at my own expense, I'll have you know. I'm a demographer; it's my own interest.

DUNCAN: All these accouterments of an organization--a staff in Washington and all those things. It overtook all the organizations. It happened first with the American Sociological Society, which even had to change its name. It became the American Sociological Association. [Paul Glick said the change was made to escape the acronym ASS. See Glick interview above.]

VDT: What's the difference between a society and an association?

DUNCAN: A society is--is a large society. It's where a group of scholars or scientists meet and discuss their scientific or scholarly problems. An association is a professional organization where people meet to forward their interests as employees, functionaries, and so on.

VDT: But PAA had been association from the beginning--the name.

DUNCAN: Well, that's the word; the name, of course, is inconsequential, but the concept of the PAA was that of a large society. They fought with Margaret Sanger about that and beat her down. They said, "We will come and we will have scientific sessions. Period." Then we started having resolutions about public policy issues and we started having committees for this and officers for that and so on. The whole thing ballooned into what you now have--a mass membership group that forwards the professional interests of some category of people called demographers, rather than confining itself to the scientific interests of a discipline called demography. It's a very distinct thing. I couldn't get that through Cal Schmid's head. I'm sure a lot of people resist the distinction. It's very real. The historical transition was being made in this period. And a large number of organizations have made it at different times, at different rates. It was confounded and mixed up with the unrest of the civil rights movement and all those student riots, things of that kind that came along about the same time; it was part and parcel of that. I suspect those things accelerated it, in various organizations--women's caucus and black caucus, this, that, and the other thing--using the organization as a vehicle for interests other than the scientific analysis of the subject matter.

I'm proud that I stopped it for a little while, but it was something that one couldn't do indefinitely. I didn't have the power base or influence after I was out of office.

VDT: Well, thank you. I'm glad you said that. It answered some questions that I had.

I want to talk a bit more about your presidential address, "Inequality and Opportunity." Obviously, it resulted from your research. I was struck by your pointing out that racial discrimination was the greatest factor in unequal intergenerational mobility between blacks and whites--the gap between black and white earnings--and that discrimination had to be attacked directly, because public policies in education and occupation wouldn't eliminate the gap otherwise. Was that because civil rights was such an issue at that time?

DUNCAN: No, that goes back to the Moynihan poverty seminar--the idea that there was a cycle of

poverty, that poverty was destined to recapitulate itself in every generation.

VDT: Culture of poverty.

DUNCAN: Culture of poverty. And our work had not suggested that was true. It showed quite the contrary; that our older generations of people got out--they moved up. Of course, some moved back down. That meant just the opposite of the cycle or culture of poverty--except that generation after the generation, the blacks were clearly disadvantaged. So I wrote the piece called, "Inheritance of Poverty or Inheritance of Race?" The principle of a cycle that is destined to recapitulate itself was applicable to the disadvantage of blacks but not to the disadvantage of other people who happened to be poor in any particular generation. Most of them get out of poverty. That's the main thing that happened; if they're in poverty they get out. Some dropped back down--there's circulation. But with blacks, it has a very different and additional mechanism, a discrimination mechanism. I was trying to use our research to sort of calibrate how that was working.

So I wrote a paper in Moynihan's volume and the speech to the PAA merely recapitulated some of those results. It was just a summary of my work on that topic--maybe the last thing, or one of two or three things that I wrote, trying to sum up what I thought I had learned from a decade of work on that topic.

VDT: It dealt with social mobility, which figures in that famous definition of "what is demography"--the field of demography--in The Study of Population. That's not a very popular aspect of demography now. Why do you suppose that is?

DUNCAN: I don't know. I haven't had anything to do with demography, or social stratification, for a long time.

VDT: Was it a part of the field in the 1960s? Obviously, not a focus, because, of course, fertility, mortality, and certainly migration--but social mobility . . .

DUNCAN: Well, you see, I learned to think about population as having four parts--fertility, mortality, distribution, and composition--because after all the census collects data on age, sex, occupation, this, that, and the other thing. So we get a snapshot every ten years of the composition of the population. If you look at distribution, that's where people live. The dynamic aspect of distribution is migration--people move around. The dynamic aspect of composition is social mobility. That's just a logical, kind of a taxonomic thing.

Now, social mobility often has a somewhat narrower meaning of going up and down the social scale. But it can be extended, as did Sorokin in his book, Social Mobility. He extended it to cover all changes of any kind of status, whether socioeconomic or civil status. Some changes that we make, like from being a minor to being an adult, are irreversible, of course. Others are changeable; you can move back and forth, be a Democrat and then a Republican. There's nothing logically distinct about enumerating the population according to political party or religion as compared to enumerating them by marital status or something else. They are all definable characteristics of individuals which you can aggregate and get statistical distributions for them. And you can do them at more than one point of time. It's better than not having them. It came about in The Study of Population just as a kind of logical consequence of thinking through what is the subject matter of demography. It had no necessary relationship to my interests in occupational change.

And if people don't see it that way, that's their tough luck. [Laughter]

VDT: Thank you. I guess social mobility is tainted with the idea of the struggle of the blacks, perhaps.

DUNCAN: Yes. Well, all these things get ideological appendages. That's why we can't do much with social science concepts. As soon as the social scientists work out a way of ordering their data then the larger community picks it up and uses it for propagandistic and agitational purposes. That makes it questionable whether you can have a social science. That's why I say I'm not sure I like the fact that I've spent my life being a social scientist. I think it's an incoherent enterprise.

VDT: Well, if everybody were not to work on things that were liable to be attacked, I guess a lot of things wouldn't get done. But okay.

On a lesser level, at the same dinner meeting where you gave your 1969 PAA speech, Frank Notestein gave his wonderful tribute to Fred Osborn, who was celebrating his 80th birthday ["Frederick Osborn: Demography's Statesman on his Eightieth Spring," Population Index, October/December 1969]. I gather you invited Fred Osborn to come, specially, to that meeting?

DUNCAN: Well, those guys put the arm on me. They said they wanted to have a tribute for Osborn.

VDT: Who put the arm on you?

DUNCAN: It wasn't Frank directly. Somebody proposed that and I acceded. I respected him, of course. I was glad to give up my time to him; I wanted to make a short speech anyhow. It might have been Dudley Kirk--one of those people in the Notestein circle spoke to me about that [see Duncan's afterthought below].

VDT: It could have been Dudley Kirk, whom I interviewed two days ago. He's always been very history-minded. He's the one who in recent years has invited those who are over 70 and more than 35 years a member of PAA to have a little extra gathering at PAA meetings, a dinner. Kingsley Davis always boycotts those; he considers that age discrimination. He refuses to have anything to do with his age peer group.

Fred Osborn--who had done so much for the field, moneywise and otherwise--you in your talk referred to his book that had just come out [The Future of Human Heredity, 1968], one of the latest books on eugenics, in a sense, which, of course, wasn't considered . . . Well, of course, now everybody cringes when anybody connected with demography gets labeled as a eugenicist. Yet some of the early people in the field, like Fred Osborn . . .

DUNCAN: And Frank Lorimer. I took a little interest in that about 1950 and wrote one paper on whether the intelligence was declining.

VDT: I read that ["Is the Intelligence of the General Population Declining?" American Sociological Review, August 1952].

DUNCAN: Lorimer and Osborn had concluded that was true. But all the direct measurements of change in IQ scores showed they have gone up, rather than coming down. So I wrote a paper summarizing that, commenting on it, and had a little exchange with Lorimer in the columns of the ASR [American Sociological Review].

Note: From Dudley Duncan to Jean van der Tak, letter written after this interview on the same day, May 3, 1989.

"In the interview you asked how it came about that a tribute to Mr. Osborn was included in the session of the PAA where I gave my presidential address. I indicated that someone connected with him called me and made the suggestion, possibly Dudley Kirk. Now the name has come to me. I feel sure it was Parker Mauldin. I believe he was at the Population Council at that time, but am not sure of that. Nor do I know what his connection with Osborn was, possibly not direct but via one or another demographer who did have some association with Mr. Osborn.

"Osborn was part of the 'old guard' that included Lorimer, Notestein, Kiser, and others who ran the PAA when I became a member. Later a group of us, led by Margaret Hagood, put through a reform of the constitution involving a less restrictive nomination procedure and mail ballot for officers. Margaret used to write letters that began, 'Dear Dan, Don, Dudley, Phil, and Rupert'--that is, Dan Price, Don Bogue, Dudley Duncan, Phil Hauser, and Rupert Vance. Despite this modest venture in insurgency, I had great respect for that old guard, including Mr. Osborn, with whom I was on cordial terms. At one time I served as VP of the Eugenics Society, until I learned that it was not an annual post but a perennial one, when I proceeded to resign."

VDT: In 1969 when you were PAA president, Beverly became editor of Demography for three years, following Donald Bogue. Don Bogue wrote about his tenure as Demography editor in a PAA history vignette on "How Demography was Born" [PAA Affairs, Fall 1983]. I've been responsible for all these vignettes. I pursued Don for a couple of years to get him to write this vignette about his rather controversial tenure, that ended with his big thick issue on family planning with the red inverted Indian family planning triangle on the cover ["Progress and Problems of Fertility Control Around the World," special issue of Demography, Vol. 5, No. 2, 1968]. That created a lot of stir, because--in the vein you were pointing out--here was the beginning of PAA's scientific role evidently yielding to advocacy--advocating family planning. That was the general feeling of the membership at that time.

DUNCAN: Yes.

VDT: At the end of his piece, he'd said: "Dudley and Beverly Duncan, as the second editorial team, were very instrumental in shaping Demography into the stance it has today." You pointed out, rightly--and I should have checked as editor of his piece--that it was Beverly who was the editor.

DUNCAN: It was hers entirely. She consulted with me about whether she should take the job.

VDT: Did Don ask you, ask Beverly, or how did that come about?

DUNCAN: I don't remember. I don't think I was asked, but if I was, I turned it down. [Paul Glick says, in his interview above: "The first editor after (Donald Bogue) was Dudley Duncan's wife Beverly. She was my pick for that. I thought she was a businesslike person and would do a good job. And she did."] She asked if I thought it would be a good thing to do and I said, "Well, if you want to do it." It seemed to me it was a professional contribution that was important to do, a worthwhile thing to undertake, so if she wanted to do it, she shouldn't hesitate to accept. She pondered that in her own way and decided to do it, without asking my permission or anything else.

I had no more to do with the editing of that than any other colleague at Michigan, perhaps a little less. She used all of us very sparingly as article referees and advisers--only if something came along that was directly related to our own interests. Otherwise she went outside to other people--Ansley Coale or Nathan Keyfitz or whomever she could find who was a good referee. When she got

their reports, she would read them carefully and then would make up her own mind. She didn't do what many modern editors do, just abrogate the responsibility of making the publication decision to the advisory editors. It infuriates me when I encounter an editor who won't tell me what he thinks of my paper; he will only quote what his anonymous referees have told him. I think that's a shabby way to run a journal. It doesn't make a good journal; it makes a mediocre journal. Editors should have strong ideas about what is good work and should follow that policy. And if an organization doesn't like those ideas, it should get another editor. That makes a good journal. The other way is to have a journal that takes anything that comes in, but it's some kind of bureaucratic stuff that always averages out everything; it guarantees homogeneity and mediocrity.

So Beverly had a very distinctive style there. She typed up the tables herself or at least designed them so they would be economical to reproduce from the typescript, rather than typesetting.

VDT: That early on!

DUNCAN: She abolished footnotes--to the great pain of some of our wordy colleagues who like to be lazy when they write. They cost money, to set those footnotes at the bottom of the page, so she said, "Well, you can have references, but no footnotes."

VDT: Just putting the author name and publication year in parentheses beside the place they were referred to and having the publications listed alphabetically by author at the end?

DUNCAN: Yes, scientific style, but eliminating the footnotes.

VDT: Norman Ryder claims credit for starting that style, when he was not an editor of Demography but of something else.

DUNCAN: Yes, perhaps. That's probably true. Other journals were adopting it.

VDT: It must have been a lot of work. You mentioned that she taped a little piece on . . .

DUNCAN: "Sending Copy." I made a copy for you to take home. ["Sending Copy," by Beverly Duncan. Uses the sound of a clacking keyboard to depict the increasingly frantic pace of getting copy out, on time.] She had rather high standards, so it was never clear that she was going to have a full issue on time. It was a last-minute photo finish to see whether there would be a complete issue. And then, of course, getting all that ready for the printer. She wanted to maintain her deadline; she didn't want to fall behind of a publication schedule, the way many editors do. So she was very conscientious and she always found that stressful as the deadline approached.

VDT: Was she trying to do it all herself or did she have someone else helping with the typing, copyediting?

DUNCAN: She had a secretary at the Population Center, Mary Scott, who was typing the tables. Beverly would block them out. I don't remember for sure, but Mary probably typed them. But Beverly did a lot.

She did an interesting thing once. Ansley had a Japanese student, who was a very good demographer, I guess, but just totally inarticulate in English. This fellow submitted an article and Beverly surmised that it was at Ansley's direction, so she contacted him to find out if he thought the work was sound and he said, yes, he thought so. Well, she simply rewrote the paper, put it into English, and sent it to Ansley and said, "Have I got the demography right? I tried to fix the English,

did I get the substance wrong?" And he said, "No, this is just right." So the paper was published as a contribution by this Japanese student, but Beverly was in effect the second author.

VDT: I know all about that as editor of the Population Bulletin at the Population Reference Bureau. I rewrote several--a lot.

DUNCAN: Those things are not advertised, but this is what a conscientious editor may do. A less conscientious editor may send it back and say, "Revise and resubmit." But she wanted good stuff and when she thought she had something good, she'd work hard to put it into shape so it could go through.

VDT: And she did all this along with her regular job?

DUNCAN: When she got critical reactions, she didn't just use them to crush the author, but she would endeavor to work with the author if she felt there was something there that could be salvaged or developed. She would work with the author in a gentle, non-directive way, but try to get the author to correspond directly with the critic--not through the intermediary of the editor--and say, "You two guys get in contact. I don't fully understand the issues here. If you talk to each other, you'll reach agreement much sooner than if I try to mediate."

VDT: How interesting! That's rare.

DUNCAN: She did innovative things that were not advertised but I was seeing that made it into a quality journal. That was her objective--to get rid of those fat volumes that Bogue had put out that included lots of slop and low-quality stuff. I think that set the tone for the rest of the history of the journal. I don't know what it's like now; I haven't seen it . . .

VDT: You don't read it anymore?

DUNCAN: No. I quit reading it a long time ago.

VDT: Do you read anything in population? I guess not.

DUNCAN: I read nothing in social science. I quit reading almost everything except what was directly related to my research, oh, maybe ten years ago. I started reading the classics, Greek and Roman literature in translation.

VDT: In your field, because, of course, you used those extensively in Notes on Social Measurement.

DUNCAN: Yes, it was a coincidence. I'd just started doing that at the time I got the invitation to write the Notes. I had thought of someday writing on the history of statistics or social measurement. But I was not ready to start that, hadn't decided when I would start, when Merton called and asked if I would write on social measurement. Quite independently, I had gotten interested in reading poetry and had heard about a guy named Homer, so I thought I better go read Homer and, sure enough, I liked it. I was lucky to hit the Fitzgerald translation of the Odyssey and it was just beautiful; I was enthralled by it. So I had launched onto the pursuit of reading of that kind at the same time that this invitation came along, and I saw that the history of social measurement went back to the very people I was reading. So I began to make notes about my reading and included those in places. I think it gave the book the historical depth that it wouldn't have had otherwise.

VDT: It's called, Notes on Social Measurement: Historical and Critical.

DUNCAN: Yes.

VDT: It's a very interesting book. I can't say I followed it too well.

DUNCAN: There are some parts that are very easy to follow and other parts are specialized.

VDT: You had switched your interests to, well, this broader field already while you were at Arizona [1973-83] and while you were here at Santa Barbara [1983-87; then emeritus]. Were you still teaching sociology when you came to Santa Barbara?

DUNCAN: Yes, I taught here until 1987, several things.

VDT: You were teaching general sociology, or specialized?

DUNCAN: I didn't have much chance of teaching my specialty of statistical methods of survey analysis, but I taught an undergraduate course in research methods; I taught a course in stratification several times. I don't think I taught social change, but I gave a few seminars to a few students. I didn't have a very successful teaching career here.

I had a great teaching program at Arizona, had a sort of key course in the graduate sequence. It was the second course in statistics, but I turned it into a course in the methods of analyzing qualitative categorical data, log-linear models and the like, and then used it kind of as a warmup in how to write research reports. I had the students write a short report every week that got them introduced to the idea of how you get results and then report them and discuss the implications and their bearing on ideas of the discipline. By the end of that course, they had enough confidence to start a master's thesis or something of that sort. I trained a lot of people and had some good students there. So that was very successful.

When I came here [University of California at Santa Barbara], they already had people who were doing the statistics. They didn't want to let me in on that; I taught the course once. And I did undergraduate teaching; I didn't have great success with that. This is a lousy university, by the way, a shabby excuse for a university. The reason for coming here was to have a good climate to retire in. It has a good climate and I'm retired. [Laughter]

VDT: Perhaps that's as good a place as any to stop. We've already talked about your most interesting pursuit in composing electronic--just--this is a just notification?

DUNCAN: Just intonation.

VDT: Do you think your musical interest has grown out of your mathematical bent?

DUNCAN: Well, just intonation calls on your skills in algebra and in fractions and logarithms. It's still a modest amount of mathematics, but there is a mathematical aspect to music. So I get some fun out of that. But that is not where it began; it began as a social activity. I just learned a little music, read about it, listened to it, collected records. Over there . . .

VDT: Two shelves-full!

DUNCAN: That's a small part of my collection. It's a life-long interest.

VDT: You know, there are several demographers who have this musical bent. Just among those I have interviewed, there is Jack Kantner, whom I interviewed in Bedford, Pennsylvania, where he's retired. He has the trumpet in his background--one of the horns; he's the director of the annual music festival in Bedford and his son is in the Grand Rapids orchestra.

DUNCAN: Good!

VDT: Then Sam Preston, one of the younger demographers, writes country and Western music, quite seriously. He's sent out several pieces, hoping to get published.

DUNCAN: More power to him, that's fine.

VDT: And Paul Demeny was a bassoonist in his youth.

DUNCAN: That's another who distinguished himself. He was a colleague at Michigan.

VDT: He was offered a job in the orchestra in his hometown and was seriously considering it, because it wasn't sure that he would get into university in Hungary under the Communist regime because his father had been a judge. But he did get to university and gave up the bassoon.

Then, of course, you know about Joe Stycos and his piano. He's a jazz pianist from way back. And Lee Bouvier, a good friend of mine from his Population Reference Bureau days, made his living as a jazz trumpeter until he was 33 and went back to university. So I think perhaps there's a connection.

You said you've written your autobiography, is it?

DUNCAN: This is a short statement that was written for the National Academy of Sciences. It's kind of a pre-death obituary. [Laughter] The Academy publishes memoirs about its extinct members. When they die, somebody has to write up their achievements and so on, so now they have members write about themselves before. Somebody will rewrite it, I guess, when I die. It was written at the time I was elected, in 1973.

VDT: Just like Sam Preston. There are not too many demographers in the Academy.

DUNCAN: Jane Menken was just elected; I got the notice yesterday.

VDT: Oh, fantastic! She and Sam have usually been neck-in-neck in their careers.

DUNCAN: Ansley Coale is a member and, obviously, he has been instrumental in bringing their work to the attention of the scientific community at the Academy. One of the first members was Kingsley Davis. Demographers got in early because they had the quantitative methods and a relation to the biological sciences. That was the foothold for the social sciences.

VDT: Isn't Nathan Keyfitz also a member?

DUNCAN: I'm not sure. There's the problem about Nathan's citizenship; he was a Canadian citizen for a long time.

VDT: Born there, of course. I expect now he's a naturalized American.

DUNCAN: I think he is a member [correct].

VDT: You're the first one who's told me about this pre-obituary autobiography for the NAS.

Is there anything you can write about music, the work you're doing, or will you just leave that record in the computer?

DUNCAN: I have a forthcoming tiny article, my first article in music theory, called "Septimal Harmony for the Blues." That means you use ratios that have sevens in them--seven/four, seven/five, seven/six. The blues typically flats the seventh, the fifth, and the third. My argument is that it not only flats them, but it makes them in a certain ratio of the seven variety--seven/four, seven/five, or seven/six. I took the "St. Louis Blues" and put it into just intonation with these septimal ratios and it sounds better to me that way than any other way. That article comes out in the journal of the Just Intonation Network, called 1/1. There are a little less than 200 members.

VDT: And there is your guru, whose name again is . . . ?

DUNCAN: Partch. He died about 1974. Subsequently, a group of young San Francisco musicians got interested in pushing this and they formed the organization, so I work with them a bit.

VDT: Is that one of the reasons you came to California, besides the climate?

DUNCAN: No, it happened after I came here. I loved Harry Partch's music, but I didn't understand the theoretical basis of it until just a few years ago when I read his book. And by coincidence, I saw a notice about the organization, so I joined up with them and started going into this subject.

VDT: Well, that is a very good reason for being here, even if the university is not . . .

DUNCAN: It's been fortunate to be in California without coming for that reason, but, yes, you're right.

Addendum

VDT: A little afterthought; there is nearly always an afterthought in these interviews.

There is a picture of Dudley with a research colleague at Arizona, who composed an "Ode to O.D.D." in his honor.

DUNCAN: She's the wife of my research assistant at Arizona. They're both now at Tulane University and she teaches music and he teaches sociology. She had written this composition at South Carolina and it was performed in New Orleans in November 1988, along with some other work of hers.

VDT: And beside this, there is a picture of his daughter, from Dudley's first marriage.

DUNCAN: Eleanor Duncan Armstrong. Her husband is Dan Armstrong and they are both assistant professors of music at Pennsylvania State University. She's a flutist; he's a percussionist. They're called the Armstrong Duo when they play together. And she's also in the Pennsylvania Quintet at Penn State, the usual woodwind quintet, and she's kind of taken the leadership there.

VDT: They play all over the state?

DUNCAN: They play in that region of the country, in Ohio and Pennsylvania and so on. They're making an expedition to Connecticut sometime. They will in the future have a broader venue, I'm sure, because they're just becoming well known; they've been in existence only about five years. They're about to issue their first recording and about to commission a composition for the first time.

VDT: Obviously, there's a strong musical bent in your family. And you kept up your interest through the years, but you were kind of fighting it, I guess. That's interesting.

EVERETT S. LEE

PAA President in 1969-70 (No. 33). Interview with Abbott Ferriss at the home of Everett and Anne Lee in Athens, Georgia, June 29, 1979.

CAREER HIGHLIGHTS: Everett Lee was born in South Carolina and grew up in North Carolina. He did undergraduate work at Armstrong Junior College in Savannah, Georgia (from which he obtained an A.A. degree), Emory University in Atlanta, and simultaneously with his graduate work at the University of Pennsylvania, from which he obtained the Ph.D. in sociology in 1952. His career as a faculty member and researcher has been spent at the University of Pennsylvania (1954-66), the University of Massachusetts at Amherst (1966-70), and the University of Georgia at Athens (from 1970). He has served as consultant to the Census Bureau, a member of the U.S. National Committee on Vital and Health Statistics, and on several committees of the National Academy of Sciences. His publications, focused particularly on population distribution and migration within the U.S., include the seminal article, "A Theory of Migration" (Demography, 1966), and the monographs (with others), Population Redistribution and Economic Growth, United States, 1870-1950 (1957), The Development of the United States Census (1975), Net Migration of the Population, 1960-70, by Age, Sex, and Color, Parts 1-7 (1975-77), and Population Estimates: Methods for Small Area Analysis (1982).

FERRISS: Everett, we'd like for you to tell us some of your early experiences in demography. When did you first become interested in demography?

LEE: I first became interested in demography at the University of Pennsylvania [as a graduate student in sociology, beginning about 1947]. I took one course in demography in sociology. Then when Dorothy Thomas came to the University of Pennsylvania the next year, in 1948, I took a course in demography with her and after that I knew that demography was what I wanted to work in.

FERRISS: What first struck you about it, interested you?

LEE: I had come out of biology and had not really found sociology particularly interesting. When I came across demography, I found that indeed there was a subject [in sociology] in which you could get data which could be used to arrive at some sort of conclusion. I also found that demographers wrote clearly and well, so I decided that within the social sciences, this was the field that was the most interesting.

FERRISS: You were an undergraduate then?

LEE: I was a strange combination. I was an undergraduate and graduate student at the same time at Pennsylvania. I had left Emory University without taking a degree there, but I'd come to Pennsylvania and had entered the graduate school and was completing my undergraduate work at the same time. Up until that time, I'd had practically no work in the social sciences.

FERRISS: Who else was on the faculty at Pennsylvania then?

LEE: Particularly Thorsten Sellin, the criminologist. Most of the work I took was with Thorsten Sellin or Dorothy Thomas.

FERRISS: When did you first become associated with PAA?

LEE: In 1950 I went to my first meeting of the Population Association at Princeton. I gave a paper there on migration differentials ["Some Recent Contributions to the Study of Selective Migration," presented in a session on "Internal Migration," chaired by Warren Thompson]. It was, in effect, the beginning of my dissertation on migration differentials.

FERRISS: What were some other things that happened at that meeting at Princeton?

LEE: The thing I remember most was practicing my paper for the next day, along with Mike Lalli, and the people in the room next to us hammering on the wall to get us to shut up so they could go to sleep.

FERRISS: Did they have many people at that meeting?

LEE: No. The interesting thing about the meetings in those days was that they ran about 50 to 75 people, I would guess, and everybody was able to go to the Princeton Inn and have drinks together; everybody stayed at the Princeton Inn. I also recall that at midnight, we would all move down to the end of the one big room in the Inn so we could continue drinking. The township line ran through the Inn and you could drink in one township after midnight but not in the other. So we all ostentatiously moved to one end of the room.

FERRISS: This was the social hour; there weren't meetings going on at that time?

LEE: In the old days of the Population Association, it was very hard to distinguish completely between social hours and the meeting. They all went on simultaneously--not just simultaneously, but the one melded into the other. At the social hours, the topics were population, just as they were in the formal sessions. I also remember in those days, the chairman of the session would look around and say, "We'll have to wait a moment or two," and hold up the session until he could make sure everybody was there.

FERRISS: Do you remember anything else that happened at those meetings?

LEE: Either at that meeting [1950] or one soon thereafter, I remember Kingsley Davis and Frank Lorimer were arguing with each other over the proper approach to fertility.

FERRISS: This was the result of Lorimer's cultural studies?

LEE: Yes. It was the result of Kingsley having written a paper, which he gave at the Population Association, and then Lorimer commented that Kingsley should have read his book [Culture and Human Fertility, 1954]. Whereupon Kingsley said he had read Lorimer's book and that's why he gave the paper. [See Lincoln Day's more detailed description of this incident, below.]

FERRISS: When did you become an officer of the Association?

LEE: I frankly don't remember. I recall running for director [on the Board] several times before I finally got elected [for the term 1965-68]. Once I was elected a director, then I became, I believe,

second vice-president [1966-67], then first vice-president [1967-68], and then president [president-elect 1968-69; president 1969-70] in succeeding years.

FERRISS: Do you recall some of the issues that were discussed at the early directors' [Board] meetings that you attended?

LEE: There were many issues. One was whether or not to admit people freely or whether we should be very careful whom we admitted. In these earlier days, there was great fear that the birth control people, whom we did not necessarily regard as scientists, might take over the Association. We were very careful to be sure that the members of the Association, or certainly those who were running for a directorship or for higher office, were pure demographers.

FERRISS: Not family planning people.

LEE: There was no objection to family planning people per se; it was simply that many of the family planning people were considered to be people who had an axe to grind and were not necessarily interested in pursuing science. There was a great feeling of maintaining the Population Association for scientists.

FERRISS: Had demography become very well established in the universities by that time? This was about 1948 or 1949?

LEE: No, this was about 1952, 1953. [Lee was first elected to the Board for the term 1965-68. The controversy about qualifications for PAA membership mentioned above presumably refers to the heated discussion on this issue about 1966-67, described in, e.g., Paul Glick's and Ansley Coale's interviews, above, and Paul Glick's PAA history vignette, "PAA Committee on Organizational Management: 1966-67, PAA Affairs, Summer 1982.] No, demography had not become well established as a discipline in most places. It was true that in most sociology departments, demographers dominated the departments if there were demographers. But there were not many departments with demographers.

FERRISS: Was there any concern about increasing the credibility of demography as a profession at that time?

LEE: No, I think the demographers felt that demography was doing very well for itself. It was the rest of the social sciences that had to be geared to demography.

FERRISS: Were there any other issues that you can think of, back in those days?

LEE: Not really important ones: Whether or not we should make awards; whether or not we should raise the dues; how we could get out of the red. That sort of thing; nothing of earth-shaking importance.

FERRISS: Do you remember any other incidents that happened at some of the early meetings of the PAA?

LEE: I remember many fine papers, from time to time. I also remember the general feeling as the Association grew larger, that the good old days were gone when everybody went to every session and

heard every paper. It was something of a shock when we began to have two sessions going at one time. It was also difficult to leave Princeton, because in early May, when we always met, everything was in bloom at Princeton, the place was beautiful, the Inn was very hospitable, and everybody knew everybody.

FERRISS: Did you always meet at Princeton?

LEE: Yes. I'm not sure when we stopped meeting at Princeton. It was a matter of growth; Princeton became too small for the Association. [Last meeting at Princeton was 1955; first was fall 1936. Between those years, there were also annual meetings in Washington, 1939; Chapel Hill NC, 1940 and 1951; Atlantic City, 1942; Philadelphia, 1948; Cincinnati and Oxford OH, 1953; and Charlottesville VA, 1954.]

FERRISS: What are some of the other ways in which PAA has changed over the years, besides the growing numbers?

LEE: PAA has gotten much larger, of course. It has members from many disciplines. The discipline itself has broadened and has established relations with all the social sciences and increasingly with biology. It's a different field. It's not quite so in-bred; it's not quite so narrow in its focus. Increasingly, we do not attempt to define demography as the formal aspects of population study, leaving out other aspects of population study, other fields. I think there has been an improvement both in the quality of the research being done and it's certainly true that the field has broadened enormously over the time in which I've been involved.

FERRISS: What was the best meeting that you recall attending, the one that you enjoyed the most?

LEE: I can't think of a PAA meeting that I haven't enjoyed. I think the meetings always offer something of interest. You always see people that you want to talk with. You always learn something--at least I always do.

FERRISS: For the record, although this was an IUSSP meeting, could you relate the incident that occurred at the London School of Economics, I believe it was? [IUSSP meeting in London at the LSE, September 1969.]

LEE: I think I did a great deal for the Population Association at the London School of Economics. There we had a meeting of the finance committee [of PAA] and when I rushed out of the meeting [IUSSP session] a little late to go to the meeting of the finance committee, I went into the hallway, found the elevator door open as somebody was coming out, and got in. When I got to the first floor, I couldn't get out; the door was locked. I could make the elevator move back and forth, but I couldn't get it to open anywhere. Finally, the elevator started moving on its own after about an hour and it stopped at some floor, I knew not where. The door opened, somebody got in, and I got out.

At that point, I found myself within the stacks of the library at the London School of Economics, with my briefcase, trying to come out from the stacks, and I was stopped by a very suspicious gentleman who told me that nobody could have gotten on that elevator and what was I doing in the stacks. It seems that that elevator was never unlocked except for library personnel who went up and down the floors of the London School of Economics. Anyway, I had managed to do it and gotten myself locked in.

It seems that the finance committee, however, without my bothering them, had been able to

work a way of coming out of the great financial difficulty which the Association had gotten itself into during this particular year [1969].

FERRISS: Let's go back to the University of Pennsylvania in your days as a graduate student. You were doing research at that time, weren't you?

LEE: Oh, yes. At the University of Pennsylvania in those days, the university was regarded not as a place to be taught but as a place to learn. We had a faculty which steered you in the right direction and then left you alone. They let you take whatever courses seemed most appropriate. What I did was to take demography with Dorothy Thomas, a course with Ed Hutchinson, criminology with Sellin, and I went over to anthropology and studied with Pete Hallowell. I also took psychology with Morris Viteles. In fact, I wandered around there and elsewhere to suit myself. Then, finally, they gave me an examination and passed me and I was prepared to go ahead and write a thesis.

FERRISS: Were some of the problems in doing research different in those days than what you have now?

LEE: No, the problems weren't different. The fact is that no matter how much you taught, you were expected to do research. You did it as a student, even as an undergraduate student, and you did it as a graduate student, and you did it as a faculty member, regardless of whatever duties you had. Life was very simple in that regard.

FERRISS: And you had a calculator with which you ground things out?

LEE: I had one of the mechanical calculators where you turn the crank--the Friden. I thought it was one of the marvels of its day. Anything that would multiply any six-figure number by another, no matter how many twists of the crank--quite a marvel. It cost me \$75.

FERRISS: Is that what you worked with for your migration study?

LEE: Yes, until I bought an electric one for \$500, one of the first electric ones. Even that one you had to move the carriage by hitting a button; it didn't move automatically on multiplication. You would learn, for example, that to multiply by 98, you multiply by 100 and subtract 2 and that's 98.

FERRISS: What were some of the other research problems that you had?

LEE: Oh, many. We dealt with the range of migration problems, fertility, population policy, in addition to migration and population distribution. There was no problem in population that we were particularly reluctant to tackle.

FERRISS: Let's talk about your tenure as president of PAA [1969-70]. What were some of the issues that you faced as you went into office? Tell us something about that.

LEE: The first one was the great shock of finding out that the Association was essentially broke--that is, had expenses, as I remember, of about \$50 per member and an income of less than \$25 per member. At that time, the Association looked as if it might indeed be on the rocks. So Abbott Ferriss [secretary-treasurer 1968-71] and Anders Lunde [secretary-treasurer 1965-68] and I formed ourselves into a committee on finance. For the first time, we prepared estimates of the expenses of the Population

Association for five years and tried to establish committees which each year would prepare a five-year estimate, bringing up to date the older estimate.

The outlook was very glum, so we went from foundation to foundation, looking for money for the Association. I don't recall how much money we collected during that particular year [1969-70], but it was enough to finance Demography for a long period of time, to enable the Association to give more of a subsidy to Population Index, and to get ourselves out of the red in general. In fact, my memory is that Abbott had to go out and buy bonds with some of the money we had at that time in order to establish reasonable financing for the Association. My memory is that we were very successful in raising money but, at the moment, I have no idea how much we raised [see Ferriss interview above on Population Council grant of \$75,460]. I do remember visiting almost all the foundations in New York and several of the government agencies and foundations in Washington. My memory is that we did very well. Among other things, we opened a floodgate for membership in the Association and greatly increased its membership. [Membership numbers were: 1,495 in 1968; 1,552 in 1969; 1,862 in 1970; 2,075 in 1971; 2,262 in 1972.]

FERRISS: Even running the risk of some of them being family planning people?

LEE: Well, by that time I had decided that birth control was a very good thing. I even thought I might practice it myself. In that regard, I remember when we had our final meeting of the year in which I was president [Board meeting of fall 1970?], the new officers were all coming in. We had been very successful in dealing with the at that time apparently militant women's group, who were asking for nothing more than they deserved. We went to our final meeting in a self-congratulatory mood, at which time I announced that one of the new directors had four children and that the zero population growth people had protested against his being a director because of that. I then announced that they also would have protested against my having been president, having had four children, but I had had my four children before birth control was invented. At this point, Amos Hawley [president-elect 1970-71] spoke up and said he had four children too; he had had one by each method of birth control. That I recall as the end of my tenure as president of the Population Association.

FERRISS: Where was the meeting when you read your presidential address ["Migration in Relation to Education, Intellect, and Social Structure," Population Index, October/December 1970]?

LEE: That was in Atlanta [banquet on the evening of April 17, 1970, during PAA annual meeting].

FERRISS: Do you remember anything else about that meeting?

LEE: Oh, we had a very nice party afterwards; lots of people came and we had a good time. When you're president of an association, you're just so anxious to get through this last meeting and you're so tied up with board meetings and so forth that you don't get much time to really enjoy the Association. The very nice thing is--or has been for me since then--is that you can go to a meeting of the Association and actually hear the papers you would like to hear.

FERRISS: How about the meeting where Phil Hauser arrived with his fancy suitcase that nobody could break into, but Phil couldn't open it either.

LEE: Had all his clothes, had everything, and he couldn't get it open. General Osborn--Fred Osborn--used to arrive with his pasteboard suitcases. The shabbiest luggage that ever came to the Association was Osborn's.

Remember the joke about Osborn, that he had all his children for Thanksgiving dinner and announced to all of them that the first one that gave him a grandchild would inherit a million dollars and he said grace and looked up and they'd all left the table!

FERRISS: Do you remember some other stories about the early demographers?

LEE: Well, let me think. I remember Alfred Lotka [PAA president 1938-39]. He was at a meeting, I guess he was at the 1950 meeting; I remember he made some remarks. I also remember my first knowledge of the IUD. That was having cocktails with Frank Notestein [president 1947-48], who came into the cocktail party and announced the development of the IUD as one of the great instruments for holding the world's population down to reasonable levels.

FERRISS: Did he have one to show you?

LEE: No. In fact, he wasn't quite sure what it was either. I also remember Notestein at the same meeting telling us he had had a call from Margaret Meade to come by and see her very quickly--a matter of great urgency. He went by to see her and the matter of great urgency was that the population of the world was growing too rapidly and might in time get out of hand. He was indignant, having been summoned to New York, to the Museum of Natural History, I believe, to hear what he thought he had known for a long time. She had just found out.

Among the people who came [to PAA meetings], there were the scholars who came from abroad, the established people, and of course there were always in demography many foreign students. The foreign students were much more likely to go to the meetings than were the American students, in part because the United Nations and other agencies paid their way, and also in part because they were concentrated at Princeton or Pennsylvania and some at Michigan and those places, where it was easy to get to where the meetings were usually held.

One of the nicest meetings of PAA, I think, was one which Anne and I [as local arrangements committee members] arranged at the University of Pennsylvania [May 1957], in which we arranged for the banquet to be in the museum among the mummies. [Several PAA oral history interviewees have recalled that occasion!] Anne and I did not get the dinner, because we had a limited number of seats. You were supposed to have a ticket for the dinner [in advance], but it was true that we were still selling a few seats for the dinner at the meeting itself. But after the total number of seats had been assigned, we expected that the people who had come only for the cocktail party [also in the museum, before the dinner] would leave. But it turned out that some of those were very reluctant to leave and simply sat down. And when Anne and I were about ready to go in and have dinner, we found that Lady Somebody from Britain and her husband had no seats, so we gave our tickets to Lady Somebody and her husband and went out and got ourselves a hamburger while the Population Association had its dinner.

FERRISS: But it was a great meeting?

LEE: Oh yes, I think so. When I gave my presidential address to the Population Association at the banquet in 1970 in Atlanta, the thing that surprised me most, looking out, was simply how many people were there [590 registered for 1970 meeting] and thinking how large the Association had gotten over the years in which I had known it. It's much larger now than it was then.

FERRISS: We no longer have the custom of the dinner.

LEE: No, in many ways I regret that. We are not as close as we were, in part, of course, because there are so many of us as compared with the corporal guard in the beginning.

FERRISS: At that time [1970], the president's address was delivered at the dinner?

LEE: Oh, yes--in fact, always. I remember that Margaret Hagood at her presidential dinner [Princeton, 1955] had decided that she would not do it herself, for reasons I don't know, but instead had someone else [Carl Taylor] do it, who chose to address us on the villages in India ["India: Three Hundred and Sixty Million People Plan for Their Development"]. Before that dinner meeting, I had visited some people at the Princeton Inn and elsewhere and we had had several martinis and we came into the dinner and this gentleman came in with a huge ream of paper--at least 500 sheets, I thought--and started reading from the top, taking up each of India's 300,000 villages one at a time, was my initial impression. At that point, I got very thirsty after those martinis, but there was nothing to drink whatsoever. So I waited until Chris Tietze looked the other way and then I took his glass of water, as we were going through the 1100th village, as I recall.

The thing which impresses me about the Population Association meetings, in addition to the camaraderie which is still there despite the size of the Association, is the generally high quality of the papers that are presented. I go to the meetings of a large number of associations and I go to a large number of meetings in which presumably eminent people have been invited to make pronouncements on some topic or other. Still, I think that, on the whole, the meetings of the Population Association are the best planned and the papers are the best presented of any association that I have ever dealt with. Not all are superb papers, but, in general, the very low-quality ones are not there. And almost always at the Population Association, there will be one or several papers which affect the direction of thought in the field, or which give you some new leads for work which you yourself are doing.

FERRISS: Do you recall any topics of the past that have done this?

LEE: Yes, I recall many such instances. I have always, for example, gone to hear whatever Ansley Coale had to say; in most instances, it is something new and path-breaking. I've always found it very useful to listen to what Kingsley Davis has to say, even though I am not particularly interested in the topic which he may happen to deal with at the moment. Kingsley almost always has something which makes me think about the field or which is a statement which I find relevant to things which I am working with. And there are many other people who do that. Most of the advances in the field of population have had at least their foreshadowings in paper which were given at the Population Association. It always has been, and still remains, a first-class organization.

FERRISS: Do you recall any session on population quality, for example?

LEE: A few. Much more attention was given to what is called population quality in the early days of the Association than at present. There are many reasons for that. The psychologists can't make up their minds about it. The biologists are likely to go off in various directions on the topic.

The fact is, of course, that there is no such thing as "quality," but a person or a people have many qualities. In fact, I tried to address that topic in my own presidential address ["Migration in Relation to Education, Intellect, and Social Structure"], in which I attempted to make the point that a particular person has many abilities that we call intelligence, or intellect is a combination of many abilities, on most of which we are simply average, on a few of which we might be good, on many of which we are extraordinarily bad. The whole reason for specialization is that specialization makes it possible for us to choose some of these abilities which, at least for us, are in better quality, let's say,

than some of the others, and by specializing in those things and leaving to others the things which they can best do, we get a society that can operate much better than if all of us tried to do everything for ourselves. I still think this is a topic which has not been given adequate attention, even in demography or the psychological and sociological sciences.

FERRISS: Take topics in the area of providing advice on public policy. Do you recall any notable sessions the Association has had in this area?

LEE: No. In the beginning, the demographers kept themselves almost completely away from policy questions. There were, of course, people like David Glass and Hope Eldridge who did very good work in these areas, and there were people like Dudley Kirk and Ed Hutchinson who were extremely interested in population policy. It was a long time before the demographer came into his own as a person who advised governments and businesses.

This is something which is changing, in part because demography over time has become varied enough and complicated enough so that people who need detailed demographic information and analysis by necessity turn to demographers. It's true, of course, that over time the realization of population as a major economic factor has increased as economists came to deal with underdeveloped areas and as they realized the limits of equilibrium or Keynesian analysis. Population has come back into its own and is now being seriously considered again by economists and government--and, interestingly, by business also.

FERRISS: Could you recall some incidents associated with some of the early PAA presidents?

LEE: I just looked over the list of past presidents and I have known, or at least I have met, every president up until now [1979], except the first one, Henry Pratt Fairchild [president 1951-35] and his successor Louis Dublin [1935-36] and just one other, Lowell Reed [1942-45]. I read a great deal of Lowell Reed's work and I was on the National Committee for Health Statistics at one time, which still honored Lowell Reed as the founder of that particular group. All of the other presidents of the Association I have had some acquaintance with and, actually, most of them I have known very well. It is by and large a very able and distinguished group. I also looked at the present Board of Directors and I find that I not only know everyone on it, but I happen to be married to one of them.

Incidentally, one of the nice things about the Population Association in the past has been the ability to know so many of the husband-and-wife teams which were in population. I've known a good number of these, beginning with the Taeubers, both of whom were president of the Population Association [Conrad in 1948-49; Irene in 1953-54]. As far as I know, they are the only husband and wife who have both been president of the Association [Kingsley Davis and Judith Blake were husband and wife when he was president, 1962-63, but no longer when she was president in 1981]. And I must say, they both [the Taeubers] richly deserved to be president of the Association.

The Population Association has certainly always been one in which women have played an important part. So far as I know, there was never in this association any form of discrimination against women.

FERRISS: Even in the early days?

LEE: Oh my goodness, how could you discriminate against Dorothy Thomas and Irene Taeuber and Margaret Hagood and Hope Tisdale Eldridge? Those people would have run completely over you had you had the nerve! They were extremely competent people and with people of this ilk, it was quite immaterial from the intellectual point of view what sex they were. Although I

must say that they were all quite feminine and charming people as women. That had nothing to do, one way or the other, with their intellectual qualities. They were just bright--and, I must say, much brighter than the great body of men. They made their own way. Had they been male or female, they still would have done a great deal.

FERRISS: So when the Women's Caucus was first organized [1970], they had a good deal going for them already?

LEE: They certainly did, but within the Population Association, there was not the same need to fight for recognition which I think was found in some other associations. In fact, most of the people that I mentioned earlier were by and large indignant at the idea that women had not had a fair deal, at least within the Population Association.

They did not assume that women had had a fair deal within universities; certainly, women had not. I recall at the University of Pennsylvania, Dorothy Thomas was the first woman professor and I am told that before she came, there was a saying that you could not have women professors in the Wharton School of Business at the University of Pennsylvania because the toilets there were marked "Ladies" and "Faculty" and it would be confusing to have a woman professor. Also at the University of Pennsylvania, a woman could not belong to what at one time passed as the faculty club, the so-called Lenape Club, named after the early Indians who had inhabited the area around the university. Dorothy Thomas was the first to break the stranglehold that men had had on the faculty of the Wharton School at the University of Pennsylvania.

FERRISS: In the early PAA meetings, did they distribute papers about the way they do now?

LEE: Yes, there was very early in the Population Association the idea that you should give something to people to take home with them. We didn't have much in the way of electronic graphics at that time, so for the most part we distributed ditto-graphed tables and that sort of thing. But tables were distributed for almost every paper which used data.

FERRISS: Did they use slides or overhead projectors, things of that sort, in the early days?

LEE: Very seldom. They were only occasionally used and then by and large by somebody who had come down from a foundation with all sorts of assistants.

FERRISS: Do you recall any of the people who really contributed to the Association in the early days?

LEE: Yes, Clyde Kiser in particular did a great deal for the Association. I've had occasion to review much of the work which Clyde Kiser has done and I have come to increasing appreciation for somebody who was a real pioneer in several fields of demography. His work on the early movement of blacks into the North, Sea Island to City [1932], I think, is one of the classics of its kind. And his early work in fertility was path-breaking. We owe a great deal to Clyde Kiser and to the Milbank Memorial Fund, with which he was associated and which supported a considerable amount of important work in population in the early days.

NATHAN KEYFITZ

PAA President in 1970-71 (No. 34). "Self-interview" following a questionnaire supplied by Jean van der Tak, taped at Dr. Keyfitz's home in Jakarta, Indonesia, December 31, 1988.

Nathan Keyfitz, famous for his perceptive and original work in many aspects of demography, especially in the application of mathematical techniques, has spent several years since 1984 partly in Austria with the International Institute for Applied Systems Analysis and partly in Jakarta as adviser to the Indonesian government. He kindly offered to tape an interview on his own for this series.

CAREER HIGHLIGHTS [in Dr. Keyfitz's own words, with some additions]: I was born on June 29, 1913, in Montreal. [Ronald Freedman and Norman Ryder are PAA's other two distinguished Canadian-born presidents.] I received the B.S.A. in arts and science from McGill University in 1934 and the Ph.D. in sociology from the University of Chicago in 1952. From 1936 to 1959, I was with the Dominion Bureau of Statistics in Ottawa, Canada, as research statistician, mathematical adviser, and, finally, as senior statistical adviser. Also during that time, I lectured in statistics at McGill. Thereafter, I taught and did research at the University of Toronto, 1959 to 1963; the University of Chicago, 1963 to 1968; the University of California at Berkeley, from 1968 to 1972; and then at Harvard, from 1972 to 1983, and Ohio State, from 1981 to 1983. From December 1983 to the present time [and still as of spring 1991], I have been director of the population program at the International Institute for Applied Systems Analysis in Laxenburg, Austria [outside Vienna]. Currently [end 1988 and again in winter 1989-90], I am with the Harvard Institute of International Development in Jakarta, Indonesia, working for the Republic of Indonesia in the effort to improve its system of higher education.

At various times, I have also consulted, taught, or done research in Sri Lanka, India, Argentina, Chile, Germany, Hawaii at the East-West Center, China, Indonesia, and in some other countries as well.

[Dr. Keyfitz's many awards include several honorary degrees, election to the National Academy of Sciences, and the Mindel C. Sheps Award in Mathematical Demography of the PAA.]

My publications include half a dozen books and a large number of articles or book chapters, scattered over the years from about 1938 to the present time, and, I hope, not yet ending. [These "half dozen books" include, for example, Introduction to the Mathematics of Population, 1968, second edition 1978; Applied Mathematical Demography, 1977; World Population: An Analysis of Vital Data, with Wilhelm Flieger, 1968; and Population Change and Social Policy, 1982.]

The first questions I'm asked to answer in this self-interview are: **What led to your interest in demography and mathematical demography in particular?** and **Tell something of your career in Canada before coming to the United States.**

After I graduated [from McGill] in 1934, there was really not an awful lot to do. I worked for an insurance company for a little while and then landed in Ottawa in the Dominion Bureau of Statistics. At that time, the best that the Dominion Bureau of Statistics could offer me was a menial job, advising as it was called, editing as it would now be called, the census schedules in the 1936 census of the prairie provinces. This was rather uninspiring work, now entirely taken over by computer. So after a few months, I began to look at the census results. There was also the 1931 [Canadian] census for comparison with the 1931 census of England and Wales and the 1930 census of the United States and I compared the occupational distributions in these three countries. There were many difficulties in doing this-- occupations were differently defined--but some results stood out. I remember, for example,

that there were considerably more dentists per million population in Canada than in Britain and very many more dentists per million population in the United States than in Canada.

My work in this attracted the attention of Dr. Robert H. Coates, who was a very distinguished Canadian and the Dominion Statistician, as the post was called at the time, and my boss, but several layers up the hierarchy. He ultimately got me off the revision of the census schedules and I set to work doing one of a series of monographs commissioned to be done in-house on the 1931 census. I was assigned mostly to the monograph on unemployment--the general preoccupation of the time. My immediate superior was Murdoch McLean, a Scot who was far more interested in Gaelic than he was in statistics or in unemployment, but I was given a free hand and not responsible really for day-to-day work to anyone but myself. This privilege I used to learn some demography to write several chapters of the monograph.

I had been at this for a few years when a man named Dr. John H. Robbins, who was in charge of education statistics in the Dominion Bureau of Statistics and who was rather well known throughout the country for interest in the Canadian Social Science Research Council and who had contacts in the United States as well, told me that there were possibilities for study in the United States. The University of Chicago had indicated that it might have a fellowship open for someone interested in the study of fertility in particular and would I write a page or two, a proposal, to Professor William Ogburn, who was the chairman of the department of sociology at the University of Chicago and a very distinguished demographer. I wrote a page or two and they seemed to have made a hit, because almost by return mail, Ogburn offered me a fellowship at the University of Chicago that was intended to pay my tuition as well as living expenses. And it did so quite lavishly, actually, on the amount of \$2,500.

So I spent a very good ten months--in the first instance--at the University of Chicago. In those days, the residence requirements for a degree were much less stringent than they are now. It was possible for me to get a Ph.D. from the University of Chicago ultimately [in 1952] based on that ten months--three quarters--of residence [in the early 1940s].

Ogburn, Ernest Burgess, Louis Wirth, and Herbert Blumer were the stars of the department of sociology at the time. I got to know all of them well. I got to know various fellow students, some of whom have just disappeared, others of whom have gone on to great careers. I remember Reinhard Bendix as a fellow student, who had come from Germany and took his degree well before I did; he ultimately became the authority on Max Weber. There was a man by the name of Roy, who studied occupations, and Weinlein, who studied pharmacists under Everett Hughes.

Everett Hughes had come there from McGill. He had been at McGill when I was a student there in the early 1930s and had gone on to the University of Chicago and was very much present during the time of my stay there. I learned a great deal from Hughes and was in touch with him through most of the rest of his life. He went from Chicago just before I joined the faculty there [in 1963] and became a teacher at Brandeis and lived in Cambridge [Massachusetts]. Lived very close to us [during Keyfitz's time at Harvard, 1972-83], so we knew him and Helen well. We [Nathan and Beatrice Keyfitz] just had a Christmas card this year [1988] from Helen, who is in a nursing home but otherwise of good cheer and still brisk.

I guess that's enough to cover Jean van der Tak's first question. Except that she goes on to say that Phil Hauser, whom she has just interviewed [November 12, 1988], points out that I already had an international reputation in demography before going to Chicago for my Ph.D. in the early 1950s [receiving degree in 1952]. I think that's exaggerated. I thank Phil for making that observation. She says, quoting again Phil Hauser [not actually quoting him], that I had articles on Canada's population and on Ontario fertility in Population Studies in 1950 and 1951 and I was elected to the IUSSP [International Union for the Scientific Study of Population] in 1950, just two years after IUSSP membership was reconstituted on an individual basis. Jean asks me whether I was the first Canadian

member; it's conceivable that I was.

I should say that Phil had a very crucial part in my development; this is years later and I'll get to it again in due course. In 1962 I was at the University of Toronto, having gotten my degree in sociology, and was teaching general sociology, not demography. At that time, there wasn't enough interest in population at the very conservative University of Toronto for me to offer a course in it, so I taught an introductory sociology course and some other courses. It was quite instructive to do so for the two or three years that I was at it. Phil had offered me a post at the University of Chicago once or twice earlier and I said, "Well, I have obligations to the University of Toronto. They got me out of the Dominion Bureau of Statistics," which at that time seemed a blessing and I got the enormous freedom of an academic post as against a public service post. So I did not go. But then in 1962-63, I took a year away from Toronto on leave as a member of the faculty of the Universite de Montreal. My object in this was to learn French and I certainly learned French. To give a lecture, let us say, at 10 o'clock tomorrow morning in French means that you're scrambling through the dictionary all night and it does wonders for your French. I don't know how much it does for the students. But in any case, I had a very good year at the University of Montreal. They must have appreciated it; they did offer me a continuing post. But during that year, Phil was on the telephone again and I went along with his idea of coming to the University of Chicago. I got a visa without much trouble and showed up in the fall of 1963.

That was when my research in demography really got started. Under Phil's encouragement, I did quite a lot of things. I got into computing, which was new at the time. There weren't many demographers outside the Bureau of the Census who knew anything at all about computing. I practically lived at the computer center of the University of Chicago; programmed very extensively in FORTRAN; had students who were interested not only in demography but also interested in doing mathematical computations. Among others, there was Wilhelm Flieger, who was a priest from Bremen who turned out to be very talented in FORTRAN programming as well as an extremely able demographer. There was Michael Murphy, who went on to become a very senior official in the Canadian government, first in Statistics Canada, as the Dominion Bureau of Statistics had been renamed, and then in the Department of Health and Welfare, where he is to this day. He's a very important figure in Canadian demography--an American, who I think has become a naturalized Canadian. And his wife Mary, quite independently, has become a very senior official in the Department of National Revenue.

Well, that tells a little about my shift from Canada to the United States and how I got started doing real--I hope, real--demographic research.

Why did you decide to obtain a Ph.D. and why did you come to Chicago? I think I've pretty well covered that.

How did you manage to fulfill the requirements, commuting from Ottawa, and what was the topic of your Ph.D. dissertation? That I can answer, even after most of 40 years.

I decided to come to Chicago because Will Ogburn offered me a fellowship there. How I managed to fulfill the requirements commuting from Ottawa is that the requirements were at that time very mild--three quarters in residence--and I got leave from Ottawa to do that.

The topic of my Ph.D. dissertation was fertility--the fertility of the Canadian population as given by the 1941 census. I took a sample--I was fortunate to be on the inside to do this, because there is a matter of confidentiality of the original census documents--I was able to use my insider position to take a sample of something like one thousand women in the province of Quebec and one thousand women in the province of Ontario. In the province of Quebec, they were all French-speaking, French mother tongue, Roman Catholic religion and so on, so I had a pretty homogeneous sample. I was able

to set up a factorial design by which these thousand women were used for a number of different contrasts: the contrast between those with much schooling and little schooling; the contrast between those of higher and lower income; I just used two categories in each of these. And, in particular, those who lived near a city--they were all farm women--and those who lived far away from a city, and those contrasts proved to be statistically significant. The whole thing was done quite properly, with random sampling and all. Each of the observations contributed to something like five different contrasts, as is possible in a two by five factorial study. Between those five contrasts and the restrictions on the sample as a whole, I was able to keep about 15 variables constant when I made the comparisons. As I say, I was especially interested in the contrast between those living near the city and those living far away from the city, because it seemed to me that there was a process of diffusion of the small-family pattern, and that showed very conspicuously, in fact. The diffusion of the pattern was clear in that with everything held constant--everything that could conceivably affect fertility that was measurable in the census--there was a difference of about one and a half children between the average for the far-away-from-the-city and the near-to-the-city. The diffusion of the small-family pattern was clearly indicated.

By the way, the striking diminution of fertility in Quebec took place subsequent to 1941, just after the war. The average number of children per woman in 1941 was something like eight and a half and it was not until many years later that average Quebec fertility dropped down to something like two children per woman. Now fertility in Quebec is the lowest of all the Canadian provinces, if I remember the recent numbers. So this matter caught the population just as fertility was starting to decline, just as the influence of the Catholic church was weakening. I thought there were some bad aspects of that weakening as well as good ones. But one of the features that was very conspicuous was the decline in fertility.

So that was my dissertation and it was sent to the University of Chicago. I remember the occasion well. I had to make a trip on behalf of the Bureau of Statistics to Australia and I contrived things so that I dropped off the dissertation on my way to Australia, gave the committee a month to read it, and arranged to have the hearing [defense] on my way back from Australia; I economized a trip. It did indeed work out that way. I had the oral. It was a bit awkward in one sense, in that the people who were examining me were mostly younger than I was. There was Leo Goodwin, with whom I later joined in research when I was on the faculty of Chicago, and there was Dudley Duncan, of whom I've been a lifelong admirer. These men were both considerably younger than myself, but they managed to look respectable enough at the meeting and we got through that all right. [See Dudley Duncan's and Philip Hauser's descriptions of this event in their interviews above.]

What impressed you most about Chicago, the department of sociology, the Population Research Center, and your colleagues and fellow students there during your student years in the early 1950s and as a faculty member in the 1960s?

My student years were actually in the early 1940s, not the early 1950s, because I took most of ten years, while I was in the Bureau of Statistics, between when I did my residence and when I got the degree and I didn't have any great amount of personal contact with the department during those ten years. So my fellow students during my student years were pretty much a non-overlapping group with the people I subsequently knew. In particular, Phil Hauser, Dudley Duncan, Evelyn Kitagawa, who were later to become my respected colleagues--none of them were there during my student time. Among students who were there, as mentioned, were Reinhard Bendix, later the authority on Max Weber, who had recently come from Germany and was the star scholar in the department in the early 1940s, Roy, who studied occupations, and Weinlein, who was working on pharmacists under Everett Hughes. The faculty people that I listened to most and thought most of when I was a student included Everett Hughes, Herbert Blumer, Louis Wirth, Ernest Burgess, and, most especially, Will Ogburn, who

had been responsible for getting me to Chicago to begin with.

Ogburn I saw most of and most admired. He was a true aristocrat. I remember once at a tea he gave at his house that a lady visitor asked him where he came from; he came from Georgia. What did his family do there? He said they planted. She asked him what did they plant. He was surprised at the question; it caught him, of course. He had this natural aristocracy about him that never left him whatever he was doing, whatever he was interested in. His concern for the use of empirical data in research, I suppose, exceeded considerably his sophistication in mathematical manipulation, but he tried his best. I remember in one of his classes there were only two of us students; the other was Josephine Williams, who subsequently had a career in demography and sociology and was, like myself, trained in mathematics at least up to the undergraduate level; she had an M.A. in mathematics, in fact. We found his struggles to explain multiple correlation somewhat humorous, shall I say. Ultimately he discontinued the class; we would be better off to read it up in a book. He was a man of very great self-confidence. It didn't embarrass him in the slightest when he got stuck expounding some particular calculation.

What led you to leave Canada for the United States, also after a stint at the University of Toronto?

That's a hard thing to say, but obviously the greatest influence in that direction was Phil Hauser, who had asked me more than once, who made an offer of degree of freedom of research in demography. The University of Toronto was not very interested in demography at that time. Phil was offering research facilities, a computer and all. Computers had not been heard of in Toronto [in 1963]. He was offering research facilities that were really unmatched in Canada. Canada was altogether far less specialized than the United States, so if I'd stayed in Canada, I guess I would have continued for many years at least--until the last decade or two--I would have continued to teach sociology, introductory courses. Perhaps I would get a chance to do one or two courses every few years in demography.

By the way, I did know Norman Ryder at a very early stage. [Ryder, like Keyfitz, came to the U.S. for his Ph.D.--Princeton, in his case--and returned to Canada to work, also at the Dominion Bureau of Statistics and the University of Toronto, before being lured back to the U.S. Ronald Freedman, PAA's other Canadian-born president, migrated to the U.S. with his family as a child.] I remember his coming to the Dominion Bureau of Statistics [in the early 1950s]. At that time, there was a question in the Canadian census called "racial origin," where race was defined as whether you were Irish, French, etc. I suppose it was to a considerable extent owing to Norman's very strong and continued argument--Norman is nothing if not persistent--it was owing to him more than anyone else, I think, that that question ultimately got modified. I won't say it was dropped, but it was certainly put into a more neutral form and the word "race" was dropped in favor of "ethnic origin." It still isn't very precise and it still certainly does not satisfy Norman's sense of what is useful demography and useful social science. But, as I say, he had a great influence in making the question at least respectable during the time when race was a very nasty subject.

Now I'm asked about the Berkeley department of demography at a time when it was the first--and only, as far as I know--department of demography per se in a United States university.

The people that I knew there [1968-72] were especially the Davises [Kingsley Davis and Judith Blake] and Ronald Lee was there as a student; he later went on to Harvard. And we had other good students. I remember Roger Avery and quite a number of people of whom you subsequently heard the names.

The department flourished during its brief existence [1967-72]. But we didn't all get along very brilliantly with Judith Davis, who had very strong views as to what was the right thing to do. For

example, I was asked by John Noonan to share a course with him. John Noonan's views on birth control and abortion did not by any means coincide with those of Judith Davis. And from the time when I gave this joint course--by the way, I enjoyed it very much; Noonan is a brilliant man--my relations with Judith were strained. They were strained sufficiently that on getting a proposal from Harvard--from George Homans, who was chairman of the sociology department of Harvard at the time--I accepted with alacrity. I might well have accepted the Harvard invitation even if I'd been getting along swimmingly with Judith, but her antipathy to my activities helped to make the decision very easy.

At the same time as I made that decision, Sam Preston, who was on the faculty, and Etienne van de Walle, who was there as a visitor and whom we'd hoped to attract as a member of the faculty, both left, for Pennsylvania. [Preston went to the University of Washington at that time.] So it looked as though Judith was the only member of the department. The university did not want to have her alone vote in a new faculty, so it proposed that a committee be set up, including non-members of the department, of course, that would appoint a new faculty. As I remember it, Judith didn't want this and she carried on a war with the administration. Her carrying on a war with me didn't really cause her any trouble at all. But the administration of the university, right up to the president, was too much for her and ultimately the department was disbanded and she found herself down at the University of California at Los Angeles.

But, you know, the department was disbanded and yet in a way it continued. There is a [Graduate] Group in Demography now that is doing remarkable work under Ronald Lee. It has in it Eugene Hammel, who is a very distinguished anthropologist whose main interest is population, and it has other very good people. It has managed to collect together some brilliant students--Andrew Foster and others whose names I've forgotten. I've had little contact with it, but I have been enormously impressed with the quality of students and faculty that have been assembled there under Hammel and Lee, and then Kenneth Wachter, who is one of the stars of the department. In a way, there is continuity between what Judith Blake started--well, what Kingsley Davis and Judith started--and the present activities. I've always admired Kingsley Davis enormously and I'm still in contact with him. Indeed, I'm working to present a paper to a conference that he's holding in Palo Alto in February [1989; papers published in K. Davis and M.S. Bernstam, eds., Resources, Environment, Population: Present Knowledge, Future Options, supplement to Population and Development Review, forthcoming July 1991]. Well, so much for the Berkeley department of demography.

Do you think that U.S. demographic training has suffered from a lack of specific departments of demography other than population research centers?

I've always held it that demography by itself is a methodology that is invaluable, of course--it uses masses of data; it has its own techniques--but that the substance of demography is not really self-contained. It has to go along with sociology or anthropology or economics or biology and, by itself, it really doesn't have the weight of substance that a discipline needs. So I'm not averse to demography being done in departments of sociology and other disciplines and I would say that probably demographic training has benefited from close association with sociology and other disciplines.

Jean asks: **What are your brief recollections of Harvard and Ohio State?**

I guess she wants it brief, because I spent some eleven years at Harvard [1972-83] and found it just a great place to be. I had outstanding colleagues, partly in demography--Roger Revelle, Howard Hyatt--but in sociology especially, including Talcott Parsons, George Homans, Daniel Bell, Orlando Patterson, Nathan Glazer, David Riesman, who retired during my time there, and many others. Everett Hughes

was not a part of the department but he was very close to the department and we saw a great deal of him. So Harvard and the Cambridge community were just great. We had eleven good years there.

In due course, about 1981, Bill Petersen recruited me, somewhat indirectly but in effect, to replace him in a chair that I think had been set up for him at Ohio State University--the Lazarus chair for population research and teaching. Lazarus is a major department store in Columbus. I guess Ohio State was the largest single campus in the country, about 55,000 students and an enormous faculty, among whom there were some very, very good people. We had good associations there. My wife and I spent three summers there, as I recall. We bought an apartment and made ourselves a part of the community in Columbus, Ohio. We still own the apartment.

I subsequently was made emeritus of Ohio State University as well as of Harvard, so I was really on the faculty of Harvard simultaneously with Ohio State. In my declining years, that is between 65 and 69, Harvard's rules permitted me only a half-time appointment, only a one-semester appointment. So it fitted well with my work as Lazarus Professor at Ohio State. Mind you, work was exactly the same whether I was at Harvard, Chicago, or whatever. I was doing the same demographic research. I just moved my peanut stand, as it were, to a new location, but I was selling the same product. [Jane Menken, in her interview, below, cites Keyfitz as an example of "someone in our field" who has perhaps been more active than ever as a professor emeritus: "I remember laughing that when Nathan retired from Harvard, he remained on the faculty half-time and at the same time accepted a professorship at Ohio State which was two-thirds time. That was `retirement.'"]

Who were the leading influences on your career and why?

Well, I suppose John Robbins, who back in 1939 or so was in touch with Will Ogburn. John Robbins was a Canadian social scientist, a member of the Canadian Social Science Research Council, who knew his way around the funding situation. He was a colleague of mine in the Dominion Bureau of Statistics, head of education statistics. He told me that there was a fellowship available at the University of Chicago for someone who had worked on fertility. I proposed to Ogburn that I come down there and work on fertility and he accepted the proposal and I started my Ph.D. studies. That was one factor, working through Robbins and Will Ogburn.

I suppose that the biggest element in my getting going on research and being reasonably productive was Phil Hauser, who had this great American spirit of generosity. It came as a surprise to me; Canadians aren't quite that open and quite that generous. He got me funding, showed me how to get further funding myself, got me connected up with the computer at the University of Chicago, got me some very good students, and in general launched me in the research career. I wasn't that young at the time. Since this was 1963, I must have been fully 50 years old when I got started on my real research work. I had done a few papers before, but my real research work started in 1963.

I worked for some five years to produce this book. Jean is very generous in her remarks, but I can't say that I was publishing two books a year. It happened that between 1963 and 1968 I was working on two books and they both appeared in 1968 [Introduction to the Mathematics of Population and World Population: An Analysis of Vital Data, with Wilhelm Flieger].

I was enormously aided by one of the students that Phil Hauser recruited for me, Wilhelm Flieger; we did this World Population together. I did a lot of programming for it, but then Bill really re-programmed all of what I had done and got it much better, much simpler. Ultimately we got the book out, using the very primitive technology of that time, using the mainframe of the University of Chicago.

I should say that there was another book of the same kind, also I believe called World Population, put out by Freeman a few years later [Keyfitz and Flieger, Population: Facts and Methods of Demography, 1971]. Most recently, we've been in contact with the University of Chicago Press.

Bill is very active in the Philippines in the Catholic University of San Carlos in Cebu. He and I have been in touch and have agreed that there's room for another issue of World Population now, using the data subsequent to 1965. What is more important is that the University of Chicago Press also thinks there's room for a new book. We hope that it will be out in the next year or two, using very modern technology, all done on microcomputers, much of it in Bill's place. Although I've never been there, I can imagine that's not a world center of computing or demography, so it will mostly be done on micros in his little population research center at San Carlos University in Cebu.

Among other influences on my career, besides Phil Hauser and John Robbins and Will Ogburn, there was Leo Goodman. Leo Goodman impressed me from an early point; his way of thinking and thinking about a problem until he got down deeper into the essence of it than anybody else had done and coming up with a simple answer to matters that had puzzled the rest of us. That he did, for example, on the kinship question that we published jointly [in, e.g., L.A. Goodman, N. Keyfitz, and T.W. Pullum, "Family Formation and the Frequency of Various Kinship Relationships," Theoretical Population Biology, Vol. 5, 1974, pp. 1-27].

Then there was Dudley Duncan, whom I didn't get to know personally so much but whose work always inspired me--his ability to learn a brand-new field, his ability to get at the empirical aspect of a subject, his ability to cut through nonsense.

I haven't mentioned Don Bogue. I saw a good deal of him at Chicago; he got me involved in the journal Demography. I thought there was a good deal of no-nonsense in Don. He didn't get deep into mathematics. He had an incredible capacity for work; he produced books the way other people produce articles. His books are very solid, very well organized. I must say I learned a great deal from Don.

Then, of course, there were many people [at Chicago] not in demography. I took classes with Richarn McKeon, a classical scholar and philosopher, and I was influenced by his sharp way of organizing his philosophical argument. I learnt something even from Robert Hutchins, who was president of Chicago and very much in view at the time I was a student. He wasn't specially liked by the sociology department, and yet I thought he had a great deal to say. I later made contact with him at the Center for the Study of Democratic Institutions in Santa Barbara. He was then getting on in years but his same self.

There were people at Harvard when I was on the faculty there, in my first years there. I saw a great deal of Talcott Parsons and of George Homans. They didn't get along well with one another. They had very, very different perspectives on the world: George, down to earth and empirical and practical and with his psychological view of society; Talcott, rather high up in the stratosphere often, but nonetheless a scholar in his knowledge of the literature, in his willingness to discuss matters with someone like myself who really hasn't ever gotten very deep into the great works of Weber and Durkheim.

At Ohio State, the man who impressed me most and with whom I had many, many discussions was Saad Nagy, who became chairman of the department during my time there and who did excellent studies of rehabilitation and other matters, rather on the margin of medical sociology.

Also in medical sociology, I ran into David Mechanic at one stage, at Wisconsin, where I was for a semester, and I saw a lot of Paul Starr, who wrote that great book, The Social Transformation of American Medicine.

What took you to Austria and the International Institute for Applied Systems Analysis? Tell something about your work there.

At the end of 1983, I was at the age when neither Harvard nor Ohio State really had any further use for me. Under rules then operating, nobody, but nobody, could be a member of a university faculty after the age of 70, so I was, in effect, put out to pasture. These rules no longer apply, by the way;

circumstances are quite different in the last five years. But at that time, I was effectively put out--as everybody else was--at the age of 70.

The International Institute for Applied Systems Analysis had an opening. Andrei Rogers, whom I'd seen on previous occasions as a visitor to IIASA and who'd been a very effective head of the population program there, doing great work on migration using his multistate model, was leaving IIASA and going to the University of Colorado at Boulder. There was a post there and I was invited to come over and fill it by C.S. Holling, the well-known ecologist who was the director of IIASA for the first couple of years of my tenure in that post. I changed rather considerably the direction of the IIASA population work, moving it toward the study of aging. I was able to get good material from member countries and from the United Nations. We were able to get very good computer facilities. IIASA had excellent software and very skilled computer operators. So I found IIASA a fine place to work. I repeat that I really only do one thing and it turned out that IIASA was a good place to do it.

While I was there, I had contact with many people--some better than others. Ake Andersen, who hails from the University of Umea in Sweden, closer to the Arctic Circle than any other institution I've been associated with or known about, an economist and statistician, was a great inspiration to me for the three or so years that we overlapped.

I'll be going back to IIASA in May [1989]. This is the fifth consecutive winter that I've spent in Indonesia; spending six months in Indonesia and six months in Austria. Choosing the best part of the year in each one has been very satisfactory. But just how long I'll be able to keep up this shuttle is not at all clear. My present contract with IIASA extends to the end of 1989 and whether I'll be able to continue beyond that remains to be seen. [Dr. Keyfitz was still at IIASA as of spring 1991.]

Shortly after I got to IIASA, Mr. Reagan decided that we were giving valuable material to the Russians and cut off the United States contribution. That was a very serious financial blow, of course. In a way it was flattering to us to think that we were providing such valuable material to anybody. I had thought that we were working in systems analysis in a rather abstract fashion, making models of various kinds. The Russians had very good mathematicians; they're probably better than the American or other mathematicians. And it seemed just so unlikely that the kinds of things I was doing, or other Americans were doing, would have been of great military--or any other--value to the Russians, more than they were of value to any other member country. In any case, Mr. Reagan cut us off and gradually we've been cutting back on a project basis; American aid to IIASA has tended to be on a project basis rather than the unconditional annual contributions that we get from the 16 other countries.

Who were your most outstanding students? Tell something about your love of and expertise in teaching.

I was fortunate in having many, many good students. I've mentioned some of them--Mike Murphy, Ronald Lee. McFarland comes to mind; Andrea Tyree comes to mind. I think that Bill Hodge was still a student when I first came to Chicago in 1963. [See list of students, toward end of the interview.]

I learned a great deal from students and I learned a great deal in attempting to present matter so that they would understand it. If my writing has any clarity, it's due to students saying again and again, "We don't understand that argument." Students are a great discipline to an academic. It seems to me of just fundamental importance that teaching and research be combined. If you have research only, you get narrower and narrower; if you have teaching only, you get broader and broader, but more and more superficial. It seems to me that the two are absolutely essential.

I conceive something of the way in which teaching and research stimulate one another in the United States by noting the absence of the same here in Indonesia. I'm going to tell more later about my work in Indonesia. But now I can say that that peculiar combination that it seems to me the United States academy presents in its best and most developed form, that peculiar combination of teaching and

research in graduate school, is designed to produce the most worthwhile results in research and produce a new generation of scholars that will ultimately replace the teacher.

The absence of it in Indonesia makes me clearer than ever on how important it is in the American academy. In Indonesia, research is not all that important. There isn't any sense that it's possible to gain new knowledge. There's some sense that really everything has already been discovered and it's only a matter of reading about it in books and finding out about it and then teaching it.

That sense that there are plenty of things to be learned, that much of what is thought to be known really isn't true--that is what you get in graduate school, and, of course, through the process of submitting your work to refereeing in journals. That is something that has been developed over the long history of academic work in the United States. It's come about gradually; it's had a great emphasis in the postwar period. And you see how difficult it is to obtain it when you experience a less developed country and you find that it just does not exist.

One of the advantages of the American academy over the Indonesian one is that an academic in the United States really has no place to go except to better research and attracting better students. That's the only place he has to go. In Indonesia, the academy is part of the civil service; being a professor is just one stage in rising through the civil service. You may well move from being a professor to being a dean to being a director of a division in the public service, not necessarily with respect to education at all; it might be in trade or somewhere else. You then go up to be director-general; you may ultimately become a minister.

This fact that the academy is just a part of the promotional system in the civil service makes it that nobody takes the work of the academy seriously. It's something that you're going to do for three or four or ten or twelve years, depending on how lucky you are and how quickly you get promoted out of it. And in those circumstances, nobody does very serious research--or very serious teaching, for that matter.

We're trying to get that changed here. Whether there's any prospect, we don't know; it's very strongly ingrained. That attitude toward knowledge is something that stands out so clearly in the American university. In other parts of the world, in less developed countries, it's going to take a long, long time to develop.

What do you see as the leading issues in demography over the years that you have been involved?

I guess that is something that I have thought a little about. The issue in demography at one stage--this goes back to my student days--was whether one should use models or whether one should just talk about the subject. I suppose Dudley Duncan had more influence than any other single person. Again, Phil Hauser--although he himself was not a master in manipulating models--still had a sense that the way Dudley was doing it was right and he pushed me in that direction.

It's very interesting to me that when I travel in Czechoslovakia or the Soviet Union, for instance, and talk to demographers there, I find the demography of, say, the United States in the 1930s, when talking about the subject, writing about the subject, was pretty well the style. There's some sense that one really has to do it a little differently and getting themselves modernized is what the Eastern countries are very anxious to do now.

Another issue was whether demographers should study the demographic situation of the world and their own country and own locality or should try to do something about it. I have tended to be on the side of those who wanted to study it. I always took the view that doing something about it could be carrying you in the wrong direction if you didn't know what the problems really were. It seemed to me that there's room for a division of labor, of course, between those who are going to do and those who are going to study and think. And I tried as much as I could to be on the side of the studying and

thinking.

In the 1930s, there was a lot of writing about how the birth rate was falling catastrophically. I remember that Enid Charles, a British demographer whose name, I imagine, is not very well remembered now, wrote a book called The Twilight of Parenthood, complaining about the fall of the birth rate in the 1930s and how the race was going to decline and disappear. That phase of bemoaning the low birth rate, saying something should be done about it, gave way--I guess as soon as the war was over; well, let's say in the 1950s anyhow--to a sense that there were just too many people; the birth rate was much too high. Some said even in the developed countries it was too high on account of the baby boom, and certainly was too high in the less developed countries. There was quite a controversy at that time between those in the advanced countries who said that the less developed countries would grow much faster economically if they would grow more slowly demographically. The less developed countries at that time didn't believe it and there were strong voices among them that said, "We need more people; people are our only strength; we don't want to hear those in the advanced countries who are trying to deprive us of our one source of strength."

It's curious that in recent years that situation has been reversed. The less developed countries have learned that they don't need all those many people, that they will advance far more quickly and have far fewer problems--economic, political, ecological, and others--if they cut their birth rate drastically. That applies conspicuously to Indonesia. It applies practically to all the other countries that I know, except perhaps Malaysia and Singapore. In any case, that's the side that is now taken by the less developed countries.

The advanced countries, especially in the United States, especially in neoclassical economics, have changed their side and they are now where the LDCs were in the 1960s, saying that a rapidly increasing population doesn't really constitute a great disadvantage to economic growth and, in effect, saying, "Sure, people should have choice as to whether they're going to have children or not have children; the great argument for birth control is essentially an ethical one--people have the right to decide. The economic argument for checking growth is not a strong one." That's a neoclassical argument.

I think it's fairly wrong. I think the less developed countries know their own interest by now and their efforts to control population are based on what they see. That is unemployment and what they see as the difficulty of getting capital, because capital to them is not a matter of something that you make at home. Capital is something you buy from the United States or Japan; capital is equipment that can't possibly be made in the less developed country and there's only so much possibility of buying it. They want to hold down population to what will be able to work with the capital that they can secure.

Experiences in Burma

My first contact with the less developed countries was in Burma around 1951 when I spent three months in Rangoon helping with a kind of trial census that was being done there. The government of Burma then, as now, had only a very tenuous hold on the country. It was firmly in command in Rangoon, but once you got out of Rangoon the various kinds of minority groups, ethnic groups, brigands, and others were fighting the government and there was not the slightest chance of taking a census for the whole country. We did our best in respect to Rangoon; developed procedures for editing the schedules, procedures for punching cards, making tabulations and such.

I worked then with U Kyaw Khine and U Soe Liang. U Soe Liang was a young government worker official with ambitions to become a demographer and I got along extremely well with him and his family, to the point where I actually moved into their house during the last month and a half of my stay. I lived with this couple and their children and learned a lot about the less developed world. They

were formally Buddhist, of course, and Buddhism was their religion if anybody asked. In the front living-room, there was a shrine to the Buddha and the lady of the house, who was probably the more pious member at that time, would do her meditation in front of the Buddha regularly every morning, starting about 4 o'clock. But I discovered in the course of my living with them that there was also a shrine in one of the back rooms of the house to the tree spirits. These tree spirits are regarded as very, very powerful. They are effectively a pre-Buddhist religion; they go back to very ancient times. And they were available as a religion for the practical contingencies of life. If you wanted to overcome an enemy, you wanted to deal with the contingencies of ordinary life, the tree spirits, who had a very elaborate shrine, would give you the right kind of advice, would actually do some of your work for you. This to me was very interesting, this combining of Buddhism and the pre-Buddhist, very primitive--well, primitive is not a fair word, that's the word that came to mind--primitive religion.

U Soe Liang died a few years ago, but we've kept in touch with his wife and son Nathan, named for me.

I found Burma just about the most romantic place I had ever been and I still don't know of any place that shows more dramatically the contrast between East and West, the nature of an alien--I have to say as an alien--culture and civilization. I've had no contact with Burma since, to my regret. Phil Hauser went there. He must have spent quite a bit longer time than I did [a year, 1951-52], and he got to know Soe Liang and the other people. Of course, Phil got around far more than I did. I think he traveled a good deal through the country; the government provided him with suitable protection. All in all, I think he had a great experience.

In Indonesia

I had another opportunity in Indonesia. I took a year in 1953 with the very young Republic of Indonesia. A Roundtable Agreement, in which the Dutch had signed a treaty giving Indonesia its independence, had occurred only four years before, in 1949. So everything was very new. There was a great sense of euphoria: "Now that we've got the Dutch rulers out, we're going to take over and we will make ourselves a proud and independent country." I got this sense of euphoria from a Dr. Sumitro, who was the Minister of Finance at the time and for whom I worked, and from Ir. Djuandra, who was head of the Planning Bureau and subsequently became Prime Minister, perhaps Indonesia's most distinguished Prime Minister ever. Unfortunately, he died; I believe he died in office. Very conscientious, intelligent man. And I did some demography at the time.

I also found that the Planning Bureau, for which I was supposed to work, didn't really know what it was going to plan, and the amount of activity there seemed to me not such as to encourage my making great contributions. So I spent some of what would otherwise have been idle time, I guess, living in a village in East Java with some young people, including a Professor Widjojo, who subsequently became the architect of the Indonesian economy, with Dr. Permadi, who subsequently became a director of the Bank Rakyat Indonesia, a rural banking system, and with others. We went and lived in this village. These were students who had been assigned to me as counterparts and with whom I worked. I was very fortunate in the selection of my counterparts, all of whom proved how good they were by becoming truly important in the subsequent development of the Republic.

Living in that village in East Java in 1953 taught me a great deal and also taught my student collaborators a great deal. They were city-bred; they didn't know all that much about the way the rural population lives. We ended up by all of us knowing quite a lot. I took 150 pages or so of notes at that time. I would interview somebody and then rush off to the room in which I lived and type up these notes. I never made any use of them; they were kicking around.

I rediscovered them in my files around 1983 and it seemed to me that it would be a great opportunity to examine that same village and find out how things had changed. That was the

experience that was described in my article that Jean refers to, in the December 1985 Population and Development Review ["An East Javanese Village in 1953 and 1985: Observations on Development"].

I went back in 1984, I guess it was, during my first of five [recent] trips to Indonesia, and lived there for a while and saw the changes. And the changes were tremendous, extraordinary.

Here there were two schools in the village, where in 1953 there were no schools. Nobody could read and write, virtually, aside from the headman. No one could speak Indonesian among the adults; they all spoke the local language of Javanese that I never got to understand. I could communicate mostly through the headman, who did read and write and did know Indonesian, as did very few of the senior members of the village. By 1984, everyone under 40 was able to read and write. They'd all been through primary school and many had gone on to a secondary school in a neighboring village.

They had paved the main village road. Electricity was coming to the village and there were already several television sets. The primitive, home-made houses of 1953 had given way to professionally designed and built houses by 1984. The physical aspect of the village, the amount of education and knowledge of the world on the part of the people who lived in it, the level of living, were all just very, very different.

In 1953 there was a very rough time for the three or four months before the big harvest. The local language calls it a paceklik; that means just not enough food. People who had started after the harvest eating rice dropped down to corn a very few months later and by the time of the paceklik were eating nothing but cassava, which is not really at all nutritious. These people of 1953 had given way to villagers who were unquestionably eating rice the year round. There was no food problem at all. The pounding of rice to hull and polish it, which was done by the village women from prehistoric times and certainly in 1953, was now done by a hulling machine, and for a trifling sum, the farmer could get his whole crop hulled, saving the village women an enormous amount of arduous work.

Those were the sorts of changes that I reported, including, by the way, the changes in the attitude toward the family, so that the small family was well on its way in this peasant village by 1984.

I certainly agree that micro-level demography, the way John Caldwell practices it, is very much a major approach in our discipline, even though my own study was not extensive enough or deep enough that I could produce results that were specifically demographic. My time there was short--it was a matter of months rather than years--so I didn't really get into attitudes on population and practices in relation to childbearing, causes of high mortality, and such. In retrospect, I would have liked my two visits to have concentrated a little more on the demographic aspects of life in that village, rather than being of a general ethnographic character.

Publications

Jean has a question about my work on policy and indeed I have written many pieces that profess, at least, to deal with the policy consequences of demographic research. I don't have my publications in front of me, so I'm not in a position to say what is important and what is less important in my work, as she asks me to do, in any detail.

I certainly spent the first half of my life on the mathematical and technical demography that she describes. There were two books on that subject. Introduction to the Mathematics of Population came out in 1968 [revised second edition, 1978] and dealt with what the title says, the mathematics of population, rather than demography as such. The book has been quite widely used, but when I saw it in print I was a little dissatisfied with its somewhat abstract character and wrote another book, also mathematical, called Applied Mathematical Demography, published by Wiley in 1977. This had no overlap at all with Introduction to the Mathematics of Population and concerned, as again it says, applied mathematical demography. In that I had some original pieces, but mostly it was an attempt to

round up what was known on the subject regarding everything from the effect of contraception, the making of life tables, on a great number of questions that could be dealt with mathematically and that involved techniques needed by demographers.

As I've mentioned, it's considerably exaggerated and I'm flattered that Jean thinks this, but it's not true that I wrote two books every year. There's only one year, 1968, in which I published two books [Introduction to the Mathematics of Population and World Population: An Analysis of Vital Data, with Wilhelm Flieger] and I'd written them in the preceding five years. Subsequently, I have written maybe half a dozen books and they were spread over a considerable length of time.

Population and development

The policy questions have indeed been interesting me more and more and right now, as mentioned, I'm preparing a paper for a conference that Kingsley Davis--who is one of my heroes--is holding in Palo Alto [at Stanford's Hoover Institution] in the next couple of months [proceedings cited above]. I don't know whether I'll be able to get there or not, but I'm very anxious to contribute a paper, in which he has asked me to deal with the question of population and development. As I see it, that question breaks down into four pieces.

The first has to do with the Malthusian notion of why it is that increasing population is a handicap [constraints on land and resources]. According to John Stuart Mill and the whole English economic tradition of the 19th century--up to World War II really--when you have more people you have to use marginal land, marginal sources of energy, and you just push further out on a curve, a line of diminishing returns. There are some increasing returns to scale in manufacturing industry, but sooner or later you bump up against the limits of the environment. That's the resources argument.

That has been greatly altered by the process of invention; the process of almost automatic invention of substitutes in the 20th century. With copper still being used in conducting electricity but with high tension much less of it is needed; copper for transmission of signals being made really quite unnecessary through the advent of glass fiber and many other ways of communicating--satellites and such. In respect of tin, for instance, we've learned how to put the tin on the tin can more thinly and save about half of the tin. In respect to rubber, we've learned how to produce rubber in factories, which is every bit as good for most purposes as natural rubber. We've learned how to make fibers that are more satisfactory than jute and sisal. We've learned how to use cellulose sponges, artificial rubber or acrylic in place of kapok, so that many of these tropical products, especially, have been substituted.

It seems to me no coincidence that the process of substitution came simultaneously with the ejection of the white colonials from the tropics. Once the Europeans lost their colonies, they really had the strongest incentive to work hard in the laboratory and that is what they did. They replaced the colonies in effect--Holland is an outstanding example--by clean, highly productive, capital-intensive factories in the former metropolitan country.

Germany had really set an example of just that in the 19th century. Not having colonies, having been disunited and hence late in the race to grab off Africa, Asia, and any other part of the world that was to be had for the conquest, being late in the process vis-a-vis France and Britain especially, she was far behind. She turned to her laboratories and made among other things--I think the first article that she made was a substitute for indigo, for dye-stuffs, and the German dye industry very quickly replaced thousands of hectares of indigo plantations in India and elsewhere.

The process was picked up by the former colonial powers after World War II. It meant that the tropical populations, which had really built up on the need for tropical products--one example is sugarcane, which was very important in the development of Indonesia in the 19th century and which was responsible for the growth of population--that is to say, the law and order established by the Dutch and the minimum of medical care and perhaps the somewhat better nutrition that the sugar incomes

made possible all went to increase population quite rapidly. Then the populations of India, what is now Pakistan, Indonesia, and Asia generally, built up really on the strength of these colonial products, were left high and dry. That is part of their population problem here.

In any case, because of the process of invention, the world is not going to suffer starvation for lack of food. Africa, in this as in many other things, is an exception. But the Green Revolution as I see it in Indonesia and as I read about it in India ensures that lack of food--the original Malthusian threat--no longer sets a limit on population.

The economists got rid of resource shortage shortly after World War II. The [Ansley] Coale and [Edgar] Hoover book [Population Growth and Economic Development in Low-Income Countries], published in 1958, dismissed the possibility that India could ever have serious food difficulties as its population increased. That was fairly radical for its time, but it has since proved to be correct.

But Coale and Hoover did find another reason why population growth was disadvantageous in the shortage of capital. If the population is growing rapidly, then each new generation has to be schooled, has to be clothed, has to be fed, and has to be equipped for industry, and this could go on at only the level that the previous generation had and only the level of equipment that the previous generation had and still use a fair part of the limited national savings that were available. So Coale and Hoover concluded that because of shortage of capital, not the absolute population growth but its rate of growth was disadvantageous to development.

Subsequently, economists--neoclassical economists in particular--have also got rid of that. They show that capital has not been all that important in the more developed countries, that it only explains a small part of growth. Much more is explained by improved education added to human capital, and the suggestion is that human capital effectively supplants physical capital. Of course, if people are all that are needed in production and neither land nor capital, [the other two features] of the classical trinity, are required, then the population can grow just as large as it wants and get ever richer. I may be unfair in describing it so, but that seems to me essentially the model that is now dominant in neoclassical economics.

I have to say that in so far as I'm an economist at all, I'm an economist of the older school, the one that at least believes that capital is important and that capital is not available in indefinite quantities to less developed countries. Moreover, that capital is something that has to be imported from abroad to a considerable measure. The old idea that capital is made in the form of a loom by a village carpenter that is then used to give employment to the village women in weaving cloth--that kind of capital is not really all that important. It can't compete with automated capital, which is beyond the capacity of the less developed country to produce itself right at the outset. So you have the whole phenomenon of indebtedness.

The countries do their utmost to buy capital in order to employ their young people. And this employment problem is what continues to preoccupy them and it preoccupies them especially with the stringency of the availability of capital, the fact that those loans have in effect gone into reverse, so that the flow of funds is now from the less developed countries to the more developed in the form of service on the preceding loans being greater than the amount of new loans and the merchandise trade surplus. So the preoccupation with giving the younger generation, now relatively well educated, employment is the third reason for restricting population.

Classical economics says that all this would be overcome if the country had a free labor market. There's no reason why a free, unrestricted exchange won't clear labor markets just as completely as it clears capital markets or commodity markets. And yet nowhere do people allow a free market to work in the case of labor. Free labor markets don't exist anywhere. United States probably comes closest. Europe has innumerable restrictions in the form of minimum wages, restrictions on hours, restrictions on who can work, extensive payments to those who are out of work, great restrictions on dismissals of employees, so that an employer, for instance in Italy, will think very carefully before he takes anyone

on, knowing that he's either got him for life or has to pay very heavy severance penalties to get rid of him. All these restrictions on the labor market, which are very familiar and found everywhere in the less developed countries--perhaps even more than in the developed countries--make it that the young people can't get into the circulation of jobs and goods and such. The more fixed the people that are already there are and the higher the wages are set above the equilibrium level, the more impossible it is for the younger generation to get jobs, and especially to get jobs that accord with their education and expectations. So the employment problem is thought of everywhere in the less developed world as a reason for restricting population.

The fourth reason for population planning--I've mentioned land and other resources as a first constraint on population growth; capital as the second; and employment as the third--is ecological. The environment won't stand it, especially in an era of hectic development, with the middle classes of the world--I've written extensively on that; I had an article in Scientific American some ten years back--increasing perhaps at the rate of four and a half percent per year, as measured by automobiles in use, television sets in use, and so forth. The resultant increase of carbon dioxide in the atmosphere, accentuated by the cutting down of forests, especially in the Amazon but everywhere in the world in fact--that is itself partly caused by the increase in population--the process is making it that we're undergoing an irreversible increase of carbon dioxide in the atmosphere. That is regarded as virtually sure. This isn't my view alone; it's the view of the people who know something about it. A virtually inevitable warming of the biosphere. And that's going to have dreadful results. The results will be much more awkward for the less developed countries than for the more developed countries. The people who live in the deltas of the Ganges, for instance, the Irrawady and other rivers--Mekong in Vietnam, for instance--are going to find that the sea level is rising. Something of that kind has been seen in the floods that we've had right here in Java in this particular year [1988]. Not only deforestation has been responsible for some of the flooding but also the permeation of ocean water in the low-lying coastal areas.

In any case, you have a real damage to the atmosphere, to the ozone layer, brought about essentially by the mode of living of the developed countries, and whose bad effects are going to be most keenly felt by the less developed countries. And it's going to be very, very difficult to get international cooperation to deal with this question.

Beyond that there are other difficulties facing the less developed countries. This indebtedness problem that involves a paradox--that American, German, and Japanese banks want the money they loan returned with interest, they want up-to-date servicing on those loans at the same time that their governments, under pressure from their manufacturing industries are doing their utmost to keep out manufactured goods from the less developed countries. And without knowing any economics, it's very clear that those loans can only be repaid by goods. They can only be repaid by the more developed countries standing for a negative balance in their commercial trade. They have to receive that money back in the form of goods; they can't possibly get it in the form of cash. Indonesia now has a debt of some \$40 billion; it's far from the worst. It has a treasury with four or five billion dollars in the form of assets. There's no way that it's ever going to be able to repay the \$40 billion unless the developed countries will accept its textiles, its other labor-intensive manufactures. And the more developed countries are putting barriers against the acceptance of these goods.

The policy consequences of all this very much involve population. They don't involve population alone, but they much involve population. And that is the subject of my present researches.

On PAA

I'm now asked whether I remember the first PAA meeting I attended and Jean was good enough to provide a list of all the meetings that ever were. Despite that, my memory is not very sharp. I think it

would have been certainly in the early 1930s. I'd come down from Canada for the meeting. I believe Leo Schnore was there and certainly Paul Glick and Henry Shryock. The thing that struck me most, I remember, was there was just a tremendous enthusiasm, a tremendous loyalty to the PAA. These people were determined to put together an association that would last. And it has not only lasted, but has expanded from the 38 people who were present at that [organization] meeting in 1931 to--as I see it on this list--well over a thousand at all the recent meetings, including meetings out on the West coast and in New Orleans that are not all that easy to get to. The success of the Association, I think, has been due to a continuance of that enthusiasm and devotion on the part of a certain number of people.

Now I was president in 1971. I indeed presided over a meeting at the time when the Women's Caucus proposed three resolutions. [Presented at the Board meeting and at the business meeting during the April 1971 PAA meetings in Washington, D.C., these resolutions proposed: (1) elimination of discrimination on the basis of sex in graduate admissions and professional opportunities in population studies, and special recruitment programs to increase the proportion of women in the field; (2) removal of legal and financial obstacles to access to contraception, sterilization, and abortion; and (3) development of non-familial roles for women. For details, see Harriet Presser, PAA history vignette on the Women's Caucus, PAA Affairs, Winter 1981.]

The truth is, I was a little baffled at the people who spoke against them, because the existence of discrimination in demography--I don't think we're in any way different from sociology or economics or engineering or anything else--the history of discrimination was rather clear and these were attempts somehow to get around that discrimination. That had somewhat of an aura of affirmative action that seemed not to appeal to the membership. In any case, all three were turned down.

All I can remember now, is my feeling of puzzlement that they were regarded as apparently dangerous by the majority of the membership. [Presser points out that the Board rejected all three resolutions, but the membership at the following day's business meeting, while rejecting the second and third resolutions, passed "a modification of the first resolution which added references to race as well as sex and additionally called for a committee to be appointed to study the extent of sex and race discrimination in the population field."] My guess is that there was a subsequent considerable liberalization on the part of the membership.

My own view, by the way, of the women's liberation movement, that is no longer the last word, no longer in fashion, is that it left a permanent residue in American society. It isn't the residue that the proponents of ERA and such would have liked. And yet it is a permanent and very clear residue, as noted in the recruitment of women by the medical, engineering, and other professions and the increase of women in the academy. It is not enough to satisfy the more enthusiastic liberated women, but still a definite, clear advance that is not likely to be abandoned.

What changes do you see in PAA meetings?

The much increased number of sessions. And I don't think there's any doubt that there have been substantial improvements. We've become an important national society now, where in the 1930s we were a small clique, a small sect if you like, of people who had a special scientific interest and where there was no thought of masses of recruits.

What do you see as the outlook for demography and demographers?

I am certain that there is room for new theoretical and technical contributions. You can read Demography and Population Studies and other journals. And the number of journals is increasing. I think that prior to around 1963--I may be wrong on this--but I think the only journals devoted to population were Population Studies and Population of INED, in French and Population Index. Now

it's at least half a dozen that I know of, and some other national journals in various languages--in German, Hungarian, Spanish, Indonesian for that matter--are flourishing. There's an Australian journal, a Canadian journal. So the expansion of the field and the large number of journals and the fact that they continue to publish worthwhile material seem to be the answer to Jean's question of whether the oldtimers of my generation said it all. I think it's quite clear that there's plenty to be said still.

What do you see as the outlook for world population? Are you discouraged by the slower than projected decline in the population growth rate, which has been stuck at 1.7-1.8 percent for several years?

I am, somewhat. But I don't look at the growth rate; I look at the absolute number of people. The growth rate, the birth rate, has really nothing directly to do with what I now see as the constraint on population. It is the absolute number of people that is important. Those who point to the growth rate and say it has been declining and so the population problem is solved have to look at the fact that the absolute number of births has been increasing, will be increasing rapidly by the end of the century and till about 2030, if I remember right, and projecting far into the future. It will not be until the middle of the 21st century that the annual number of births will come down to the number that we have today. In other words, the absolute increment to the world population is going to be as great or greater than that of today until the middle of the 21st century. And when you think of the effect on the environment of this rapidly increasing number of people and especially think of the increasing fraction of them that will be driving automobiles and otherwise in high consumption and contributing to the carbon dioxide in the atmosphere, then it seems to me you really have to worry about the problems ahead. And it seems to me also that the sooner people take into account the effect of all this on future generations, the better it's going to be for the long run of the world.

It isn't fashionable at the moment to think of future generations. You see that in the low savings rate in the United States. There's no point in saving, people say, we've got insurance against sickness, we've got old-age pensions--no point in saving. Our children are independent and hopefully professionals of one kind or another and well established. Saving is out. The statistics show that. And when saving is out that means that nobody is concerned about the long-term future of their family or of the country. And even less are they concerned with the long-term future of the world as a whole.

So we have a real problem in this population growth that I think is more acute, perhaps, than it's been at any time in history. I'm not even referring to Africa, where I think chronic starvation is ahead. But just thinking of the world as a whole, the expansion of the middle class--which in itself is a very good thing--the kind of consumption, the prospects for unemployment of the younger generation in less developed countries as well, that is going to have tremendous implications for social stability. It seems to me that the demographic component of the world's future troubles is by no means trifling.

What are you doing now in Indonesia?

I got into Indonesia in the first place, five years ago, in the interest of carrying on my demographic studies, and I did that for a couple of years. But then my Indonesian superiors felt that I would be more useful in a study of the higher education system in Indonesia. And it does indeed involve problems.

Indonesia has gotten to have something like a million students in higher education. Well over 200,000 come in each year, either to the public or the private institutions. The institutions got started at a time that really was not very favorable; it was the late 1950s. Whatever may be said about the Dutch colonial regime, the Dutch faculty members who were sent out here were absolutely first-class scholars and they maintained an extremely small but extremely high-quality university system that

consisted of four or five small but excellent institutions. That obviously was not good enough for the new republic, which needed masses of highly trained people. And in a populist move, Sukarno in the late 1950s, when he had no money and there were no faculty from abroad left in the country, established most of the 44 public universities that now exist.

Some of these are still fairly good and in the best ones there are some very good faculty. The best teachers in the best schools are doing a wonderful job. But the mass of graduates who are being turned out really don't know enough to be very useful to the private sector, to the developing industry. Indonesia, with the disappointment of oil prices, is turning to manufactured exports, and relatively successfully, despite all the difficulties. And it needs large numbers of trained people. The people that come out of the universities are not highly trained. The engineers are not skilled engineers; the managers are not skilled managers, with some exceptions. And so they don't get jobs. This means great unemployment at the same time that there is a great need for skilled people.

And that is the problem of the universities: how to up-grade themselves; how to make themselves high-quality, without diminishing the number of students. And without greatly increasing the budget; there is no large amount of money available for education. And without stirring up any political difficulties. Education has become a rather political issue here because it is the means for social mobility and anything that you do in regard to the universities is being watched by many people. These things are politically dynamic--much more dynamic than the government likes. The whole situation is very touchy.

I'm trying to make proposals for the improvement of quality in the universities. And that's quite incidental to my demographic interests.

What are your plans for the future?

I'm not sure how long this shuttle between Vienna and Jakarta is going to be able to continue. My health is fairly good, but my endurance is not as unlimited as I once thought it to be. So I just can't say how long this shuttle is going to continue. Then I will settle down and stay at home--wear slippers all day. I just don't know when that is going to start, but it could be moderately soon.

Additional comments:

On students

One respect in which the preceding [self-interview so far] is particularly weak is the recollection of my students. I don't have any list here by which I can systematically recall them. But quite a number do come to mind among many others.

Lee-Jay Cho at the University of Chicago. Lee-Jay is Korean. He really did very well in our work there and subsequently got to be head of a very important center, the East-West Population Institute. Jay Palmore, Bob Gardner, and Bob Retherford, students of mine at Berkeley, are also in the Population Institute in Hawaii. Also at Berkeley were Frank Oechsli and Bob Lundy.

Reynolds Farley, who of course is well known to PAA members and subsequently became president of PAA, was a student of mine in the first year when I joined the University of Chicago in 1963. Judah Matras was a student at the University of Chicago.

Among the people who studied with me at Harvard, Noreen Goldman has subsequently done some brilliant work. And Nick Eberstad is a very distinguished writer, somewhere on the boundary between demography and journalism, but perhaps better known to a wider audience than most of the rest of us. Also at Harvard, I had an Indonesian student, Mayling Oey-Gardiner, who subsequently has been teaching at the University of Indonesia in Jakarta and been doing important research here.

There was a Brazilian couple, Juan Carlos and Maria Elena Lerda, who both were students of mine, first at Berkeley and then at Harvard. They subsequently divorced; subsequently had good careers, both of them, down in Brazil.

Some of my students [list added April 25, 1989]:

Chicago: Wilhelm Flieger
 Michael Murphy
 Andrea Tyree
 Tom and Starling Pullum
 Dhruva Nagnur

Berkeley: Griffith Feeney
 Jay Palmore
 Roger Avery
 Robert Lundy
 Robert Retherford
 Frank Oechsli
 Robert Schoen
 Robert Gardner

Harvard: Noreen Goldman
 Mayling Oey-Gardiner
 Juan Carlos and Maria Elena Lerda
 Jose Gomez de Leon
 Robert Semiring

Ohio State: Susan Mott

On colleagues

Among colleagues who have influenced me greatly must be mentioned Sam Preston, whom I had the good fortune to have as a colleague at Berkeley. I guess it was just after he got his degree at Princeton, he was very young, he came to Berkeley and we saw a great deal of Preston and Winnie.

Paul Demeny--we were both at the University of California in Berkely for a period of time. He was one of the people that we'd hoped to attract permanently to that program in demography.

Evelyn Kitagawa was a colleague and we were very close to one another from the first days of my coming [in 1963] to the University of Chicago.

Jane Menken I've known for a long, long time and she is indeed a relative by marriage of mine. [Her brother is married to the daughter of the Keyfitz's.]

On children

I didn't say anything about my family. My daughter Barbara got a Ph.D. in mathematics at NYU and subsequently has climbed the academic ladder. She taught at Princeton, Columbia, and now at Houston [University of Texas] and she has finally got to the full professorship [of mathematics]. And is becoming fairly well known in a field that I don't read in and I doubt that many members of the Population Association are familiar with--partial differential equations as applied in aerodynamics and

hydrodynamics.

My son Robert went on to get a doctorate in economics at the London School of Economics and is now a valued member, in fact he is the manager for forecasting of a big consulting firm in Toronto.

On Beatrice

The most serious omission in what precedes is my wife Beatrice, who really is responsible for a very large part of what I have done, if it's of any value at all. When we first met, back in 1936, I was an extremely junior employee of the Dominion Bureau of Statistics and I spent a good deal of my time outside of the office in such desultory activities as amateur photography, taking the car apart, playing chess now and again, listening to gramophone records I collected--all these sorts of things that really are more appropriate to a man who is retired than one who is trying to get going in some kind of career. Well, Beatrice really changed all that. She decided to make something of me.

Her first move was to see that I wore a clean white shirt every day. She herself personally washed and ironed me a shirt each morning. And that white shirt actually did rather well by me; it ultimately got me to a middle-level position in the Bureau of Statistics. But, of course, that would only go so far. She needed to do something a little more basic than that. And so she got me into this business of writing. She typed my work; went through innumerable drafts. She rather pushed me to take an academic position when the opportunity arose. I turned down a couple of opportunities first and she wasn't going to see me turning all of them down and getting older and older. So Beatrice really helped at every stage.

Not only that, she doesn't have any formal schooling to speak of, but is extremely knowledgeable. She has an incredible memory. Any book that she has ever read--and that is quite a large number--she simply knows. I can ask her the plot of any English novel, of many French novels, even Russian novels, although she doesn't read Russian in the original, and she will have it at her fingertips. Her knowledge of biology, of nutrition, of medicine far exceeds mine. I just constantly refer to her as to an encyclopedia.

She is able to entertain me, in the swimming pool--we do half an hour regularly swimming up and down the pool each morning--or after dinner she will entertain me with a literary criticism, with an account of what James Joyce had in mind in some of his what is to me more obscure writing.

So her part in making me whatever I am is really fundamental.

You know, various people have spoken about how an individual is formed. Freud thought the individual was formed in his first few years of life. Marx thought the individual was formed essentially by his work. I think the individual is formed to a considerable extent by his or her spouse. I don't know what effect I've had on Beatrice, but the effect that she has had on me is enormous and entirely positive.

AMOS H. HAWLEY

PAA President in 1971-72 (No. 35). Interview with Jean van der Tak at Dr. Hawley's home in Chapel Hill, North Carolina, April 6, 1988.

CAREER HIGHLIGHTS: Amos Hawley was born in St. Louis, Missouri. He received all his degrees in sociology: the B.A. from the University of Cincinnati in 1936 and the M.A. and Ph.D. from the University of Michigan in 1938 and 1941, respectively. He was on the faculty of the Department of Sociology at the University of Michigan from 1941 to 1966 and was Chairman of the department from 1952 to 1961. From 1966 to 1976, he was Professor of Sociology at the University of North Carolina at Chapel Hill and since then has been Professor Emeritus at North Carolina. Among other posts, he has been demographic adviser to the Government of Thailand (1964-65) and to the Economic Planning Unit and Statistics Department of the Government of Malaysia (1973-74). He has been president of the American Sociological Association [1978] as well as of PAA. He has been a leader in the field of human ecology and also in urban studies. His numerous publications include such monographs as The Population of Michigan, 1840 to 1960 (1949), Human Ecology (1950), Demography and Public Administration (1954), The Changing Shape of Metropolitan America: Deconcentration Since 1920 (1956), R.D. McKenzie and Human Ecology (1967), Urban Society: An Ecological Approach (1971), The Population of Malaysia (1976), and Human Ecology: A Theoretical Essay (1986).

VDT: How did you first become interested in demography?

HAWLEY: As a graduate student at Michigan, I had a course in population--not a formal demography course. It was not a very sophisticated course but it was enough to excite me, so I went on from there. It fitted well with my other interests in human ecology. It was given by R.D. McKenzie, the professor I went to Michigan to study with.

VDT: You went to Michigan because McKenzie was there; he was well known in human ecology?

HAWLEY: Right. He was one of the so-called "Chicago School." He'd come to Michigan as chairman of the department of sociology--the first chairman, as a matter of fact; sociology had just separated from the department of economics. I read a great many of his things as an undergraduate and decided I wanted to pursue study with him. I was introduced to demography, and it remained a long-lasting interest.

VDT: What were the topics of your master's and Ph.D. theses?

HAWLEY: My master's thesis was on migration, the resistances to migration.

VDT: In the U.S.--internal migration?

HAWLEY: On migration generally, internal and international.

My doctoral dissertation was on the significance of size in the complexity of organizations. That was a quantitative study, for the most part, based on the Census Bureau's Census of Business, which started around 1933. They still have them, but at that time they were fairly new. It was a new

data set that hadn't been available before.

Before I was very well along in my studies at Michigan--I think it was in my fourth semester--McKenzie became very ill and I had to take his classes. One of those classes was a huge course in human ecology--about 100 people. The other was a course in population. This was presumptuous, but it wasn't my fault; I was told to do it.

Then I stayed in that role. McKenzie died very soon after I started this teaching assignment. I was the logical person to fill this spot and they kept me. This was in the late 1930s. Candidates for jobs were not numerous and positions were even less numerous. So, in retrospect, I was delighted to have that opportunity. So I stayed on in the department of sociology at Michigan and became chairman in 1951.

VDT: Along the way you picked up your doctorate; you did that while you were teaching?

HAWLEY: Yes. The graduate program at Michigan was not very structured. The students taught themselves about as much as anything else.

VDT: You learnt from reading assignments?

HAWLEY: Oh yes--and seminars. Well, my first years as a master's candidate, I had to take a full load. After that I never took more than a course a semester.

But we had a very rigorous preliminary examination program. We had five examinations, no choice, done on five consecutive days and for eight hours a day. This was for Ph.D. candidates. That happened about my third year at Michigan. That had been preceded by a very intense period of reading and tutorial consultations with faculty members. Of course, students learn a lot from one another, and, of course, as a teacher I was learning a lot. Teachers learn more than their students, I think. In any event, that's how it was done.

I finished in 1940. I was supposed to take the oral examinations in May; convocation was in June. McKenzie died the last week of April. He was my dissertation chairman, and I couldn't have the final examination. They had to get a new chairman and reconstitute the committee.

VDT: This was for the doctorate. You had finished your dissertation?

HAWLEY: Yes, this was the oral examination. I don't know whether McKenzie had read it. Anyway, I did get the examination concluded in June or July, but Michigan had no summer convocation, so I had to wait till January for the convocation.

Beginning in September of 1940, I was a teaching fellow. When I got the doctorate a few months later, I became an instructor. They were very sticky about these things; I couldn't be an instructor without the degree, though everything else was done, but the degree hadn't been granted.

January 1941 came and I got the degree and became a full-fledged member of the faculty, and continued to teach ecology, population, migration courses, occasionally an introductory course. Twelve hours of teaching were then required. Now it's an insult to be asked to teach more than two courses. We had to teach four. Pretty soon I found myself teaching a course in statistics; there were no courses in statistics before then. That was again presumptuous. Well, so much for those years. Where do I go from here?

VDT: You mentioned that you were interested in human ecology already as an undergraduate. Then you have done a lot in urban studies and in population density. How did you come to be particularly interested in those fields?

HAWLEY: As an undergraduate at Cincinnati . . . I had been out of school for three years after my freshman year at Miami University and decided I had to get back to finish an education and I thought I'd go back and study English literature. But I happened to hear about a very exciting professor of sociology. I sat in his class a few times and decided I'd work in that area. He happened to be a Chicago product and one of his interests was ecology, so under his guidance, I read everything that was written on ecology. James Quinn was his name. He later wrote a book on ecology. McKenzie came to give a talk at Cincinnati. I was very much intrigued by him and his writing, so when I applied to graduate school, I applied to Michigan.

In those days, the education offered to both undergraduate and graduate students was not very good. As I mentioned, there were no courses in statistics.

VDT: You're talking about demography?

HAWLEY: I'm talking about quantitative techniques. There was no instruction in that at all at Michigan, either at the undergraduate or graduate level, until I started a course in statistics, within the department of sociology.

VDT: You started human ecology under Professors Quinn and McKenzie. How did you go on to urban affairs and population density?

HAWLEY: Well, urban matters were within the purview of ecology. The Chicago group viewed Chicago as a laboratory for their studies of ecological theory. There was no clear separation between those studies and ecology. There has become one, but at that time there wasn't. So it was expected of anybody interested in urban studies to be proficient in ecology; the marriage was very close. And so it was in population. These things were different aspects of the same prism.

Population density was no major interest. It happened to arise as a concern with me quite late, and more or less shortly before my presidential year in PAA.

VDT: Your presidential address was called, "Population Density and the City" [Demography, November 1972].

HAWLEY: Well, there'd been a lot of stuff in the literature on density as a causal factor of all kinds of maladies, behavior, in the cities and I thought it was useful to look into these matters. Density wasn't really a major concern of mine, although many aspects of population complement one another. I was interested in fertility, in migration, population distribution. Most of my work was done in population distribution, I think. But I went out to Thailand to direct a family planning demonstration project [Photharam study, a baseline survey for family planning program experiments in Thailand, 1964-65]. That was my first and last interest in family planning. I haven't pursued that at all.

VDT: How was it that you were asked to do that for the Population Council?

HAWLEY: I don't really know. Well, I'd worked in the Philippines. I went there for the U.S. Overseas Missions, USOM, that preceded AID. It had a contract with the University of Michigan to provide advice to the government of the Philippines on certain matters, introducing teaching. I taught at the University of the Philippines and I was adviser to the government on population distribution [1953-54]. In order to pacify the insurrectionists, they were promising them land if they would turn in their arms, and there were all kinds of problems of mobilizing and redistributing and then settling them. I worked on that.

Also, I had been adviser to the Ford Foundation in the Caribbean area--Jamaica and Trinidad and Tobago. I went to the Caribbean many times.

VDT: What were you doing there?

HAWLEY: I was looking over the population programs they had, their census activities. I helped select the person to run the family planning program in Jamaica. Later I devised a census for the Netherlands Antilles.

VDT: Jamaica was one of the early family planning programs in the Caribbean.

HAWLEY: Yes, although I think Barbados had been doing something.

Jamaica and Trinidad and Tobago and one other island had formed a federation that broke up. Nevertheless, they continued to have very close relationships. They had a common library under one administration and common planning on various economic development issues. So family planning in Jamaica was not unrelated to family planning in Trinidad and Tobago; at least they interchanged ideas and tried to support one another where they could. So they had some general discussions and when I first began to go there the question was who was going to direct it, what kind of person should he be, and what facilities would he have. We picked a man who turned out to be not a very good choice. This was in the mid-1960s. I had returned from a Fulbright in Italy [Fulbright research scholar, University of Naples, 1959]. In 1960 I went to the Netherlands Antilles.

VDT: You say your time in Thailand, in 1964-65, was the only time you have been involved with a family planning program. But now you say you at least recommended someone for the family planning program in the Caribbean.

HAWLEY: Yes, but I was not actively involved in operations in the field.

VDT: You were involved in Thailand, with a very important baseline survey before the government really got into their program, which has been very successful--with a big help from Mechai.

HAWLEY: Right, very successful. The Thais were ready for it. They didn't have any religious problems with family planning. They were a little worried about the abortion effects of intrauterine devices, but apart from that there was no resistance. We worked in a rural village [Photharam] some 85 kilometers west of Bangkok, but an isolated place so far as people's sophistication went. Nevertheless, they had been having sterilization. It was done often by folk doctors, often with infections following.

VDT: But they were obviously that anxious to have them?

HAWLEY: Yes. As a woman told me, "My mother had eight or nine children, but only four of them got to adulthood. I'm having four and all of them are getting to adulthood." She said that was too many to handle. They were ready for it and it has been a very successful program.

Two or three doctors at Chulalongkorn Hospital in Bangkok decided they'd like to begin to supply patients with intrauterine devices. We gave them a supply of IUDs. The word got out, entirely by word of mouth, and people came in from all over the country for services. Monday morning was the registration day and there might have been as many as 800 women sitting out on the grass waiting to register for this service. They'd come by all kinds of transportation. It got to be such a burden;

these doctors couldn't do anything else but insert these things and remove them. It got very boring.

VDT: It's obviously been one of the most successful programs in the world. Although they're not down to replacement fertility yet, they're approaching it.

HAWLEY: They've cut it in half or more.

VDT: You went to Malaysia some years later [1973-74]. What did you do there?

HAWLEY: The thought there was that the economic planning unit of the government should have a demographic unit within it in order to give a firm demographic basis to the planning. I went out there partly to bring this about, partly to help the census exploit its data; spent half time in the family planning unit and half time in the census office. This was after the 1973 census. The English civil service system was such that people only did what was written into their job specifications, so the director of the census took the census but he wasn't under any obligation to analyze it. So here were all these data lying there and the economic planning unit and other agencies needing population information. So we started to exploit these data. Another young man, Charles Hirschman, who was then at Duke, went out with me. He stayed in the census full time; I was there half time. We didn't get a demographic unit established but we did make a significant impact on the five-year plan that was shaping up. For the first time, it did incorporate a lot of thinking about population trends.

VDT: You had some local Malaysians who worked with you on the analysis? Had they been trained before, or did you train them on the spot?

HAWLEY: No. In economic planning there were three, in addition to myself. But one of them was an administrator and he didn't do anything. The second one was very casual, concerned mainly with other interests. The third one was a young Chinese, a bright fellow, who did a lot. He since did his MBA at Harvard.

VDT: One question I wanted to ask you was about the importance of Michigan, where you were until 1966, and the University of North Carolina in the training of demographers from less developed countries and the work you did out in those countries, the close ties between them. Why is it that those two universities became outstanding in that? Was it because you and Ronald Freedman did go out and do these things?

HAWLEY: Well, Ronald Freedman and I and David Goldberg at Michigan developed what is now the Population Studies Center at Michigan. Under Ron's guidance, as you know, it has flourished as the outstanding center in the country.

VDT: In all ways, or in its ties with less developed countries?

HAWLEY: In all ways. It's really first-rate. I assume it will remain so, now that Ron has retired. That remains to be seen. He's a man who is gifted in everything he lays his hands on; he's a success.

In the late 1960s, interest was being developed here at the University of North Carolina. Before I got here [in 1966], there was interest in population training. It was largely training for services rather than training for research. That was when the Ford Foundation money was coming in. Moye Freymann was hired as the director of this developing center [Carolina Population Center], before it had really taken form. I came in at the same time to bring sociological interests to the Center.

This became a developing and growing thing at the university. It is now on a sounder basis than it was when Freymann was director [1966-74]. Freymann was more interested in services. The faculty is more interested in doing research and teaching.

VDT: Which faculty are you talking about? I get confused between the Carolina Population Center and the main faculty of the university.

HAWLEY: The Center has a director and some supporting staff. All of the professional personnel are on the faculty of the social science departments, the School of Public Health, the medical school. Now they're called associates--and this was from the 1970s, I think--who get services from the Center and do a lot of the work of the Center. So the faculty are in a sense the Center.

[Richard] Udry has done a very good job in holding these people together, stimulating them, and providing them with resources. So under him, the Center has flourished. Under Freymann, there were all kinds of stresses and strains, because a lot of the faculty wanted little or nothing to do with services. Others did a lot with services. The public health people wanted to advise on family planning programs, engage in institutional development, as they called it, including means of introducing population programs into developing countries. They even tried to start a program called population education, for elementary schools.

VDT: In the U.S. or developing countries?

HAWLEY: In the U.S. and in developing countries. I don't think it came to anything.

VDT: That was in the early 1970s?

HAWLEY: Yes. Elementary schools are not the place to start that kind of instruction.

VDT: Well, I don't know. I come out of the Population Reference Bureau, which has had a strong population department for many years. It's true they tried to get it down to the elementary school level, but it mostly comes in high school.

HAWLEY: True.

VDT: You obviously had a part in Michigan and North Carolina's developing into leading centers of population training. That leads into my next questions, one of which is who you see as your leading students. I've already had an interview with one, who gives you a lot of credit. But first, who have been some of the leading influences in your career? You have mentioned Dr. McKenzie, and Quinn, going back to your undergraduate work in human ecology. Who would you say have been some of the important people who influenced you in the directions you've taken in your career?

HAWLEY: One person at Michigan was a man named Clark Tibbitts. He wasn't interested in population as such, but he was a very nice scholar and I learned a lot from him about methodology.

Later I paid a lot of attention to Frank Notestein. My main connection was through his writings. Later, when I was connected with the Ford Foundation, he was at the Population Council and I then had close personal relations with him. He was a very encouraging man. Extremely tolerant of other ideas; himself, very inventive. Delightful fellow to work for.

I worked with Warren Thompson on a study of metropolitan changes. He was at the Scripps Foundation with Pat Whelpton.

VDT: Was that one of the census monographs?

HAWLEY: No, this preceded the census monograph. My work came out in a book called The Changing Shape of Metropolitan America [1956], which was a population deconcentration study. It was about that time that Ronald Freedman got involved with Whelpton on the national study of fertility, the Growth of American Families.

VDT: That was in 1955, the first one. What else about Freedman? You must have worked very closely at Michigan.

HAWLEY: Yes, we did. In fact, he was a student in one of my classes when I was an instructor. Even then he made an impression on me, because he asked penetrating questions. Then he went off to the war; then went to the University of Chicago; and was eager to get back to the faculty when he finished his work at Chicago. The Michigan department was delighted to add him to its faculty [in 1946].

I was chairman of the department [of sociology] from 1952 to 1961 and I was rather instrumental in helping to get the Detroit Area Study begun. My most important contribution was to get that thing on the university's regular budget. I was able to argue that scientists have their laboratory, which is financed. We needed this kind of training facility and here was a good way to do it.

VDT: By training facility, you meant here was an opportunity for students to be involved in everything from data collection to analysis?

HAWLEY: Right.

VDT: Wasn't William Pratt involved in that?

HAWLEY: Yes, he was. And John Aird, you know him?

VDT: Oh yes. Bill Pratt, of course, went on to direct the National Survey of Family Growth.

HAWLEY: Yes. A lot of people around the country went through the Detroit Area Study. It was a great training facility--painful, for them, because they had to work very hard. They had to participate in all steps of the design, do the interviewing, do the analysis.

VDT: That was done by the students--not by the Survey Research Center of Michigan, which is so well known?

HAWLEY: This was done by the students. Later it did get some administrative services from the Survey Research Center. We'd have about 20 or 30 students in the study every year and a number of faculty. It was given the opportunity to design a research study that could be carried out in Detroit through this facility. Two faculty members would work as a pair, working with the students.

VDT: The Detroit Area Study interviewed women and then went back 16 years later?

HAWLEY: Yes, there were some followup studies, some by telephone and some by direct interview.

It wasn't all fecundity and fertility. They studied various kinds of problems, some of them political problems, voting behavior, some migration intentions.

VDT: Were you responsible for the migration intentions?

HAWLEY: No, I never did. I was chairman of the department; I didn't have time for that kind of thing. The kind of problem they studied each year depended on the research interest of the faculty member who joined the project and his joining it was the decision of Ron and a committee as to whether or not it was feasible to do the proposed study and had training value for students. So there was some selection among the faculty to have this opportunity. But, as I say, the range was considerable.

One of my students at the time was Leslie Kish. He was not long ago president of the American Statistical Association. He designed the Chinese fertility survey of 1982 with Ansley Coale and Ron. He's been important--not so much in demography; his field is more in sampling, the problems of cluster sampling. He's written several books on sampling and measurement of sampling errors.

Don Bogue was another one of my students, my first student, in fact.

VDT: Your first student to get a Ph.D.--before he went off to the war?

HAWLEY: No, after he came back. His interest then was not in demography as such, it was in ecology. He wrote a dissertation on urban matters. He later got into population.

VDT: Did you steer him that way?

HAWLEY: No, I don't think so, although we did a paper together on population distribution, quite early. He then went to the Scripps Foundation, his first job, and of course they were primarily concerned with population questions. Then he went to the faculty of the University of Chicago and developed his Community and Family Study Center.

VDT: One of your students whom I've interviewed recently and who praises you as a leading influence in his career is Jack Kantner.

HAWLEY: Well, he did his dissertation under Ron Freedman.

VDT: Yes, but he nevertheless mentioned you specifically.

You have had your own particular interests but what do you see as leading issues in U.S. demography over the years you've been involved, say, since World War II?

HAWLEY: There were two issues really. One was the demographic transition, which Notestein is often credited with formulating. There had been a lot of concern about population decline in the late 1930s. Then, of course, the 1940s came and trends turned the other way, so an interest in fertility blossomed. That hadn't been terribly prominent beforehand.

VDT: Even though they were concerned about the declining birth rate in the Depression?

HAWLEY: Well, it was there. But the main problem, of course, was the relation of population to the economy and how it affected economic events and how economic events affected population. And the relation of population movements to urban growth and change. Internal migration in the United States was a big concern under investigation by the Truman committee. When Truman was a senator, he was

chairman of a committee to investigate, first, internal migration; later it was called national defense migration. That was in the 1930s and early 1940s. They put out a big 27-volume report. So these issues were uppermost at the time.

Fertility, of course, has continued to be a prominent concern and I sometimes wonder if they haven't exhausted the topic.

Now, I think, migration is becoming increasingly important, in developing countries in particular--internal migration within developing countries. In 1977 the United Nations took a survey of population policies of about 115 countries; about 80 of these countries had population movement policies. Very few of them had implemented them, but concern was there. I think since then, they've begun to do more with them.

More recently, the growth of nonmetropolitan population has attracted particular attention, in this country primarily. I don't think that has happened in developing countries yet; they don't have the transportation facilities that America has.

VDT: I thought the interest in nonmetropolitan growth in the U.S. was in the 1970s, when data between censuses showed that nonmetropolitan counties grew faster than metropolitan counties for the first time since the 1790s almost. But it's changed now.

HAWLEY: It's changed, but the question is, is it a temporary change or is it not?

VDT: You think it might be temporary?

HAWLEY: I think it might be a temporary change, yes--that there will be a shift again to faster growth in nonmetropolitan counties.

VDT: Well, from 1980 to 1984, there was once again faster growth in metropolitan counties.

HAWLEY: Yes, and adjacent ones. I think this is a temporary reversal of the long-term trend toward deconcentration.

VDT: So you think the 1970s were the beginning of a long-term trend?

HAWLEY: I think the 1970s were a continuation of an underlying long-term trend. Metropolitan areas have been deconcentrating since 1920. The area of high growth rates moved progressively farther out. It isn't a postwar phenomenon; it's a long-term phenomenon and it just looked as though it had had a quantum leap in the 1970s--to the areas beyond the counties adjacent to metropolitan areas.

It's reasonable that this should happen, because if you facilitate transportation and communication, people no longer have to be close to one another. Of course, a lot of heavy industry is being robotized and service industry is gaining in importance. So I think the trend in the late 1970s and early 1980s is an aberration. It will change again.

VDT: So, you think of the ring cities around big cities that attract service industries and then people commute from still farther out, from what is technically still counted as nonmetropolitan counties?

HAWLEY: Yes, and this area is an example. This area is growing by leaps and bounds.

VDT: Research Triangle Park? That must be a metropolitan county though, isn't it?

HAWLEY: Yes, although some nonmetropolitan counties are getting growth out of this--Chatham County to the south of us, Person County to the north, and various other ones. And Orange County is metropolitan by courtesy of the Census Bureau, in that metropolitan has been very loosely interpreted.

VDT: Of course, there's just been another redefinition of SMSAs.

HAWLEY: Where there's political clout, there's going to be a metropolitan definition.

VDT: You don't think the county you're living in now, including Chapel Hill, is really metropolitan?

HAWLEY: It's certainly suburban to Durham and Raleigh.

VDT: Well, the metropolitan definition includes those suburban counties that are socioeconomically tied in with that central city.

HAWLEY: Yes, but we have three cities, no central city--Raleigh, Durham, and Chapel Hill. Well, that's beside the point.

VDT: Not really. My very last question in the interview was going to be, what do you think about central cities? In your PAA presidential address of 1972 ["Population Density and the City"], you were talking at that time about rejuvenating central cities. Do you think that's a trend that will continue?

HAWLEY: I don't think that I was optimistic about rejuvenation of central cities.

VDT: I don't think you were. You thought that gentrification was pretty superficial perhaps.

HAWLEY: Well, we don't know. The population in those gentrification programs are either single adults or couples without children and it does still seem that when the family takes form, with children, they move out. So it's a question of whether that kind of population can supply much growth for the old centers of cities.

In the meantime, of course, the city's economic base, in large metropolitan centers, is becoming more a matter of information processing than large-scale production. That has moved out pretty far and has become mechanized as well. So it's a question of whether information processing as an economic base can support these big agglomerations. And, of course, that puts gentrification into doubt. There have been some very interesting examples of it; many cities are doing interesting things with renovating their old buildings. It's very costly; I hope they can occupy that space.

VDT: Washington, D.C., is a prime example of that.

HAWLEY: Yes. They run over \$100,000, a lot of those apartments. So I'm not very optimistic about the future of the large city. But I'm not prepared to say what's going to happen. The whole urban population might regroup in some other pattern, perhaps in smaller-scaled cities.

VDT: Do you think that would be a good idea?

HAWLEY: I don't see anything wrong with it, except that there's a big investment in the large city. I don't see how it's going to be amortized, or liquidated, as the case may be. The federal government has muddied the waters on that considerably; their policies have worked in the opposite fashion.

VDT: Give me an example of that.

HAWLEY: Urban renewal, neighborhood rehabilitation. They've put in a lot of money trying to restore the tax base of big cities, while other agencies were financing sewers and water and amenities in the suburban areas, encouraging deconcentration. I don't think the federal government has had a coherent policy at all on population distribution.

VDT: Now let's talk about your connections with PAA. Do you remember the first meeting you attended?

HAWLEY: Not definitely; it would be back in 1945.

VDT: The meetings were suspended during the war, after 1942. The first meeting after the war was in 1946, in Princeton.

HAWLEY: I think I was there.

VDT: There were two meetings in 1946. There was a second special meeting in New York in the fall [to renew contacts with demographers from other countries]. The 1947 meeting was in Princeton again and Princeton was the venue for many meetings until 1955.

HAWLEY: I became a member in 1945 and attended as many meetings as I could, depending on whether the university was paying expenses or not. I remember one particular one here at Chapel Hill in 1952, I think it was.

VDT: There was a meeting at Chapel Hill in 1951. Rupert Vance was the instigation for that.

HAWLEY: I had a paper there. And I attended several of the Princeton meetings.

VDT: What do you particularly remember about the Chapel Hill meeting?

HAWLEY: Well, one of the interesting aspects of the PAA in its early years was that it was a small group and ran one session at a time; everybody met in that one session. In the afternoon, there would be another meeting and everybody who was going to a meeting at all would go to that session. That was true of the meeting here at Chapel Hill. We stayed at the Carolina Inn. It was small, only half as big as it is now; it couldn't accommodate many people. Rupert Vance organized transportation from the airport. I think Dan Price discussed my paper at that meeting. It was on a migration study of Michigan movements. [Dan Price did indeed discuss Amos Hawley's paper on "Intrastate Migration in Michigan, 1935-1940," but this paper was presented at the 1952 meeting, in Princeton.]

VDT: All the notables were there to hear you.

HAWLEY: Everybody went to everything; had to listen. Dudley Kirk was there and the Taeubers, Dorothy Thomas, Warren Tompson and Whelpton.

VDT: Rupert Vance was elected president at that meeting and served from 1951 to 1952. He was followed by Clyde Kiser. Did you ever work with Clyde Kiser?

HAWLEY: I've known of him. He's now retired down here, in Bessemer City, about 250 miles away.

One of the interesting persons [at early PAA meetings] was Dudley Kirk. Dudley was a very serious fellow, acted like a Bostonian--reserved and very sedate. He was instrumental in my going to Bangkok, or after I agreed to go, he did a lot to facilitate my going, helped with preparation.

Another person of considerable interest at the time and for some time thereafter was Henry Shryock. I was first vice-president when he was president, in 1955-56. The meeting then [1956] was at Ann Arbor. We entertained the whole group at our house--Ron Freedman and I did. The Freedmans and the Hawleys lived next door to each other.

VDT: You had an open cocktail party and people wandered from one house to the other?

HAWLEY: Yes.

VDT: You were first vice-president then, so you were very involved.

HAWLEY: First vice-presidents then were responsible for the program.

VDT: That gave you more to do than most first vice-presidents now have. How did you manage that?

HAWLEY: One created his own internal group to help identify people and topics and other related matters, so I just drew upon my colleagues in the department--Ron Freedman, David Goldberg, and some of the students.

VDT: Was it just happenstance that the meeting that year was at Ann Arbor?

HAWLEY: I think so.

VDT: Was it as they do now, that they would plan the meeting place some years in advance?

HAWLEY: Well, at least a year in advance. The hotel accommodation problem wasn't then a consideration, and not for sometime thereafter.

VDT: Where were the actual meetings at Ann Arbor?

HAWLEY: They were in a university building. By that time, there were several overlapping sessions. Not many; maybe two or three in the morning; two or three in the afternoon. There was a banquet; I think it was held in what was then the Michigan Union. [There were two simultaneous sessions on the first morning of the 1956 meeting, the first meeting with overlapping sessions. Sessions were held in the Rackham building and the banquet in the Michigan Union.]

VDT: By the time you were president, in 1971-72 [1972 meeting in Toronto, Canada], was the president responsible for the program?

HAWLEY: No, we had a program committee by then.

VDT: Which would be headed by whom?

HAWLEY: It was an elected position, and the committee was elected as well.

VDT: So they could be people in different geographical locations. It wasn't so easy.

HAWLEY: Sure, although I think the chairman must have called upon his nearby colleagues. [First vice-presidents were responsible for meeting programs until 1976, when this became the responsibility of the president. These chairman, in both cases, appointed their own committees.]

VDT: What were the issues when you were president? I must pass on a compliment about you from your secretary-treasurer, Jim Brackett, whom I interviewed a few weeks ago. He said you were a very easily reachable and concerned PAA president; you were very cooperative. But what did you do if you didn't do the program? What was the president concerned with?

HAWLEY: He chaired the Board meetings. And we had two or three meetings over the year of an executive committee--president, vice-presidents, secretary, and one or two other people. Those were essentially the president's job. He set the agenda, of course, for the business meeting and chaired the business meeting [for all members at the annual meeting].

VDT: Andy Lunde asked me to remind you that during your time there was a flap over a change in the constitution. What was that?

HAWLEY: Did he tell you the name of the man who chaired that committee?

VDT: Forrest Linder chaired the Committee on Organizational Management, but that was in the late 1960s [1966-67], before your time.

HAWLEY: I don't know of any constitutional change that was under discussion after that, or at the time I was president. I remember there was quite a flap over Forrest Linder's proposed reorganization. He was very dismayed that they voted it down.

VDT: Ansley Coale came in with many objections and he wasn't going to carry out the committee's recommendations when he became president [in 1967-68].

HAWLEY: Forrest wanted to make it a very selective organization. But the membership--maybe Ansley Coale was responsible for this attitude in the membership--wanted to keep the organization open.

VDT: Paul Glick wrote this up as a "Vignette of PAA History" in the newsletter [PAA Affairs, Summer 1982]. It happened when he was president, in 1966-67. The committee recommended that there should be two categories of members, the "pure" demographers and the less pure people in other population fields. There were other things taken up at that time, which Ansley Coale objected very strenuously to.

Also, this was the time that Donald Bogue was expanding the PAA membership enormously through his promotion of Demography, having such thick issues of Demography. You remember that?

HAWLEY: Yes, the Association is indebted to him for Demography.

VDT: Indeed, he was editor for the first five years [1964-68].

HAWLEY: And where he got the money, I don't know. He didn't get it from the Population Association.

VDT: Well, he got some subsidy. Then the final issue [Vol. 5, No. 2, 1968], the thick issue with the Indian family planning upside-down red triangle on the cover, his Center paid for, in the end. [Demography was launched through the Ford Foundation support of Bogue's Community and Family Study Center, followed by a \$30,000, three-year grant from the National Science Foundation. See Bogue, Lunde, and Glick interviews above.]

HAWLEY: One of the issues when I was president, and I think it was also an issue when Nathan Keyfitz was president before me [1970-71], was women who wanted the Association to take a position on women's rights in the profession. Some of us felt it wasn't the responsibility of a professional association to become a political pressure group. In any case, there was only one department of demography at that time--at Berkeley.

VDT: The women were pressing for women's rights within university departments?

HAWLEY: Primarily. In the profession, but the profession was still largely within university departments. I don't know whether women have free access within the Census Bureau or not; probably not as much as they would like. But this was a concern.

There were other issues that came from the floor from members, recommendations on policies in developing countries, which of course the Association could not support.

VDT: Can you give me an example?

HAWLEY: There was a case in Argentina, where a demographer had run afoul of the government and some members wanted us to send a memorandum to the Argentine government concerning this.

VDT: Had that demographer been jailed or something?

HAWLEY: Probably. And there were other issues of that kind that had come up in totalitarian countries.

We did, however, at the end of my term [1971-72] send out a one-page questionnaire, printed at the back of PAA Affairs, asking about the problems of women in the profession: the rank they held, salaries, demographic data, and then a series of questions on professional problems they faced. That was analyzed and reported to the next meeting. I was not then present; I never heard what that report had to say about all that. [A Committee on Discrimination Against Women and Minority Groups in the Profession, appointed after passage of a Women's Caucus resolution at the 1971 meeting, designed a questionnaire to be distributed to all PAA members "to ascertain the number of women and minorities in the profession and their professional status." This survey was conducted in May 1973 and some results were published in PAA Affairs in 1974 and 1975. See Harriet Presser, vignette on the history of PAA's Women's Caucus, PAA Affairs, Winter 1981.]

VDT: Of course, the Women's Caucus was formed, and there have been membership surveys and the women's angle has been analyzed.

HAWLEY: There was a Women's Caucus during my term. That may have been the first one. [PAA's continuing Women's Caucus was formed at the Atlanta meeting in 1970.]

VDT: The Women's Caucus was involved in the famous Toronto meeting in 1972, where you gave your presidential address, when women were excluded from the bar in the hotel. That's my hometown and the first meeting I attended. I was embarrassed that PAA was meeting in that hotel [King Edward], because it was seedy and rundown. It's not any longer; it's been fixed up.

Have you attended most PAA meetings over the years?

HAWLEY: Yes, up until recent years. Recently, I haven't attended that many. I took the position that I wouldn't attend meetings unless I had something to do there.

VDT: Now they've got you doing something in New Orleans [April 21-23, 1988, meeting]. You'll preside over that luncheon roundtable discussion on "The Ecological Perspective in Population Study." You're certainly known as Mr. Ecology of population. Was anyone else in that field--population ecology?

HAWLEY: Oh, a lot of my students: Krishnan Namboodiri, now at Ohio State; Jack Kasarda, who is chairman of the department of sociology here [Chapel Hill]; Jack Kantner, whose interests have turned more toward fertility; Don Bogue, of course, who has been in and out of this and he published some papers recently on ecology and population in the city of Chicago. A fair number.

I guess I'm more distinguished for my preoccupation with theoretical questions. Most of these others were more concerned with empirical problems. I've done some of that, but it seemed to me that the theoretical questions were unsolved and needed a lot of attention.

VDT: The over-arching theory?

HAWLEY: Yes. Well, some not so grand.

VDT: I've heard in some of these interviews that some think, for instance, that demographers nowadays have become too technical and micorcomputer-oriented, to the detriment of over-arching theories.

HAWLEY: I'm not too sure too technical. I do think that some of the problems are pursued to the micro level excessively. I think now is the time for someone to do a synthetic job on fertility--a review of the field and draw together the state of knowledge, a synthesis.

VDT: You don't think that's been done?

HAWLEY: Ron Freedman did it for a United Nations committee some while ago, but that's almost 25 years ago. It's a little booklet; I can't give you the title. It was on the status of fertility research, sort of a state-of-knowledge summation, very good, very compact ["The Sociology of Human Fertility: A Trend Report and Bibliography," *Current Sociology*, Vol. X/XI, No. 2, 1961-62, prepared for UNESCO]. I don't know if anybody has done that recently, do you?

VDT: I've just read Forty Years of Research in Human Fertility [Milbank Memorial Fund, 1971], which was the proceedings of a two-day conference in 1971 in honor of Clyde Kiser when he retired from the Milbank Memorial Fund. But, no, I don't think anyone has summed it up recently. You'd like to see something of that sort?

HAWLEY: I think it's needed.

VDT: Ron Freedman did do that long article in Studies in Family Planning recently ["The Contribution of Social Science Research to Population Policy and Family Planning Program Effectiveness," Studies in Family Planning, March/April 1987]. It was mainly the work in developing countries, not exactly the state of fertility research, which he could do.

HAWLEY: He'd be the one to do it. So would Ansley Coale.

VDT: Ansley might be a little more technical. I'll suggest it to them--or you suggest it to them at New Orleans. They're both on the program; they'll be there.

What do you see as the outlook for demography as a discipline in the United States? Are demographers more needed than ever?

HAWLEY: Oh, yes. Well, I think their value is widely recognized. As a consequence, there are very few departments in the United States that do not now have courses in demography. And I think demographers are being consulted, obviously in governmental offices, not just in the Census Bureau but in the Department of Agriculture and numerous other places.

Last week some lawyers called me from Washington, who were trying a case on deregulation of the trucking industry, and they wanted demographic advice on what constituted a commercial zone. To put it differently, the Interstate Commerce Commission was saying that places of 100,000 or more should have deregulation extended to 50-mile radiuses, so the question was: Does a commercial zone reach out to a 50-mile radius from the city center? Then it's 100 miles across. They were contesting this and they wanted to know if there was any demographic information that would help them resolve that. That's a case in point.

Businesses are using demography, state governments are hiring demographers. One of my students is the demographer for the state of North Carolina.

So I don't think there's any question about the security of the demographic profession in the public view. And demography is a major concern now in a great many developing countries--India, China, Japan. Some of these countries are producing first-rate people.

VDT: Who are being trained within the countries themselves?

HAWLEY: Yes. They used to depend on us; now they're doing it themselves. And very well. I helped institute a program in the University of Kerala; that must have been 1962, 61.

VDT: Just before you went to Thailand. You actually went out and helped on the spot?

HAWLEY: Yes, I went out there and designed a curriculum in consultation with people in the statistics department. Then they took two of our students from Michigan to staff that program. One of them is now at Ohio State. He couldn't stand the seniority restrictions and left. He's Indian; he's a Brahmin as a matter of fact. He went there on that faculty and became dissatisfied and came back to this department, North Carolina, and recently moved on to Ohio State. That was Namboodiri.

VDT: How did you find time to have these overseas postings--India, Malaysia, Caribbean--while teaching?

HAWLEY: I took occasional leaves of absence. Sometimes universities are glad to have people leaving.

VDT: To give some of your colleagues a chance to teach courses?

HAWLEY: Yes, and to give universities some budget relief. At Michigan they used to say if all our faculty came back in any one year, we'd be in desperate financial state.

VDT: Who, for instance, funded you when you were in India, setting up the Kerala program?

HAWLEY: That was the Ford Foundation. It probably came through our population center.

VDT: Do you think those ties with developing countries will continue--with American universities, in demography?

HAWLEY: I think so. The big unexplored area is Africa south of the Sahara. If that ever stirs, it will be a great opportunity for consultation.

VDT: Well, of course, there have been Americans who have gone out to Kenya and Nigeria. Frank and Susan Mott were there, and others.

HAWLEY: Andy Lunde went out to Nigeria once. He was there only about a month; that was training in the census, vital statistics, and record-keeping. Nevertheless, there's an awful lot of territory there that needs some assistance. And elsewhere--Latin America and parts of Asia still are fertile ground for some aid.

VDT: You enjoyed those overseas assignments?

HAWLEY: Very much. Particularly the Thai one. I was dissatisfied with the Malaysian one. It seemed to me that if a country really wants a person, you can go there and do a job. But if the country has been sold a bill of goods by someone, then the person that goes out has trouble. In Malaysia, I was sent out to organize a demographic unit. I was confident shortly after I got there that they had been persuaded to do this, although they didn't really believe in it.

VDT: Was that the result of Bucharest, when the World Plan of Action said there should be such units? That was 1974.

HAWLEY: This was 1973. When I got there, they hadn't made any preparations. It was three weeks before I had a desk to sit down to. I found the people I wanted to talk to were elusive and hard to find. I had to drag something out to make the thing worthwhile, and I was very discouraged. Although I had a fine experience in the census office.

VDT: They didn't think it was their job to analyze the census data but they were willing to let you do it?

HAWLEY: Oh yes, we did a lot of things with their data that wouldn't have been done otherwise. They had a computer establishment. You couldn't do more than straight runs on it; couldn't use it. I think they've since improved that situation.

VDT: Of course, Malaysia has always been a bit ambivalent about population policy. It's the one

country that has backtracked and decided that it now wants to expand its population--at least the prime minister says that--wants to aim for a much larger population, after the family planning program was apparently bringing down fertility.

HAWLEY: Well, there are two things. One is the role of the sultan. Of the 11 states, about seven or eight are still pretty well controlled by the sultans, not officially but symbolically, and their wishes are respected. Sultans are against family planning. So you have a family planning agency in the government, but it's very weak in the rural areas. It's better in the cities, but weak in the rural areas because of the attitude of the sultans. Four of the states, the British kicked the sultans out. That's one of the problems.

Another is the friction between Malays and Chinese, and that is almost palpable everywhere in everything. The Malays are very envious of the Chinese; they're educated and control the economy. The British kept the Malays down on the farm and now they're trying to get off the farm. They have very meager qualifications. They have such legislation as a third of every employment force should be Malay, but you can't find qualified workers. Employers can't find them; even the government can't find them. The government, which is Malay-dominated, won't consult the Chinese, who occupy most of the faculty positions in the universities. They created a Malay university, all the instruction in Malay, but all the books and articles are in English or French and to translate all that material is virtually impossible. The Chinese are walking around in fear. There were major riots in 1967 in which a lot of them were killed. But still, they are the intelligentsia. The separation of Singapore from Malaysia was the result of that.

VDT: What are your plans for the future? In 1978 you were president of the American Sociological Association. You must still be involved with that.

HAWLEY: No, not very. I've gone to a few of the meetings.

VDT: Do you have any special plans for now? You've stayed on in Chapel Hill.

HAWLEY: I retired in 1976 and for about ten years after that, I spent half-time in my office in the university. I had a number of graduate students who hadn't finished and I was on a number of committees. When that finally ended about a year ago, I had very little reason to visit the office. I don't have much left to say. I write little papers now and then and once in a while they get published.

VDT: I hope you'll go on writing those papers, and prodding others to write the papers they should, such as you mentioned with Ron Freedman; he should write a history of fertility research, up to date.

HAWLEY: Not so much a history of research, but a statement of the state of knowledge, what's left to be done. For example, are there any other important unexplored areas in this field or do we continue to look at the infinitely small area? These are the kinds of questions that I think ought to be addressed.

VDT: Do you feel there's a need to do that in human ecology?

HAWLEY: Oh, yes.

VDT: Could you do it?

HAWLEY: Well, I've just recently published a book which is called Human Ecology: A Theoretical

Essay.

VDT: I didn't know that.

HAWLEY: In 1986. In the book, I tried to organize the theory in the field and to bring to bear on it whatever empirical material is available. That's the kind of thing that ought to be done in fertility research.

VDT: Good. I think it's splendid that you did that.

HAWLEY: It's something that came out of the years of teaching.

VDT: You've obviously enjoyed your career.

HAWLEY: Very much. I can't imagine a better one.

VDT: That's a wonderful note to end on. Thank you very much.

HAWLEY: I enjoyed talking with you.

NORMAN B. RYDER

PAA President in 1972-73 [No. 36]. Interview with Jean van der Tak at the Office of Population Research, Princeton University, May 11, 1988.

CAREER HIGHLIGHTS: Norman Ryder, who was born and grew up in Hamilton, Ontario, is one of PAA's three Canadian-born presidents, along with Ronald Freedman (president in 1964-65) and Nathan Keyfitz (1970-71). He received a B.A. in general studies, with emphasis on mathematics and economics, from McMaster University in Hamilton in 1944 and the M.A. in economics from the University of Toronto in 1946. He then went as a Milbank Memorial Fund Fellow to Princeton, where he received the M.A. in economics in 1949 and Ph.D. in sociology in 1951. He returned to Canada for four years to work with the Dominion Bureau of Statistics in Ottawa and on the faculty of the University of Toronto before joining the Scripps Foundation for Research in Population Problems at Miami University in Oxford, Ohio, where he worked on the design and planning for the first Growth of American Families study of 1955. From 1956 to 1971, he was on the faculty of the department of sociology at the University of Wisconsin, where he established the Center for Demography and Ecology. Since 1971 he has been at Princeton with the department of sociology and Office of Population Research, becoming Senior Research Demographer Emeritus in 1989. He is an adviser to Statistics Canada and, among other awards, received PAA's Irene B. Taeuber Award for Excellence in Demographic Research in 1987.

Norman Ryder's insightful research and prolific writings have been focused on the analysis of fertility and family demography. He is particularly well known for his emphasis on the cohort over the period approach to studying fertility and on time patterns in fertility and family changes. With Charles Westoff, he was principal investigator in the National Fertility Studies of 1965, 1970, and 1975, which were reported in, for example, Reproduction in the United States [1965, 1971], The Contraceptive Revolution [1977], and National Fertility Study: Married Women Interviewed in 1970 and 1975 [1980], and they prepared the core questionnaire for the World Fertility Survey.

VDT: What led to your interest in demography?

RYDER: I was a student at a very small college in Canada--small then--named McMaster [in Hamilton, Ontario] and my professor of economics had population as his research speciality. He was the only academic in Canada with an interest in population. His name was Burton Hurd. He did most of his publications on ethnic groups in Canada, working with government statistics. He hired me to help him with the revision of a monograph on ethnic origins. He was instrumental in getting me to become a demographer because of a very specific chain of circumstances. He was on the Board of Directors of the Population Association and therefore had contact with Frank Notestein and Notestein was, of course, director of the only training program that existed then at the graduate level in demography. Notestein asked if there was a likely person in Canada as a candidate for the Milbank Memorial Fund fellowship.

Accordingly, Burton Hurd said to me, "How would you like to be a demographer?" I had to ask him what that was; the word was new to me. He described it and I found it interesting because I'd always been pretty good in arithmetic. Accordingly, he managed to have my name put forth successfully for the fellowship. And that's how I became a demographer.

Now there's a sad part to that story as well. Hurd had a particular reason for wanting to have a successor and that was that he was in very poor health and, in fact, he died three years later.

VDT: You mean he really had you in mind as his successor?

RYDER: That's right. My role was to become trained at Princeton and return to Canada to become the Canadian demographer, at least in the academic world. And I followed that prescription faithfully. I returned to Canada immediately after my education was completed at Princeton, after I got the Ph.D. I went to work for what was then called the Dominion Bureau of Statistics. After a year and a half there, I was on the staff of the University of Toronto for three years, and I was indeed the only academic demographer in Canada.

On the other hand, that has a lot of disadvantages. I was expected to have some kind of professional skills in diverse topics like migration, housing, labor force, education, fertility, life tables, or whatever, and it spreads one rather thin. I also had a strong urge to do research and there were literally no sources of research support in Canada whatsoever. Accordingly, when Pat Whelpton approached me with the idea of joining him at the Scripps Foundation and working on several large research projects, I decided that I was more wedded to my profession than I was to my country.

VDT: That's happened often, at least in those days, with Canadian academics. If you wanted to make your mark academically, you had to come South.

RYDER: Nathan Keyfitz and Ronald Freedman did, and a number of others. [Ronald Freedman came to the U.S. as a child.] It's sad for Canada.

VDT: It is; I hope it's improving. I graduated in history from your University of Toronto and the top people in our department all got good scholarships here at Princeton or at Harvard and so on. Well, the migration still continues. What was your thesis topic?

RYDER: I chose as my thesis the concept of the cohort [The Cohort Approach, Ph.D. dissertation, Princeton, 1951]. It was at that time a subject of some interest and curiosity, particularly in the study of net reproduction rates. I found a number of lengthy series, particularly one for Sweden, which could be converted into cohort form and I turned out what was not so much a dissertation as a collection of essays on the wonders of the cohort approach. I never thought of publishing the work, because it was clearly a methodology in progress [but it was published by Arno Press in 1980]. I wrote up a bibliography for many years and found that the cohort approach was well known in history, psychology, political science, and so on and that, on the other hand, only the demographers had really attempted to seize hold of the technicalities of it. I guess I've spent my whole professional lifetime as a salesman for the cohort approach.

VDT: You have indeed. You wrote already of the parity progression ratio in 1951, your first publication of that. Was that part of your thesis?

RYDER: That was in my thesis. It had originally been a paper that I gave at the Population Association meetings, in which I talked about Canadian cohort fertility and used the parity progression ratio as a particular example of how to analyze data that had that richness of detail ["Long-run and Short-run Changes in Canadian Family Size," presented at the 1951 meeting in Chapel Hill].

I invented the idea as a graduate student here at Princeton. I remember the first time I announced this idea. I said that it is not really a ratio; it's really a probability. One of my fellow students said, "Norm, you're not going to impose on us a PPP, are you?" And that's why it became the PPR.

VDT: Interesting! But then two years later Louis Henry came out with something [in Fecondite des Mariages: Nouvelle Methode de Mesure, 1953]. Had he copied you or was it independent? And his was called a probabilité, wasn't it?

RYDER: He called his a probability; it was "probabilité d'agrandissement." But it was exactly the same thing, and I am quite sure that I didn't borrow from him and he didn't borrow from me. I like to think that in this way I was a little bit in the class of Louis Henry.

VDT: Indeed, you were. I recently reread an article by William Petersen on doing his Dictionary of Demography ["Thoughts on Writing a Dictionary of Demography," Population and Development Review, December 1983]. He points out that Roland Pressat in his dictionary of demography [Dictionnaire de Demographie, 1979]--I've heard this from others too--listed [included short biographies of] only 19 persons for the development of demography and the only four non-European ones were yourself, P.K. Whelpton, Alfred Lotka, and Robert Potter [and Louis Henry among the 15 others]. You know that, probably.

RYDER: No, I didn't know that.

VDT: You didn't! I remember Leon Bouvier racing in--he was to review it [English edition, edited by Christopher Wilson, published 1985]--saying, "Look at this, only 19 demographers!" He didn't particularly point you out, but that's true--you are on that list. These are people who were important in the development of demography ["associated with the origin of concepts, methods of measurement, etc."]. It must be your cohort work. [Petersen was referring to the list in Pressat's original French-language dictionary, published in 1979. The list in the English edition of 1985, edited by Wilson, may be different; Bouvier's review in Population Today mentions that even P.K. Whelpton does not appear in the biographical section of that edition.]

RYDER: I guess so.

VDT: A lot of others were left out, including your distinguished colleagues here. You are indeed known for that. How did you come upon that idea, the importance of cohort over period fertility?

RYDER: The idea came from the assignment of an article by Frank Notestein in his graduate course. It was by Wade Hampton Frost and was on tuberculosis mortality in England and Wales. Wade Hampton Frost had shown that if you take a table of death rates by age for tuberculosis and put them on a diagonal, a complex pattern becomes a very simple one. I thought that was like money for nothing; it was just a magnificent example of how a point of view can clarify an otherwise muddy scene. And indeed the concept did have a very vigorous lifetime within the study of tuberculosis and it made theorists of tuberculosis think very differently about their ideas.

VDT: The original concept had come from the study of tuberculosis; you just borrowed the idea?

RYDER: No, the original cohort article that inspired me was on tuberculosis mortality. And one of the problems in doing my thesis on the cohort approach was that it was very hard to avoid stumbling over people in other fields who had a similar perspective and insight from the cohort approach. There was no way to draw a boundary around the possibilities. You can find a rich discussion in literary history. There's a school of French demographers associated with people like Maurice Bloch and Henri Pirenne who are enthusiastic for what they call generations, which is exactly the same thing.

There was the Spanish philosopher Ortega y Gasset who created a whole body of philosophy focused on the cohort. He called it the most important concept in history.

VDT: Well, it certainly has been seminal in the study of fertility. You were well known for that early on. For instance, you wrote the chapter on fertility in Hauser and Duncan, their landmark Study of Population; that was already in 1959.

RYDER: That was an exciting assignment. I'm quite sure that several others must have been asked before me, because I was just a kid at the time.

VDT: Indeed, you were early in your career, but you must have made your mark already in fertility.

Now, you've already answered one of my main questions, which was what led to your particular interest in the measurement of fertility. Which, of course, you have shown brilliantly is the cohort system. I most recently read, last night, your study of cohort fertility in the U.S. ["Observations on the History of Cohort Fertility in the United States," Population and Development Review, December 1986], the history from the birth cohort of 1867 right up to the present, and you've shown that if it weren't for unintended fertility, the population would long ago have faded away.

RYDER: Let me tell you a little story which is a part of the history of the Population Association that has not been completely explained in the vignettes that you are responsible for ["Vignettes of PAA History," in PAA Affairs]. Back in the early 1960s, I had been at Wisconsin for a few years and I was chairman of a building committee to get a new social science building. Once we moved into the building, there was a large social science library research room, very handsomely appointed, immediately across the hall from my office. The secretary of the department came to me and said, "If we leave that room without a name on it, we're going to lose it; the dean will steal it back from us. We've got to have a name on the door." I said, "All right, let's put the name, Center for Demography and Ecology, on it." She said, "When was it created?" "Just now." And the Center for Demography and Ecology at Wisconsin was born at that moment.

I got myself a few research funds from the National Science Foundation and hired a part-time secretary. I had absolutely no administrative skills, so it was not long thereafter that I convinced the department to hire Hal Winsborough from Duke and as soon as he arrived, I laid the mantle of director on him and he was an excellent choice for it.

The reason I'm telling this--and forgive me if I have a sort of paternal attitude toward it--is that I've just returned from Madison and that is a very thriving institution. It is large and it is successful in all sorts of empirical ways and they are a very fine bunch of people. I am very proud of my association with that organization that I started.

VDT: You can be, indeed. Why did you choose ecology--demography and ecology?

RYDER: That had reasons associated with the Midwestern tradition in demography. Although I was trained as a demographer at Princeton in a pure tradition, if you will, the tradition of demographic interest in the Midwest was heavily overlaid with the interest in human ecology. This was particularly true at Michigan and the University of Chicago. It was a style of sociological research which was macro-analytic in form and which has continued to thrive. The human ecology tradition is, I think, usually accredited to McKenzie at Michigan and was followed up vigorously by people like Amos Hawley and others. It led to a research tradition which places large emphasis on the subject of stratification, for example, and geographic distribution, segregation, and topics of that sort. I recognized that to establish a demography center within a sociology department, it was important to have strong links with non-demographic sociologists. And it seemed to me that a very important kind

of link was the human ecology link.

Now, such a tradition does not exist at Princeton. But if you look at the work of the people that are now most prominent at Wisconsin, people like Bob Hauser, Karl Taeuber, Hal Winsborough, Jim Sweet, and others, you'll find that they are very distinctive in that. Although they are certainly well-trained demographers, they are also very actively pursuing sociological questions within what is broadly called the ecological tradition. So my choice of it was a recognition of the need for breadth in that establishment in that place at that time.

VDT: It seems very logical in principle. Are the poverty people connected with that?

RYDER: There are close links, but the poverty center itself was primarily directed by economists. Hal Watts, for instance, was the head of it for a long time.

VDT: Is that where Larry Bumpass is?

RYDER: Larry Bumpass is part of the demography and ecology center. In fact, when I telephone Larry I have a little lump in my throat because his telephone number used to be mine.

VDT: Tell me about some of the other people you worked with at Wisconsin at the center that you have a paternal attitude toward.

RYDER: The group of people that were gathered at Wisconsin while I was there were primarily from an extraordinarily strong group of graduate students at the University of Michigan. It includes Hauser and Bumpass and Sweet. They have, of course, extraordinary merit in their own right, but I attribute quite a bit of the strength they brought to their mentor, who was Otis Dudley Duncan and his wife Beverly. Those two were a tower of strength in the Michigan department and had an extraordinary productivity in terms of the graduate students that they turned out. Wisconsin has, I think, become recognized as the best sociology department in the country and one of the reasons is that our recruiting sense at that time was that this was the place to go for the very best.

VDT: In sociology, of which demography . . .

RYDER: Is a very strong part. Now demography at this institution, Princeton, has not, I think, been successful in integrating itself closely within the sociology framework.

But I wanted to tell you about one other thing that happened about that time when I started the demography center. I invited the Association to have its annual meeting at Madison, partly to advertise our new center, even though it was not much more than a joke at the time. It [1962 meeting] was the last meeting that the Population Association had on a modest scale. The Association membership was growing so rapidly that from then on we found ourselves in the middle of big cities in big hotels. That was, I think, the last cosy, informal Population Association meeting.

VDT: That was 1962. Where did you actually hold the meeting?

RYDER: We had a new center that had been created on the campus for small conferences and it was just big enough to hold the Population Association. We were, in fact, located in several different hotels because there was no big hotel, but the convention center provided the kind of facilities that really no hotel had. It's one of the regrets of the growing population size of the Association that we have lost, I think, something of the charm of those Gemeinschaft meetings that we used to have.

VDT: On university campuses, indeed.

RYDER: The 1962 meeting had another significance that I was certainly aware of; I'm not sure how many others were. It was the 300th anniversary of John Graunt's book [Natural and Political Observations . . . Made on the Bills of Mortality]. Demography was born in 1662. And here was 1962 and what a fine time to have a meeting at our place!

VDT: Did you have a session on John Graunt?

RYDER: We didn't have one on John Graunt [but John Durand's presidential address was on "Demography's 300th Anniversary"]. I'm afraid that the tradition of study of the history of demography has been neglected by American scholars. It's very strong in Britain, and in France, of course, but very weak in the United States.

VDT: We had the session at New Orleans just past [1988 meeting], with Kingsley Davis chairing ["Two Centuries After Malthus: The History of Demography"].

RYDER: That was notable because it was rare. Look over the programs of the PAA.

Just at that time, I got engaged in another venture, and this was a thing I wanted to mention as an element of the history of the Association. I went to the Association to determine why there was no official journal other than Population Index. I found that there had been extensive discussion of this in the past and the Board of Directors turned over to me all of the minutes of the deliberations of the wisdom or folly of starting a journal. I asked them if I could have small monies for travel to go talk with people who had founded new journals and determine what was necessary to launch a journal successfully. The judgment prior to that had been, I think, excessively conservative. There was a notion that somehow or other there wouldn't be sufficient good manuscripts turned out and that we might even be a source of embarrassment to an established journal like Population Studies. Now, none of this came to pass, of course; it turned out quite the other way.

The Association decided that it was a good idea to have an official journal and I was slated to begin it at the University of Wisconsin. The university had a policy of non-support for official journals. Accordingly, I was unable to get even a spare file cabinet, let alone a half-time secretary or any other person. Reluctantly, therefore, I abandoned the idea. Because I knew it was good thing to do to start the journal, I then went to the University of Michigan and tried to talk them into taking on what is really a substantial responsibility and there was nobody available at the time to do that. Accordingly, I went to the Association and said, "It is very important to start this journal and Don Bogue of the University of Chicago is prepared to take on the responsibility," and I recommended that they do it that way.

VDT: I did not know this about the antecedents of Demography. I just assumed Don Bogue had a dream one night. It's good to know that you had the idea; that is a most important part of the history of PAA. [Ryder's part in the creation of Demography was not mentioned in Donald Bogue's "vignette" on "How Demography was Born," published in PAA Affairs, Fall 1983.]

I want to talk about your part in the important National Fertility Studies of 1965, 1970, and 1975, although that's been well documented in, for example, the series of videotape interviews with directors of U.S. fertility surveys. [This series of six interviews was originated by Barbara Foley Wilson of the National Center of Health Statistics, conducted in summer 1985, and shown at the 1986 PAA meeting. See vignette by Barbara Wilson, "Videotaped Interviews About American Fertility

Surveys," PAA Affairs, Winter 1985.]

Charles Westoff told me yesterday how you and he learnt at a meeting in 1964 that the Scripps Foundation and Ron Freedman were not going to continue the Growth of American Families surveys [conducted in 1955 and 1960], so you rushed around and got money, saying these had to be continued--you working already at Wisconsin; Charles Westoff here at Princeton.

RYDER: There was a little detail about the very beginning that might be interesting for the record; perhaps Charlie told you this, perhaps not. We actually met and discussed this at a meeting of a small group of population geneticists and demographers who met under the auspices of General Fred Osborn for a short meeting, here at Princeton, at which we had papers in common. It was a group that is mostly closely associated now with the Journal of Social Biology. Charlie and I were at the meeting--it was around Christmastime of 1964--and the question was would there be a 1965 study. Michigan was the particular heir to it. Since Ron Freedman had been part of it [co-director of the 1955 Growth of American Families study] and the Survey Research Center [also located at Michigan] had been part of it from the beginning [conducted the fieldwork in 1955 and 1960], surely it was theirs to do if they wanted to. But it appeared that they were not going to do it, for reasons of other commitments. Accordingly, Charlie and I thought that it shouldn't be allowed to die; we should try to raise some money for it.

We went to the Population Council--not because they would be a source but they might help us find some money. At the same time they had been approached by the cancer people [National Cancer Institute], who were very interested in this new phenomenon of the oral contraceptive pill [licensed for use in the U.S. in 1960] and wanted to have some kind of denominator for various calculations that they were making. They had no idea how many women used the new pill, to put it simply. So the Cancer Institute people were prepared to put up the money. I remember we had a particularly tense meeting at which their idea was that our contract would consist of producing a tape. Once we produced the tape, we would turn it over to them and they would play games with it. But we would not operate on that level and I was delighted that the Population Council supported us. They said to the Cancer Institute people, "If these people don't do it, nobody else is available."

VDT: How did you get it done, from Christmas of 1964 when the first idea . . .

RYDER: We worked very hard.

VDT: When was it in the field?

RYDER: Fall of 1965.

Another aspect of that story is that it was apparent to all concerned that it would not be politic for the Cancer Institute to be the financial auspices for a fertility inquiry; it's just not the right connection. Accordingly, they looked around--as I understand it--at Bethesda for an innocuous alternative. And the alternative they found was the National Institute of Child Health and Human Development, who were willing to put this in as an item on their budget.

VDT: You're talking now about the second National Fertility Study? NICHD was not formed until 1968. I've just interviewed Art Campbell, so I know they're celebrating their 20th anniversary.

RYDER: No, that is the Center for Population Research [of NICHD]. Forgive me for contradicting you. The National Institute of Child Health and Human Development was in place and they were

willing to have the fertility study as a line item in their budget, even though they had had nothing to do with the survey that was being financed. Subsequently, NICHD, with this as a starting point, enlarged the idea into more extensive population activity. I know of very few people who are aware that it is a little bit odd in the structure of NIH for population research to be housed within Child Health and Human Development; it's not a natural. Well, that's how it happened.

VDT: From your first NFS survey. Interesting!

RYDER: Right. So, when Campbell talks to you about the operation beginning, what began was a formal organizational unit [Center for Population Research] connected with population research in NICHD.

VDT: That grew into a long and healthy connection, because that's where most of the funds come from for population research.

RYDER: That's perfectly true. There was, incidentally, some considerable negative opinion about the idea, particularly from one of the Kennedy ladies who was on the board and she objected to the idea of money intended for child health being used to prevent children from coming into the world.

VDT: Eunice Shriver.

RYDER: That's right. She made quite a bit of noise at the beginning, but she lost the argument.

VDT: Thank goodness, because then you and Charlie were able to carry on the magnificent tradition of the surveys, which, of course, led to your work on the World Fertility Survey.

RYDER: Yes. We feel most excited, Charlie and I, about the rather extraordinary compliment that was paid to us when the World Fertility Survey was in effect conceived as a replication of our study in many countries. That's a very exciting idea. And as you probably were told, we were responsible for the core questionnaire.

VDT: Yes, I didn't have to be told.

RYDER: Hundreds of people contributed, but it was our document.

VDT: And you can be very proud. That in turn led to the Contraceptive Prevalence Surveys and now the Demographic and Health Surveys--although I presume you've seen Kingsley Davis's just-published critique of the World Fertility Survey ["The World's Most Expensive Survey," Sociological Forum, Special Issue: Demography as an Interdiscipline, Fall 1987].

RYDER: Well, I published a lengthy and, I think, in tone quite critical review of the World Fertility Survey in a book review in Population and Development Review [review of John Cleland and John Hobcraft, eds., Reproductive Change in Developing Countries: Insights from the World Fertility Survey, in Population and Development Review, June 1986]. It seemed to me that there were a number of respects in which the survey could be found wanting, and I indicated there . . . I don't want to rehearse it now.

What I learned over the years--I don't know if this is something that others feel but it's certainly my opinion--is that a survey seems to me an indispensable medium for collecting certain kinds of

information, but at the same time it has inherent limitations. There are also some kinds of information that it's just no good at getting at all. And that the modest yield of the World Fertility Survey from the standpoint of social and cultural analysis is not because the practitioners were inept but that the medium they were using is not well suited to do that kind of work.

VDT: You have said, for instance in the article you had on "Fertility and Family Structure" in the UN Population Bulletin [No. 15] in 1983, what you've just said, that a survey neglects institutional settings. Are you now a promoter of the participant observation process that has come to be associated with the Caldwells?--going to live in a village and really . . .

RYDER: I have a great deal of empathy for Jack Caldwell's position. On the other hand, I think the prescription for advancing demography is somehow or other we have to clone several hundred Jack Caldwells, and it's not going to happen.

VDT: There are over half a million villages in India alone.

RYDER: That's true. No, my thoughts go in somewhat different directions. Without attempting to denigrate the line which he is so successful at and certainly deserves credit for, I think there is plenty of room for alternative approaches as well.

One of my simplest thoughts is that the World Fertility Surveys have managed to generate some very interesting questions at the macro level. We have a lot of indices now of demographic behavior for society, and what we need to do is combine those indices with a lot of other kinds of information about society. At the moment we do very little more with the analysis of societies than to give them a name, a geographic label, or perhaps carrying our analysis only one very modest step forward--we might call them Muslim populations, or something like that. Well, it's quite clear that societies differ in important respects and that there's a lot that demography can accomplish from this macro-societal approach. Almost none of it has been done.

VDT: It seems to me that some of it has been done--the anthropologists.

RYDER: The anthropologists don't like to use numbers.

VDT: That's true--well, not other than sample sizes of 35 or something.

A bit more on your work. Besides pointing out the superiority of the cohort over the period approach to fertility, you have in the past said that the longitudinal survey is rather more important than cross-sectional surveys. You've already said you criticize surveys in general, but do you still feel that after your work with the 1975 National Fertility Study, which was longitudinal?

RYDER: I'm sorry but it's quite to the contrary. Our 1975 study was a follow-up of a large group of women whom we interviewed in 1970. After the conclusion of our analysis of the 1975 data, by that time the National Center for Health Statistics had in place the successor to our survey [National Survey of Family Growth, first conducted in 1973]. So there was no point in our engaging in further cross-sectional survey work; it was being done already. But the people at NICHD were very interested in our pursuing the longitudinal direction in 1980. We debated the matter, Charlie and I, over a considerable period and decided that the yield from our longitudinal study in 1975 was insufficient to justify spending some million dollars of the taxpayer's money on another round. I guess that puts us in a rather deviant position within the fraternity. I am quite confident that we could have funded that and we chose not to; we chose not to ask. It was on intellectual grounds.

I published some of the reasons for our deciding this in an article--I like the title; it's called "Where do babies come from?" That was published in a collection presented to the American Sociological Association. It ended up in a book edited by Tad [Hubert M.] Blalock [Sociological Theory and Research, 1981]. In that article I attempted to explain that there is considerable flaw in our thinking about longitudinal studies and that large payoffs can come from other directions of work. One of my main concerns is that what is longitudinal about a longitudinal study is that you follow the same individuals. But if, as is the case with me, you don't think of the individual as the center of the social universe, then that's not the kind of longitudinal work that's interesting.

VDT: That's plausible. I hadn't known about the possible 1980 study.

Another thing I wanted to ask. In your book, The Contraceptive Revolution [1977], on page 349--I don't know whether you or Charlie wrote this; this has come up again in articles of yours I've read--you wrote: "Demographers generally eschew theory . . . and in style of analysis, we bypass sophisticated multivariate techniques in favor of pedestrian cross-tabulations." I don't think that's true because yours are not very pedestrian. What do you think of that? It certainly doesn't seem so from papers given at PAA; they use very sophisticated statistical techniques.

RYDER: Well, yes and no. If you look at the product that Westoff and I turned out over the course of the three National Fertility Studies, you will find virtually no attention to questions which for many people bulk very large. For instance, the question concerning the role of multicollinearity; we scarcely touched that question at all in any of our work. We have also avoided the construction of elaborate indices. You will see no use of scaling techniques or factor analysis, for example. We are working at a genuinely primitive level in the sense of statistical social analysis.

On the other hand, we have attempted to behave responsibly with respect to the measurement of demographic variables. These are questions that you do not go to a statistics book to get answers for, because the demographic variables have peculiar characteristics which call for really the building-up of a new little branch of applied statistics. That's been happening not only with us but with a number of others. We have tried to do our work on fertility surveys with a high respect for the sophistication needed to measure the dependent variable, as it were, so that in terms of social analysis, we have purposely stayed at a primitive level. But in terms of the refinement of the dependent variable, I think that we have perhaps helped to push the field ahead.

VDT: You certainly have. That explains a lot.

Another question. You write so elegantly; did you have special training as a child?

RYDER: Yes, my special training was that I had a Canadian education and a Canadian education was something very, very special. I was raised in a household of people who migrated from England and the word was something that I was taught to cherish, and I cherish it still. I think it's no gift; it requires a large amount of work. But when you get it right, it's the kind of satisfaction you can't get any other way.

VDT: That also explains a lot. Many demographers write well--or perhaps have good editors, as I've found. But yours is really elegant writing.

BREAK

While we were turning the tape to the other side, Norman admitted that his office is the last holdout for smoking in this building; he's got a lovely pipe. When did the Office of Population

Research become smoke-free? It's certainly become more and more of a campaign--smoke-free in all places of work.

RYDER: It became instituted more as a practice of making smokers uncomfortable than by law, but I think it was a year ago that it became institutionalized.

VDT: I have a question here about the influence of Wisconsin and of the OPR on the field. You have spoken with justifiable pride of Wisconsin; you began that center. What do you feel has been the influence of Wisconsin? You mentioned that many people are working on ecological, more sociological, questions than is the case at OPR. You hinted that here at OPR demography never became too well integrated with the sociology department.

RYDER: The histories are quite different. At Princeton demography began with Frank Notestein's arrival here in the mid-1930s and he was by training an economist. There was no sociology department.

VDT: His Ph.D. was actually in social statistics, according to your own excellent article on him ["Frank Wallace Notestein, 1902-1983," Population Studies, March 1984].

RYDER: The names didn't mean exactly the same at Cornell in 1927 as we think of them now. For instance, Walter Willcox, who taught Frank statistics, taught it with a large emphasis on moral philosophy, which is something you will not find in a statistics course these days. But at Princeton the demography group, particularly stimulated by the League of Nations studies of the populations of Europe and the Soviet Union and by a number of associated monographs, attracted several first-rate sociologists because they were demographers--Kingsley Davis and Wilbert Moore, for instance. And Paul Hatt came here and wrote his book on the population of Puerto Rico. Paul Hatt, who has been dead for many years, was an example of a well-known young sociologist who acquired demographic talents and used them in conducting an important early study of a developing country. Sociology at Princeton, therefore, grew as a consequence of sociologists who had come here primarily to do demographic work.

At Wisconsin, on the other hand, the tradition of sociology goes back a hundred years. It was one of the very early, very strong centers of American sociology and it had always considered that demography was something they ought to have. In fact when I was hired at Wisconsin, I was hired in part because of the death of Tom McCormick, one of the early American demographer-sociologists.

So the traditions of the two places were quite different, and they have remained different.

In terms of my own work, I have tried very hard to make bridges between sociology and demography. I have tried to teach sociologists what virtues there are in having the kind of backbone that demography can provide you with, particularly with respect to the conceptualization of problems, the construction of appropriate models for social process. Likewise with demographers, I've tried to prevent them from lapsing into merely actuarial and statistical exercises.

The job of social science is very, very difficult. Many of our accomplishments in demography are not just because we are just social scientists; it's because we are good at actuarial work, chartered accountancy, macro-biology, if you will. But once our questions verge on classic social science questions, we have no more potency than most of the social scientists.

And in trying to forge that link, I think that there was much more success at Wisconsin, in part because of the ecology tradition and in part because the size of the department at Wisconsin enabled us to have a comprehensive coverage of literally the whole of sociology. Whereas Princeton has always had a small-scale sociology department and many of the people have particular interests, at which they

are very good, but the whole does not seem to hold together as a system of sociological training.

Now, having made this pitch for organized instruction in sociology as an important part of a demographer's training, I must confess to a considerable amount of ambivalence, because I also have the feeling that good research has not depended to a considerable extent on a person's dedication to one discipline. I think the population office here at Princeton has been a very successful one and I can assure you that months go by in which nobody identifies him or herself either as a sociologist or as an economist or as any other kind of specialist. We are here because we enjoy doing research and we do not allow disciplinary barriers to get in the way of doing good research.

VDT: Do you think demography in itself is a field--that those who have been really important in it, built it up, have been those who have no barriers between the disciplines?

RYDER: I think that we have a strong research tradition and the way it manifests itself is that if a person or persons doing a piece of work need to know something substantively in a particular area, they take the responsibility of learning that for themselves. I think the best interdisciplinary work goes on inside one person's head. I don't think you can train a person to be a comprehensive scholar; it's a much too demanding exercise. But with good research questions, if you follow where those questions lead you, you find yourself acquiring what you may need--in biology, in history, and the like--just because you are going to keep on after that question to find an answer to it.

VDT: It seems to me that many demographic research questions now, as in any field, are getting narrower and smaller; the big questions have been done. Do you think that is affecting demography too? You're saying that a good research question demands widespread approaches. Demography seems to be perhaps one little box that draws on a lot of disciplines around it. Perhaps what I'm asking is, Are there still big research questions in demography to be answered?

RYDER: One of the natural tendencies of a person who is interested in a field which is not only intellectually attractive but also has extraordinary social elements to it--it is immediately evident that our work means something to major questions that people of the world are concerned with. One of the natural things to do in an area like that it seems to me is to focus on those variables, such as population size and the level of fertility, and place them somehow or other in the center of the conceptual network that you're working with. I think that's probably been a mistake. I don't think these are the proper places around which to construct theoretical systems.

I think that if we are to advance further within fertility, for example, we're going to have to know a great deal more about the societal context out of which fertility comes and of which fertility is only a manifestation--extremely important for the development of population size, but not therefore belonging right in the center of your theoretical structure. I think it's far more important for us to be students of the family than students of fertility, as an example. And I think the same thing goes for the major questions with respect to the good or harm that is done by a particular rate of population growth. That question seems to me to be part of a much larger package of economic questions in which, I'm sure, population has a very large role to play, but one of a number of players rather than as the center of the universe.

We've done our job now and made the place of our variable stand out in the minds of others. But if we're going to advance further, I think what we're going to have to do is to enlarge our models beyond the narrowly demographic framework and tackle some questions that are much more difficult but in the long run much more rewarding.

VDT: People of the National Academy of Sciences study Population Growth and Economic

Development: Policy Questions, 1986], under Sam Preston, who is an OPR graduate, felt they were doing that when they looked at population growth and economic development and came out with answers that not everybody was happy with.

RYDER: That's exactly where I look for the future of the field. The people about whose work I am most excited are people who are well-trained demographers and at the same time have something strong going for them of a substantive sort. There are some questions that can be dealt with which are essentially little mathematical games. I love playing them myself and so do others; some of them have turned out to have rather interesting consequences. But I think the future of the field belongs to people who are prepared to be in a sense both, both a sound technical demographer and a person with genuine substantive insight. Certainly Sam Preston and Jack Caldwell fall in that category. And I think the future belongs to people of that sort.

VDT: Interesting observation; very good.

Tell me about some other people who have been influences in your career. You mentioned your first Canadian professor, who obviously was of great importance; he got you here. Who did you work with when you came to OPR?

RYDER: I'd like to be able to say that I'm an intellectual heir of Frank Notestein, but that wouldn't in fact be true. I think the most important training experiences I got here were of a quite different kind. One of them certainly was the extraordinary quality of the small number of graduate students who were here at the same time as me [1946-51]. They included people like George Barclay, George Stolnitz, Robert Osborn, George Mair, Harvey Leibenstein, and a few others. We were very closely knit. We were thoroughly dedicated to demography. You could come here Sunday night at 11 o'clock and you would see lights blazing in several of the office windows. We had a high sense of camaraderie and I think we had a kind of cockiness to us because we felt that we were onto something pretty exciting. I would say to the extent that I have some influences, the influences came more from the extraordinary collection of people that happened to be assembled here at that particular time.

Now there was a second line of influence and that was that I came here as an economist and went away as a sociologist. And the reason for that was the influence of just a few people and those were, in particular, Kingsley Davis, Wilbert Moore who died last year, Marion Levy who is still here, and Paul Hatt who died 25 or 30 years ago.

In particular, I found the work of Kingsley Davis to be exceptionally stimulating. In fact, I don't think I have ever read anything of Kingsley Davis's that I haven't felt that I learned something from. He was a most remarkable intellectual influence on me. It wasn't that our time together was lengthy and we certainly haven't worked together since, but if you think that I can write well, you should read Kingsley Davis.

VDT: Yes, indeed he does. And he has shown remarkable change through all his decades of productivity--coming out decade after decade with new ideas and fresh expositions.

RYDER: I didn't want to say anything negative about Frank, on the other hand. I found him to be an extraordinary example of the importance of what you might call a "research entrepreneur." Another such example would be a man like Phil Hauser. These people seem to me to have been of extraordinary importance, extraordinary as organization creators, extraordinary as the people who managed to inspire research funds to come in our direction, people who played a major role in shaping the field just by their choice of individuals, choice of students, choices of fellow faculty members.

I had no perception. I thought somehow or other in a naive way that advances in the field came

out of the head of somebody as he or she was sitting thinking hard. And it's not that at all. There's an apparatus; there's a world system. And Notestein was master in those areas. So I have unbounded respect for him, in part because he had talent that I had no way of emulating at all.

The other way round, I was unable to convince Frank of the worth of doing the research that I was doing. In the end, he sort of just let me do it; that was my doctoral dissertation. I wrote it for him, but he didn't know why I wanted to do it, what I wanted to do.

VDT: Cohort fertility?

RYDER: That didn't catch fire with Frank, no.

VDT: I didn't realize you went to work with Scripps some time after this. Everybody credits P.K. Whelpton with cohort analysis.

RYDER: I had four years in Canada. I was doing my cohort research from 1948 up through 1954, and it was because Whelpton knew that I had been doing the same kind of work that he was doing that he asked me to come down and, among other things, help him revise his cohort tables, because they were in pretty bad shape. We got the tables back into shape and then when I left Scripps, Art Campbell came and finished up the job. Those became the basis of the Heuser tables that are now published every year [in the Natality volumes of the National Center for Health Statistics].

VDT: Which you have drawn on for your good history of American fertility. Is that another case of two independent researchers onto the same thing; like you and Louis Henry both onto the parity progression ratio, you and P.K. Whelpton were independently onto cohort fertility?

RYDER: Well, if I had to look for a person who had that characteristic more than anyone else, it was John Hajnal. Hajnal was a brilliant young man who became associated with the British Royal Commission on Population [in the 1940s]. He had had none of the ordinary kind of training one would expect in the United States to produce a demographer in some sense or other. Yet in a matter of it seemed almost months or a couple of years, he contributed mightily to the basic methodology, particularly of the cohort orientation. You can find all this in his Royal Commission paper ["Births, Marriages, and Reproductivity: England and Wales, 1938-47," Papers of the Royal Commission on Population, II, 1950]. I found it very frustrating to get an idea and then glance through some of Hajnal's stuff and find that he had exactly the same idea, but expressed with much more elegance.

VDT: Impossible.

What brought you back to OPR [in 1971]--just straightforward to work more closely with Charles Westoff on the National Fertility Study?

RYDER: No, that wasn't it; it was something quite otherwise. While I was at Wisconsin, I became very much involved with the activities associated with the student uprising in connection with the Vietnam War. For a number of years, I must have spent three quarters of my time in involvement on the campus with organization of one kind or another. My best friend at Wisconsin was Bill Sewell, the sociologist. He became the chancellor at Wisconsin and during that time, I spent a lot of my year working in a sense on his behalf but also on behalf of the student movement, because I was one of a small number of professors whom the students continued to trust.

Events at Wisconsin took a rather familiar turn. There was a xenophobic reaction among the populace. The Board of Regents and the Governor were in the wings attempting in a heavy-handed

way to make things better and in consequence making things worse. They were sending in troops with fixed bayonets and tear gas and the like; they were assembling bully-boys from the county cops. And the confrontation on campus was most unpleasant--so incongruous in an academic setting. That would be the subject of another couple of tapes.

What happened was that I became convinced by a series of actions that the higher-ups were taking at Wisconsin that they were changing the character of the university in a most negative way, until the place became a place at which I no longer wanted to work. I had been treated very well at Wisconsin and I had a deep affection for my colleagues there and have regretted ever since leaving there. On the other hand, I found that I could not, on principle, remain an employee of that university. So I gave back my chair; I resigned. I phoned Charlie Westoff and said, "Do you happen to have a job for a middle-age demographer?" And that's exactly why I came to Princeton.

VDT: I didn't know that story. Thank you for telling it.

Coming to Princeton you worked with Westoff, as you had done already. How had you been operating? Sending the data back and forth from the fertility study?

RYDER: It was remarkably straightforward. I learned to write a request for tabulations that I could communicate by mail with any programmer here at Princeton, so that all of my tabulation requests were fulfilled here and stuck in the mail in those giant envelopes and sent out to Wisconsin.

Charlie and I did have a substantial division of labor. We did not work closely on piece after piece. He would do some; I would do others. And each would serve as sort of a critical editor for the work of the other person. So it was not necessary for us to have continuing conferences. I had a chunk of work and I just proceeded to do it. So it was not at all difficult from a physical standpoint to carry on research at two places. That would have been no justification for me coming to Princeton; there was no reason for that.

VDT: Tell me of some of your students both at Wisconsin and perhaps here whom you have been most pleased with--given you great satisfaction.

RYDER: That's a rather difficult question to answer because I have not had the kind of career that, say, Ansley Coale has had with respect to students. He has been most blessed, perhaps because of his characteristics, in the long list of students he has had.

VDT: Yes, he took the OPR alumni list this morning and ticked them off for me, and it went on page after page. He has had a lot.

RYDER: My list of students is a much smaller one and there are some that I'm very proud of. But I think that if I've had any substantial role as a teacher it's been that I try when I write my articles to write somewhat in a pedagogical style. And I feel very satisfied when I see that some of my things are on reading lists in different places around the world. But as a one-on-one teacher, I've had a rather small list of names.

VDT: Well, you certainly have had an influence. In the session at the PAA meeting just past in New Orleans [1988] on "Fertility Change and Its Effects on Family Structure" that you chaired--Jane Menken had organized it--the three readers I heard all said, "Now, as Professor Ryder has said . . .". They were all, I think, following your lead in doing some empirical study.

RYDER: Let me say something about that. I think part of that has to do not so much with any grand

theorizing, because I really think that in the rather primitive stage of social science as a discipline, what we really need are good questions. I don't think we're really terribly near good answers. Good questions, though, can provide you with a research agenda. They say, "Here is something we need to find something about, because it's interesting and it's important."

And what I have tried to do throughout my whole career is really to lay out research agendas for people. And it's turned out in several cases that I have in a sense been able to strike oil, because I fingered a problem early on and then others gradually perceived that, yes, it was an interesting problem.

For one thing, for example, I have been almost obsessed with the question of the time pattern of fertility. That was a subject on which practically nothing was known in the late 1940s when I got started as a graduate student. Bit by bit I kept hammering away at the theme; we've got to pay attention to the time dimension of the whole process because it's important. And, of course, the cohort approach has that as one of its running threads as well. I am delighted now that this has become a major industry, as it were, within the research field in demography.

Another example is the area of family demography. Now, I happen to be fortunate enough to get in on the ground floor so that I wrote several pieces, which were really prospectuses, saying, "Take a look at the gorgeous number of interesting questions that can be addressed within the framework of not individual demography, which we all know, but family demography, in which there are some prickly questions that are of major social importance."

So I think that if I have some kind of influence that a large part of it has been in flagging good questions. I certainly haven't provided the answers to them but I have, I think, managed to indicate the crucial role that an answer would play in the larger development of our body of knowledge.

It's interesting to me how fields proceed like that. Sometimes, of course, there are questions that come up on which a very large amount of money is expended and then in the end you realize it was a waste of time. Optimum population is a concept, for instance, on which a colossal amount of time has been wasted and I'm glad that it died and has been buried. But what could we have done with that talent had it been redirected a bit!

VDT: On family demography, did you ever work with Paul Glick?

RYDER: When I said that I was interested in the time dimension of fertility, there is a rich old literature in a staple part of sociology on the family life cycle, and particularly the rural family life cycle. One of the reasons was that the agricultural experimental stations in the United States all had their little rural sociology groups associated with them and it was a very active research tradition. Agriculture experiment stations have built-in research programs. Unlike the ordinary academic departments where you teach for a living, these people had a half-time research component, just like the other more practical aspects of ag experiment stations. So there was a large tradition of rural sociology, research-oriented, collecting life cycle data for farm families. This is a big literature, and it's a literature that goes back a long way in Europe as well. In the Soviet Union, for that matter, before it became the Soviet Union, there was research on farm family life cycles. Now, Paul Glick was quite clearly a sociologist from the beginning and had knowledge of this rich literature. Then, of course, he had the ability to convert it into some practical calculations, using really good data for national populations.

The whole life cycle literature has been an important one to me, in part, of course, because I have always insisted on the importance of carrying it along a cohort orientation rather than in cross-section, but partly also because it's connected with this whole time-pattern question, the time pattern of human behavior.

Paul and I have never worked together closely, but we must have been on many, many panels together because of the overlapping of our interests and the time dimension in human behavior,

meaning such things as your educational career, your occupational career, labor force history, marriage, employment history, veteran's history, and so forth. All of these threads are being woven together by some people working at the University of Wisconsin in that demography center. It's become itself a major growth industry within the sociology profession, but with very strong demographic roots.

VDT: Didn't Wisconsin have a good rural sociology tradition?

RYDER: Yes, it's one of the strengths of Wisconsin.

VDT: That explains in part what you just said about family sociology developing there.

RYDER: Yes. Well, as you probably know, a lot of social research in the United States came out of a tradition of social statistics without any particular other identification to it. People who were happy to be working with numbers--the numbers were predominantly numbers that were generated out of the census and other such sources. They were sometimes called sociologists, but they were clearly quantitatively oriented, and there wasn't a clear distinction between what was demography, what was sociology, and what was even statistics. They used to call it statistics, although nowadays, of course, statistics is the methodological apparatus rather than the numbers you're looking at.

VDT: What do you see as the important issues in demography over the years you've been involved? You never worried too much about the baby boom, did you? That has loomed large in U.S. demography. I mean that kind of thing.

RYDER: It's almost inevitable that one's thinking tends to be heavily impressed upon by the particular context in which you're studying. And in the immediate postwar days when I was studying, the preoccupation in the literature we were reading was low fertility--that was a large part of the reading list that Frank Notestein gave us--and it seemed to be a little out of date, to say the least.

I remember that early on I got concerned with an issue. How it came about was this. I was the Milbank Memorial Fund Fellow and my job was to read proofs on the Population Index. Louise Kiser was the editor here; Irene Taeuber was our editor based in Washington at the Library of Congress. Louise Kiser used to sit with me and for a while she would read and for a while I would read and we would actually read proofs on all of the papers of the Index. This required spelling out many words, because we were a multi-lingual index from the very beginning. So if you ran into a Hungarian name, you literally would spell out every letter in the name.

Louise went to Frank Notestein and said, "This is a lousy job to have a graduate student doing because he's not going to learn anything out of that; it's just scut work. Is there anything else that you could have him do?" Frank thought for a minute and said, "In the back of the Index we publish birth and death rates and things like that." Most of these came from routine sources and it was just a matter of copying them out. This was, of course, prior to the United Nations; the Population Division was in process of being created at that time. So the international sources were, for a while, the Population Index.

In the Index were published not only the routine calculations but also net reproduction rates, intrinsic rates of natural increase. These required a little calculation. Accordingly, Frank said, "Why doesn't Ryder do those calculations?" Now, I have a bit of a stubborn streak and I didn't want to do a lot of calculations unless I understood exactly what it was that I was calculating. So I started to investigate the literature on the net reproduction rate and found that there were a number of aspects in which it was defective. I think Frank was a bit wary of my radical notions with respect to the net reproduction rate, for which I had developed a substantial amount of contempt. He therefore paired me

with George Stolnitz and together we thought and put together a manuscript, which became the first publication for him and for me. It was called "Recent Discussion of the Net Reproduction Rate."

It had a huge bibliography and we attempted to sort out the main threads, all of them quite critical of what had become a sort of linchpin of demography. Stuart Chase in a book called The Proper Study of Mankind--one of these popular histories of the social sciences advertising what the social sciences were for the layman, a Book-of-the-Month-Club sort of thing--said when he got to the study of population that, "The strongest single contribution that social science has ever made is the net reproduction rate." And here we had come in with a damaging indictment of the net reproduction rate. It was published in Population Index [April 1949]. Right at the beginning we had a statement that goes something like this: "Until now the questions surrounding the future growth of the population were regarded as reasonably well understood. This is now a matter of some doubt."

VDT: Oh boy!

RYDER: At that time, Joe Davis of the Food Research Institute at Stanford was attacking demographers vigorously for being charlatans because all of their forecasts were going screwy; instead of going down, everything was going up. He seized upon this particular quotation of ours and said in a number of places, "Some doubt, indeed." We thought we were being daring as graduate students to say "some doubt," but he took it the other way round.

VDT: Right then you were seeing that population growth was speeding up in developing countries [and in the U.S.]?

RYDER: The net reproduction rate was just a misleading index, particularly because it was period-oriented. I had quite a bit of contact with Alfred J. Lotka at the time, because the index is obviously derivative of his larger stable population model. What bothered him was that people were confusing our criticism of the particular substantive application with the quality of his model. Now, his model was impeccable--and is. It's one of the great glories of scientific creation in this century. But the net reproduction rate as calculated was, and still is, badly misused. We'd be better off without it.

VDT: What would we have in its place?

RYDER: Perhaps a little thinking.

VDT: Come now, we have to have something--replacement level fertility. We've got to have that line across the chart so people can say you're above or below.

RYDER: I have been studying some recent statistics for the United States and the total fertility rate has been moving in a way that leads people to think that fertility has stopped declining and that our level is such and such and so and so. All of these are contaminated by the period orientation underlining the data. If you look at the same information from a cohort standpoint, you get a quite different story. It's just as true now as it was 40 years ago. Unfortunately, there isn't a button you can push and get one answer that answers all the questions.

VDT: You did show in your article in Population and Development Review of December 1986 that the last two birth cohorts that you looked at, which were 1955-59 and the one before that, had had cohort fertility below replacement, but a little higher than the actual period rate.

RYDER: So far they seem to be almost identical, these two adjacent cohorts. In other words,

whatever has been happening appears, at the moment at least, to have come to a halt. [See Ryder's later publication on U.S. fertility trends, "What Is Going to Happen to American Fertility?", Population and Development Review, September 1990].

VDT: I guess recent women aren't going to give up entirely having children.

The question was: What do you see as important issues in U.S. demography over the years you've been involved? You've just given a marvelous story of the net reproduction rate.

RYDER: We were talking about the phenomenon of the baby boom. My reaction to it was that this was a temporary divergence from a long-term trend. I always felt that demographers showed indecent haste in abandoning a long tradition of understanding of the forces underlying fertility decline just because they had a baby boom. Now, of course, in terms of practical consequences the baby boom was an enormous phenomenon. We will live with it for the rest of our lives, and our children's lives, for that matter. On the other hand, from a standpoint of more pure theory, what we really needed was a healthy dose of methodology. We had to start measuring what we were talking about instead of a poor substitute for it.

VDT: You certainly managed to do that.

What accomplishments have given you the most satisfaction? I think we've heard that--your cohort fertility, the emphasis on that.

RYDER: Let me tell you about an accomplishment that I think was a very big mark that I was able to make and I was able to make it quite fortuitously. For three years, I served as editor of the official journal in sociology, the American Sociological Review. During those years, one thing that struck me was that sociology had, in my judgment, been suffering under the burden of an extremely old-fashioned method of bibliography and footnoting. So I got together with the printer of the journal, Henry Quellmalz, and said, "Why don't we revise this and bring it more in line with scientific practice?"

So I instituted a completely new system of footnoting and bibliography citations, the one in which you give the last name of the author and the year of publication in parentheses and then at the back of the article, you read the list--names, titles, years--all the way down. That became the style not only for American Sociological Review but for all sociology journals. It became the style for all sociological dissertations, for Demography, and for journals all over the world. And this is a contribution, because I've changed the way secretaries behave all over the world!

INTERRUPTION

VDT: We've just had a visitor, which had to do with the turmoil that OPR is going through because a number of staff appointments have to be made and some people are retiring. Norman says he is technically going to retire next year.

RYDER: What I wanted to say is that retirement technically means that I will no longer be a professor and therefore I will no longer be giving a course or a seminar and will no longer be going to department meetings. Those are activities that I can live without.

As far as demography is concerned, on the other hand--it's a little hard to convey this and it may sound silly, but when I got into demography I didn't do it because I had any yearning to save the world, I got into it for very selfish reasons. It looked to me as if it had interesting problems. They are problems that I have enjoyed working on in exactly the same way that people will enjoy working on a

crossword puzzle or something like that. And it has always been an extraordinary aspect of these games that I enjoyed playing that I am allowed to continue to play them, someone pays me a salary, and occasionally it makes a difference to people with respect to genuine social problems around the world. These things are fringe benefits of something that I would do even if nobody were paying me at all. So, as to what I am going to do in my retirement, I'm going to continue playing my games.

VDT: Wonderful! What a wonderful career you've had. Not many people have that opportunity.

The question was, what have been some of the accomplishments that have given you the most satisfaction. You've just told of a very practical one which I'm interested to know about, the method of footnoting in sociological and other journals.

RYDER: I told you before about identifying problems that I thought were worth working on, advertising them to the profession, and then finding that they became attractive to young people and then became larger activities--this is something I got a very great deal of satisfaction out of.

VDT: You've mentioned the net reproduction rate, family demography . . .

RYDER: And the whole problem of dealing with the time dimension in demography. And dealing with the time dimension means not only adopting a cohort rather than a period orientation, it also means making a serious effort to cope with the very difficult problems of handling the succession of events that are important in people's lives.

These events, of course, are certainly many of them demographic, but many of them are somewhat more subtle than demographers can handle very well, such as the passage of the child from the family of orientation, the family of procreation, and other such subjects--movements in and out of the labor force, transformations of households by the passage of people and the like. This area in sociology, which was quite moribund only a few years ago, is now a rich field for exploration and many people are involved at the most highly technical level and history analysis but also at what I consider to be a very important task of any social scientists and that is learning how to describe the behavior of people. It sounds pedestrian but I think that our discipline has suffered--our discipline meaning sociology--because we tried to fly before we learned how to crawl. Every science, in my judgment, has begun by a very careful attention to the details of description of what it was they were interested in. And we are only now, I think, getting around to actually describing what people's lives are like.

VDT: Are you going to write all this up, going to write your autobiography or history? You mentioned earlier that Americans do not have a good tradition of looking at the history of demography; you said the French have done it.

RYDER: I have a very close friend for whom I have a most profound admiration. He was once also president of the Population Association [1968-69]. His name is Dudley Duncan. He, unlike many demographers, played a major role within sociology as well. He has recently retired and he wrote a book about two years ago called, Notes on Social Measurement [1984], which was a collection of essays on the history of efforts to make measurements, with particular attention to social questions, such as how do you vote and such pedestrian kinds of questions as that. I found myself thoroughly bemused by the questions he was raising, particularly with regard to how one can go about making measurements in the social and psychological spheres.

It reminded me that when I was a graduate student I must have wasted a very large part of my time reading the history of early fumbling attempts at creating demography. That is something that I

had to wean myself away from at that time because it was much too all-embracing a subject for anybody who had to write a dissertation. But I think that one of the luxuries one has in the emeritus status is to expand a little in that direction. I wouldn't be at all surprised if I spent a lot of my time reading old demographers and trying to figure out just exactly what they were trying to say.

VDT: And going on and writing something from that, I hope.

RYDER: Well, I am a fairly compulsive writer.

VDT: You wrote an excellent tribute, story, on Frank Notestein, which appeared in Population Studies in 1984, the year after his death, that was a wonderful history of the field, like Notestein's own article ["Demography in the United States: A Partial Account of the Development of the Field"] that appeared in Population and Development Review in December 1982, just before his death.

RYDER: One thing that has made me feel especially fortunate to be a demographer is that I was able to meet many of the major players in the beginning of modern demography as a student and as a young person. Not only Frank Notestein but also people like Frank Lorimer and Irene Taeuber. The people who were the movers and shakers in the field were friends in the sense that I was part of the Association and the Association was a small roomfull of people.

VDT: Do you remember your first PAA meeting? It obviously must have been at Princeton.

RYDER: It was at Princeton; it was in the spring of 1948.

VDT: Actually, 1948 was in Philadelphia.

RYDER: Yes, that's true. I was a graduate student at Princeton and we did go down to Philadelphia. I think that was probably where I first met Westoff, who was a graduate student under Dorothy Thomas.

VDT: He said his first meeting was 1949, at Princeton; he's not missed a single meeting since. So you remember going to Philadelphia, that was the year before. Anyway, one of those was your first one.

RYDER: You understand that we here at Princeton were the graduate students in demography; there were no others.

VDT: Okay, that's the way to put it.

RYDER: What I was going to say was that talking with Frank, I became aware that so much of what we think of as the structure of modern demography has been created in a relatively recent past so that although it's certainly well before my time, it was not before Frank's time. In fact, I think one outstanding characteristic of Frank Notestein's life was that he arrived on the scene and it was at this critical juncture in his career and in the career of demography as it was being created. There was a meshing.

I have a great feeling for the people who have made up our history. It was a very rich one and one that we can be extremely proud of, I think. The number of names who were making it possible was a finite list, a small number of people.

VDT: Indeed, it was. It's almost encompassed by those who have been president of PAA. That loses

a few, like Raymond Pearl, who was never president because he died rather young.

I'm interested that you know Dudley Duncan; I hope to interview him next year [see interview of May 3, 1989, above]. I know he's in Santa Barbara. Do you still have contacts with him?

RYDER: Oh yes, I had a long letter from Dudley just a week ago. You should certainly interview Dudley. He is a man of extraordinary gift and is truly generous.

VDT: How did you become friends?

RYDER: He was a professor at Chicago and my contacts with Chicago when I was at Wisconsin were very close. Phil Hauser has been a friend of mine for my whole life and also Donald Bogue. Bogue and I were colleagues at Scripps Foundation when I was working for Whelpton and then Bogue left to go to the University of Chicago. I'm sure you've talked with Don Bogue.

VDT: I hope to talk to both Phil Hauser and Don Bogue [see interviews above].

RYDER: Dudley Duncan was working at Chicago and, in fact, became a very potent force in demography during that time, not least because of his contribution to the volume, The Study of Population [coeditor with Philip Hauser, 1959], which as you probably know was something like a brief to the National Science Foundation to establish the fact that we were entitled to science funds because we were a legitimate science. The project was called "Demography as a Science." The National Science Foundation during the 1950s was beginning to get strong pressure to fund something other than the physical and natural sciences. They were certainly not prepared to fund sociology across the board. Demographers, on the other hand, had something going for them by the way of mathematical underpinning and concern for empirical testing and the like that made them a sort of attractive entering wedge within the National Science Foundation for the whole of the social sciences. We prepared the brief, Demography as a Science, to the NSF, saying: "We belong. We belong to the system because we are a science." And each assignment in effect was to say where do we stand with respect to such-and-such a subject and end up with in a sense a statement of a research agenda. That was the format under which we were working.

VDT: How did Duncan and Hauser find you?

RYDER: We knew each other already. You must remember that the profession was very small; everybody knew everybody else by first name. At the meetings we had it was possible for you to say hello to everybody; no physical problems involved at that time. So we knew each other, because we were in the same business. There was nothing extraordinary about it.

Duncan trained a considerable number of people at the University of Chicago who went on to greater things. He then moved to Michigan and had an extraordinary career as a teacher at Michigan and then chose to move to Arizona. It was only in the last couple of years that he went to Santa Barbara. His wife, Beverly Duncan, was also a very important person, both in terms of research and of training. She was ailing and I think that was probably one of the reasons for their move from Michigan to Arizona. Beverly died this past winter and it was a great loss to all of us.

VDT: And Phil Hauser, you say, you've known all your life?

RYDER: Phil too was one of those people who had the talent Notestein had for being a research entrepreneur. Phil's career, of course, is a very long one. He was a "wunderkind" in the late 1930s.

He was associated particularly with Henry A. Wallace when Wallace was Secretary of Commerce. Phil played therefore a very important role for a long time as a Census Bureau official, before he had this very important career at the University of Chicago, where he became an intellectual leader for the demography group at Chicago which after Princeton was the next, I think, major training center that was created in the country.

Hauser's work was in part his own personal creativity and industry but it was in part also his talent, like Frank Notestein, for finding very able people and making it possible for them to do what they could do. In that sense, I think that Dudley Duncan is something that Phil Hauser should feel very proud of, because he provided a suitable niche for Duncan to do his work when he was at Chicago.

VDT: Was Duncan a student of Hauser?

RYDER: I don't think that Duncan studied under Hauser, but he was a staff member within the organization that Hauser established for population research and training.

Hauser has also played a very important role as--if you will--an ambassador to the outside world. He, unlike most demographers, is able to be at home and talk convincingly with the outside world, with the world of business and industry and commerce, and talk on major issues with some technical facility in a way that doesn't bore them silly. People like that are very important to our profession. Most of us find that role very uncomfortable and don't do it very well. Yet somehow or other the health of the profession depends on interchange between these different worlds.

VDT: Indeed, it does. You pointed out earlier that because demography deals with issues that are of social importance it's inevitable that demographers will be wanted as spokespeople on these issues.

RYDER: That's one of the nice things about being a demographer. I don't know what people do when they have a specialized branch of biology or chemistry or whatever that they happen to do their professing in. But a demographer, on the other hand, when we talk shop everybody at a cocktail party enjoys the talk.

VDT: That's true. My sons used to say, "Oh, Mom, don't get onto population again; come off it," but they found it interesting. None of them went into it [but a Canadian niece is now, 1990-91, enrolled in the graduate demography program at Wisconsin]. Incidentally, Robert Hauser is a nephew of Phil Hauser, isn't he?

RYDER: Yes. The day before yesterday I was entertained at Bob Hauser's house in Madison.

VDT: It's nice to see a dynasty within the profession. It was a son of David Brinkley who used the expression "demographic dynasty" first in my hearing about the Taeubers.

RYDER: There were two Taeubers at the affair on Monday night--Karl and Alma.

VDT: Did you work with them?

RYDER: No, I didn't have a great deal to do with them except for helping to hire them. We wanted them and found ways to make it possible for them to come to Wisconsin.

The Taeuber that I knew best was Irene, because she was on the OPR staff, though based in Washington, and would come to Princeton with a fund of inside Washington stories. She had a style of communicating them that was almost conspiratorial, as if she was letting us in on something that we

should not breathe a word of to anybody else. It was all very exotic for a young kid from Canada to be able to listen to this.

VDT: Did she tell stories about people in high places in Washington?

RYDER: She had a fund of information.

VDT: Perhaps seeping through the walls at the Library of Congress. That's interesting; I haven't heard anyone say that about Irene. She must have been fun to work with, besides being such a top scholar.

RYDER: She was an extraordinarily competent person, but a very warm person.

I've never lacked for good friends in the profession. Frank Lorimer would be worth at least an hour on tape all by himself.

VDT: Tell me something about him. He's a person I most regret not having been able to interview.

RYDER: Let me tell you just one little story, because I happened to be on hand and heard it from him. It was after Faith Williams had died; Faith was his wife for many, many years. She had an outstanding career in the government as an economist. Frank had lost Faith and he was, I would think, in his late sixties and we were at the Princeton Inn, which at that time was still a hostelry, it's now a residence, having breakfast on a Sunday. I found it difficult to have a conversation with a man who had just lost his wife of so many years and it was a good marriage. But I said, "What do you think you might do now?" He said, "Well, I've always wanted to go to Africa, so I think I'm going to Africa." And I thought, isn't that great for a person who as far as I was concerned was an old man, not much older than I am now, but that he should just be able to start a new chapter in his life.

Well, while he was in Africa he was in a bar in Nairobi and he picked up a nurse sitting at an adjoining table. She was a nurse from New Zealand, and he married her. It was not long thereafter that Frank came to the Population Association meeting, which was being held at Pennsylvania that year [1963], and I remember our meeting at a big cocktail party in the museum. Frank Lorimer was strutting across the floor like a turkey cock--his wife was pregnant!

I kept in close contact with Frank, as close as I could, because he was so many different people at the same time. His career included being a minister, a psychologist, a research assistant to Fred Osborn, whom he managed to convert away from the rather racist eugenics of Osborn's uncle and really reformed Fred, turned him into an entirely different person. He was also an anthropologist; he was so many different things. He was a very good friend to me and when I was young. I got into the International Population Union [IUSSP] when I was 27 years old [1951].

VDT: You must have been one of the youngest ever to be elected.

RYDER: Yes, I think John Hajnal got in at about 26 or 27. Yes, it was quite young. It wasn't because of extraordinary merit. It was because Frank Lorimer was the General Secretary of the Union [Administrative Director, 1949-57] and he decided that the Union was a little bit narrow and stuffy and they needed a bit of young blood and they needed to enlarge their province. There were very few Canadians. The Dominion Statistician was a member, but that was about it. Had I been an American graduate student, American resident, I wouldn't have stood a chance of getting in for about 15 or 20 years. There were all sorts of eminent people not yet in the Union. I got in because I was a Canadian.

VDT: Oh, come off it! But I got in too, perhaps, as a Canadian. They always put proudly your country of birth. The Canadians are a very strong cell within IUSSP now. I remember you at the

Florence meeting in 1985 at that meeting of Canadians when they were talking about wanting to have a national committee again, which was the way the IUSSP was constituted before World War II, which made so much trouble because they became politicized. You got up and gave those Canadians a talking to.

That brings me to a question I've been dying to ask. As a Canadian I know how much you are lionized by the Canadian demographic community, which is still small. You went up there for a sabbatical year--about two years ago?

RYDER: Four.

VDT: And they look upon you--you and Nathan Keyfitz--as their proudest products. Prodigal sons that left, but you came back as lions. Tell me something about that.

RYDER: I did intend to fulfill the sacred obligation of being a Canadian demographer, but I found that I loved research more and it was not possible in Canada--nor is it easy to this day--to do research.

VDT: What about the University of Western Ontario? It's getting to be more of a center.

RYDER: The University of Western Ontario is a reasonably well established population center, as are several others: Montreal has a very strong group, and so on. But I'm not really speaking of the quality of the people so much as the niggardly funds that they have to work with. There are no supporting funds for the center at Western, for example. They don't have any money. If they wanted to buy an annual vital statistics report there would be no funds to tap to do that, for example. They don't have any money to buy a calculator or something like that. The research grants that they can get from Canada Council don't seem to come with overhead on them, which is the staple of American research financing. I think that they are in very hard-pressed shape. Furthermore, since the Mulroney regime came into power in Canada, a Reagan-like attitude toward the financing of research has prevailed. In particular, the budget of Statistics Canada has been cut unmercifully.

I have been serving the last four years on a demographic advisory council to Ivan Fellegi, the Chief Statistician. We have been attempting to help the development of their demographic and statistical programs under the greatest difficulty because they literally have inadequate staff even to perform the most routine activities, let alone to improve their product. It's been most distressing to me to see this. I have been working this past year--in fact, I spent 15 months--on a small contract with Statistics Canada to develop a new method of forecasting fertility and working closely with Anatole Romaniuc at Ottawa. I hope that will continue to be a lively activity of mine for many years.

I don't know whether you know or not, but I own a piece of Canada. I have a house on Lake Simcoe in Ontario and I spend four months a year there. It's quite handy to Ottawa and Toronto and the like. I would like very much to, in a sense, pay my dues.

VDT: That's good to hear. Some Canadians kind of resent those who went south of the border and didn't come back. Is your wife Canadian?

RYDER: Yes.

VDT: You've had a long and happy marriage, I know.

RYDER: Forty-one years, as of last week.

VDT: Did I see her name listed in the preface to The Demography of Tropical Africa [1968] as being one of the ones who did some of the charts?

RYDER: No, I think you've got the name wrong. I think Westoff's wife . . . and Daphne Notestein was for a long time a chartmaker for Princeton books. She had an important role. If you look, for instance, at the book on The Future Population of Europe and the Soviet Union [1944] and the other books that came out at that time, you will find that those are beautiful charts. Those are Daphne Notestein's charts. Daphne is still alive; she's 91 years old. She only just this past winter stopped driving her car, not on the grounds that she was incapable of driving a car but if anything should go wrong, they would blame it on her age and she didn't want them to have the opportunity.

VDT: About PAA, you've already intimated how PAA has changed over the years. You have said that when it was small, everybody knew everybody. The changes now--what do you think of having 87 overlapping sessions, as we just did in New Orleans?

RYDER: I want very much not to sound like an old fogey, but I regret very much the loss of what we once had. I do not find a large hotel to be an intellectually exciting environment. Particularly a large hotel like the one in New Orleans which didn't provide anybody a place to sit and talk with anybody else. The multiplicity of sessions, I suppose, is an acceptable facade for assuring that lots of people get their expenses paid.

VDT: Get their papers accepted, so their expenses are paid.

RYDER: Yes. But, maybe it's a personal failing, if a paper is worthwhile I want to be able to study it, meaning to read and reread. I can't pick up a decent paper through the ear at all. I don't find the meetings to be satisfactory for paper presentation. If a paper's good I will read it, but I won't go and listen to it.

It is possible to have meetings, I think, at which people who are known to be opposed on particular salient issues are actually engaged in confrontation. And there have been occasional meetings when, sometimes by design but mostly by accident, it happened to fly. It's very much like live TV as against canned TV. It would be nice if some of our meetings could be a little more structured toward nailing down issues on which people are apart and ought not to be.

VDT: Some interviewees in this series have said that in the past you could hear Kingsley Davis, with his drawl, out after Frank Lorimer, always confronting each other. They have tried to do that in recent years--this year's [1988] "Authors meet critics," the Michael Teitelbaum session.

RYDER: Yes. Another problem, of course, is that as the profession has increased in size, it has also increased in specialization. It's the nature of our modern bureaucratic world. That means that many sessions are quite uninteresting to many other people. Furthermore, the only way you can make a contribution now is to chisel away at one little corner of the enterprise and the refinements are rather boring in oral form.

VDT: Well, maybe you have yourself to blame. You said you set up the questions and now you have some of your followers, students, chipping away at the corners.

I had a question on what you see as the outlook for demography, and you have perhaps answered that. Is there still room for the great overarching theory questions, which you said you have attempted to set up during your career? And you've paved the way for people to chip away at the little

questions. Is it too late for young demographers other than to chip away at the smaller questions?

RYDER: I have not gotten that sense yet of closure in any sense. There are many large stumbling blocks that we have in front of us and it's hard for me to see how they might be resolved. I think that we have pretty well gone to the limit with straightforward survey interview techniques, for example. I don't think we are going to make a substantial advance merely by refining questions. I think we face the incredible difficulty that if the subjects are genuinely important to people about whom we are inquiring, we can expect dissimulation and non-response and lying and bending the truth and so forth, just because it's important. And of course, as you know, non-cooperation has been rising throughout the whole survey world. I don't see much of a future in a host of bigger and better surveys.

VDT: You're not suggesting that the National Survey of Family Growth should be discontinued; it should go on collecting the data?

RYDER: Oh, no. We need avenues for collecting basic behavioral data. I think questions attempting to get at why people do what they do are essentially beside the point, that's my feeling. It's a good idea for us to monitor this very important kind of behavior and do it as well as we can. But the stuff of explanation, I think, will not lie in that direction, as far as I can see.

There is a core of demography, formal demography, in which there are rather few questions, partly because at the core, demography only has a couple of crucial variables and parameters, and probably there's not a living to be made by a large number of people. But I'm quite enthusiastic about the prospect for, in effect, doing experimental, almost mathematical, inquiries using computer simulations. I think that's a large area for imaginative endeavor. There are some excellent young people, much better trained than I ever was, who are showing good signs in that direction.

But I think we have perhaps arrived at a point in demography where we have to get serious about questions of the understanding of social behavior and turn ourselves much more explicitly into social scientists. I think the easy returns from a demographic training are ones that are pretty close to being exhausted. We've now got to get on with much more difficult questions that will not yield easily. They are questions we have in common with any student of social change. The whole theory of social change is in chaos and the demographic transition is a piece of that chaotic situation.

VDT: That's a good point to end on. It leaves you plenty to do for the future when you are, quote/unquote, retired. Will you spend more time in Canada, more than four months, although there are barely four months of bearable weather?

RYDER: If the weather's good, we'll be there.

CONTINUED

VDT: We've picked up again. Something about Norman that I had not known is he's a musician, a piano player all his life. Explain that.

RYDER: Let me explain how I am anywhere near the category of musician. When I was a child, I learned the piano as so many children do, but became completely captivated by the instrument. I was from a working-class family and we couldn't afford more than a year's lessons. So from then on, I used to go to the music store and I would borrow a book which represented the next year's work in music and come home and copy it out and take the book back and say that my mother wouldn't give me the money for it.

VDT: You literally copied the music?

RYDER: I invented a little system of musical shorthand that I could get the thing done rather efficiently. When I was 14, I took up lessons again, again very briefly--another six months--and then had to have a serious conference with my teacher and with my father and mother, because either I was going to have to spend the money on training for the piano or I was going to be able to go to college. The question was which; it couldn't be both. Times were hard then, even for very good piano players, because it was the Depression. And the decision was made that I should go to college instead.

Now, I played the piano professionally in the sense that I belonged to a small jazz group. I remember that we played week after week for a dance at a small private club and in return they would give us practice space during the day. It was a good deal for us and gave us a lot of good practice. This was in Hamilton; I guess I was 13 at the time or 14. One day my mother said, "You are not going to go and play at the club tonight." I phoned my colleagues in the band and their parents had said the same thing to them. I don't know what their source of information was, but the club was raided that night. There happened to be a bordello on the second floor, which we had no awareness of.

Just to wind this up, when I came to the United States, in order to pay moving costs I had to sell my piano. And it was many, many years before I got enough money saved to buy another one. So my piano sort of lapsed. However, I was at a party of the sociology department at Wisconsin and got to playing the piano again and one of my colleagues, a professor named Hans Gerth from Germany, came over and said in his very German accent, "Ryder, you play a very good whorehouse piano." He had me nailed!

VDT: Had you had lessons?

RYDER: As I said, I had a year's lessons when I was eight and six months when I was 14. The piano was very much part of my life when I was a Canadian and when I came to the States I had to redefine myself, because I was always known as the piano-player up there. Down in the States, most people have no idea that I ever played the piano.

VDT: I never did. You play now regularly?

RYDER: No, I don't play very much. It's one of the things I will certainly do a lot of when I get a little free time.

VDT: I hope you will. And you say that Dudley Duncan has taken up much more composing?

RYDER: Yes. This has been an interest of his for a very long time, and he now has the feeling that as an emeritus he's entitled to do whatever interests him, and this interests him very much.

VDT: That's what an emeritus should do. Thank you.

ARTHUR A. CAMPBELL

PAA President in 1973-74 (No. 37). Interview with Jean van der Tak at the Center for Population Research, National Institute of Child Health and Human Development, Rockville, Maryland, February 16, 1988.

CAREER HIGHLIGHTS: Arthur Campbell received a B.A. in political science from Antioch College in 1948 and did graduate work in sociology at Columbia University. He worked at the Metropolitan Life Insurance Company in New York City from 1950 to 1952 and with the Foreign Manpower Research Office of the Bureau of the Census from 1952 to 1956. From 1956 to 1964, he was at the Scripps Foundation for Research in Population Problems at Miami University in Oxford, Ohio, where he worked on the Growth of American Families studies of 1955 and 1960. He was coauthor of the influential volumes which reported on those studies, Family Planning, Sterility, and Population Growth (with Ronald Freedman and P.K. Whelpton, 1959) and Fertility and Family Planning in the United States (with P.K. Whelpton and John Patterson, 1966). He was Chief of the Natality Statistics Branch of the National Center for Health Statistics from 1964 until 1968, when he joined the newly established Center for Population Research of the National Institute of Child Health and Human Development as Deputy Director, a position he still holds. He was NICHD's project director for the National Fertility Studies of 1965, 1970, and 1975 and has been project director of the National Survey of Family Growth since its first cycle of 1973. He has published extensively on American fertility, particularly birth expectations, fecundity trends, and, most recently, low fertility.

VDT: We are speaking at the new office of the Center for Population Research of the National Institute of Child Health and Human Development on Executive Boulevard, Rockville, Maryland, to which the Center has just moved in the past few days.

Art, how did you first become interested in demography?

CAMPBELL: I went to Antioch College and I had a co-op job at the Office of Population Research at Princeton in the summer of 1947. That's where I first became familiar with population research and got to know Kingsley Davis, Frank Notestein, Irene Taeuber, Dudley Kirk, Louise and Clyde Kiser, and Wilbert Moore. They were all there at that time.

VDT: What do you mean by the "co-op" job you had?

CAMPBELL: At Antioch they would send you off on a job for three months and then you'd go back to school for three months, then you'd go on another job for three months, and so on. They called those "co-op" jobs. And I ended up on a three-month job at the Office of Population Research.

Ansley Coale was there as a student at the time and so were George Stolnitz and Georges Sabagh. The particular work that I did, I remember, was preparing some life tables for Japanese cities for Irene Taeuber's book on Japan [The Population of Japan, 1958]. That was a big project at that time. That's where I learned how to make life tables. George Stolnitz showed me how to complete them up to age 100 with some mathematical curves. It was very interesting. I enjoyed that work thoroughly.

VDT: That was in the summer. And when you went back to Antioch you completed the degree in political science?

CAMPBELL: That's right. Then I went to Columbia University and Kingsley Davis went to Columbia at that time, so I took some courses in population from him at Columbia. In 1950 I got a job with the Metropolitan Life Insurance Company in New York, writing little articles for their Statistical Bulletin.

VDT: Were Dublin and Lotka still there?

CAMPBELL: No, Lotka had died by that time. Louis Dublin was still there, and Mortimer Spiegelman. But I inherited Lotka's 20-inch slide rule, which I suppose was the equivalent of a hand-held computer in those days. I had a good time at Metropolitan Life and learned a lot from Jack Barr, who was editor of the Statistical Bulletin.

VDT: Did you work directly with Louis Dublin?

CAMPBELL: No. I worked in the statistical division, which he headed, but I worked directly with Jack Barr.

VDT: And that was mostly life expectancy?

CAMPBELL: Many articles were on life expectancy, but they also had articles on fertility and internal migration.

VDT: In 1956 you went to Scripps. How did that come about?

CAMPBELL: Before 1956 I went to the Census Bureau. In September 1952 I joined what was then called the Foreign Manpower Research Office at the U.S. Bureau of the Census, where they studied foreign populations. There I worked with Jay Siegel.

One of my first projects was the preparation of a report on the population of Yugoslavia. I made some population projections for Yugoslavia and worked up some fertility rates. I also worked on the USSR and I remember having made an age distribution for China around the time of their first census when they were not releasing very much information and you really had to make a lot of guesses about what their age distribution was.

VDT: China, just after it had become the People's Republic, in the early 1950s?

CAMPBELL: Right. I also developed a method of projecting mortality rates that were published in a little bulletin. I worked mostly on foreign populations there. I did a lot of writing and a lot of mathematical manipulations. It was good preparation.

Then I heard about the job at the Scripps Foundation and applied for it and talked to Pat Whelpton and was hired. That's when Norman Ryder left the Scripps Foundation [for the University of Wisconsin] and I replaced him there. He was there from about 1954 to 56.

VDT: That was specifically to work on the Growth of American Families study of 1955?

CAMPBELL: That's right. They had already done the fieldwork for the 1955 survey. I was hired to do some of the analysis. I did the analysis on the birth expectations. That was my main contribution to the 1955 Growth of American Families study.

Then when we got that out of the way--that was published in 1959 [Ronald Freedman, P.K.

Whelpton, and Arthur A. Cambell, Family Planning, Sterility, and Population Growth]--we started on the 1960 Growth of American Families study, which was designed in part to check up on the birth expectations that were given in the 1955 survey. We didn't interview the same women, but we interviewed cohorts that represented the same women.

Ronald Freedman did not participate in the 1960 study, but we still had the Survey Research Center at the University of Michigan doing the fieldwork. I worked on that until 1964. Whelpton died in the spring of 1964 and we were just finishing the book at that time, reporting the results of the 1960 GAF. It wasn't quite done, so John Patterson and I kept working on it and eventually it was published in 1966 by the Princeton University Press [P.K. Whelpton, Arthur A. Campbell, and John Patterson, Fertility and Family Planning in the United States]. The 1959 book was published by McGraw Hill.

VDT: What was it like to work at the Scripps Foundation? Frank Notestein in his last published article, describing the development of demography in the U.S. ["Demography in the United States: A Partial Account of the Development of the Field," Population and Development Review, December 1982], said it would be hard to overemphasize the importance and pioneering work of the Scripps Foundation, along with the Milbank Memorial Fund, in making demography a respectable field. So what was it like to work at Scripps and, in particular, working with Pat Whelpton?

CAMPBELL: He was a very easy person to work with. He had developed the cohort fertility tables. He was a very thoughtful man, very thorough, and very careful in everything that he did. He was very easy to work with, as he had no temperament, so to speak.

The Scripps Foundation at that time consisted of Pat and myself and we had two secretaries, Mrs. Minnis and Mrs. Smith, and for a while, Dick Tomasson worked with us. That was in preparation for the 1960 Growth of American Families study. Then Tomasson left to go to the University of Arizona and John Patterson joined us. John had worked at the State Department, down here in Washington, before that.

VDT: Wasn't Warren Thompson still there?

CAMPBELL: No, Warren Thompson wasn't there when I joined the Scripps Foundation in 1956. Don Bogue was still there part-time, but he went back and forth between Scripps and the University of Chicago. Warren Thompson had retired by that time. He would come in occasionally, so I met him, but he didn't do any work.

VDT: So it was a very small outfit and here you were tackling this enormous project. Well, of course, you had the Survey Research Center of Michigan.

CAMPBELL: Yes, they were very good. It's certainly one of the best survey research organizations in the United States. They had done some preliminary work in 1954 in the Detroit Area Study. Freedman had asked some questions on birth expectations then. This was the aspect of it that I was particularly interested in--birth expectations and the prediction of fertility. I wasn't as interested then as I've become since in methods of contraception and fecundity trends and so forth. In the 1960 survey, I analyzed not only the birth expectations but I also worked on fecundity impairments and that was quite interesting.

VDT: That was one of the main interests in the first survey too, wasn't it--fecundity impairments?

CAMPBELL: Yes, but they didn't go into it as thoroughly as we did in 1960. For example, they did

not try to distinguish between operations for contraceptive purposes and operations for remedial purposes, which we did in 1960. These studies have grown bit by bit. First things first, and then you keep adding new elements to the study.

VDT: You have always been interested in fertility, most of your publications have pertained to that, although you mentioned that you started off in life tables. For example, in your PAA presidential address of 1974--you called it "Beyond the Demographic Transition" [published in Demography, November 1974]--you looked at the 18 developed countries that had very low fertility in the 1930s, the postwar baby boom of the 1960s, and declining fertility since then and pointed out how these trends were related to shifts in the timing of childbirth and that the baby boom was due to a rise in childbearing among younger women, earlier fertility, and some makeup fertility among older women. Then the declining fertility since then was due to a steep drop in fertility at older ages. What do you think the outlook is now for fertility, with the U.S.'s very low, below-replacement fertility since 1972?

CAMPBELL: It's really sort of a guessing game; we have nothing substantial to go on. Demographers in their lifetime may have only one cycle to look at and there are new cycles coming upon us, these cycles of rise and fall in fertility. My guess is that we'll remain at or below the replacement level, because I don't see anything that would cause fertility to increase in the near future. I've been following the fertility of the 1954 cohort.

VDT: Why that particular cohort?

CAMPBELL: They were 30 years old when I started doing this, in 1984, which is an age at which most women have had all the births that they want to have. What I've been interested in particularly is the proportion of zero parity women. The proportion of zero parity women was as high as 23 percent for the 1906-10 cohorts, and the 1954 cohort appears to be approaching that level.

I have two projections, a high and a low projection, for the 1954 cohort. One assumes that they will ultimately have 22 percent childless and the other assumes about 12 percent childless. Then each year that I get the data, I plot the rate of first births for this particular cohort. So far, it's slightly below the 22 percent projection, although when you ask these women how many children they expect to have, the proportion who expect to remain childless is much lower than 22 percent; it's on the order of 12 to 15 percent.

So I think that women are revising their birth expectations downward as they proceed to the older childbearing ages. I've seen no trend that suggests that anything different will happen. Fertility has remained relatively flat for approximately 15 years now. So, unless Easterlin is right . . .

VDT: Yes, the idea that the small male baby bust cohort will want their wives to stay home and have babies while they work. However, I think most birth expectations, as you say, have shown that women consistently overestimate the children they will have. But with your cohort of 1954, you must have data for them only up to about age 32 or 33. There is a tendency to delayed childbearing--the biological clock ticking. I guess, though, it's not such a large proportion that are having children after the age of 33.

CAMPBELL: No, it's really very small.

VDT: It gets a lot of publicity.

CAMPBELL: But it really involves a very small proportion of women having first births in those age groups. Between ages 30 and 34, it's something like only one percent are having first births in any

given year and the proportion keeps declining. One percent is 10 per 1,000, but it's a small proportion.

VDT: Working with fertility and birth expectations must have been very exciting and fun. Would you say that in U.S. demography the baby boom has been practically the leading issue in the last few decades?

CAMPBELL: Yes. It's a phenomenon that's difficult to understand. Why did it occur, this return to emphasis on early childbearing, larger numbers of children, and fewer women childless? I think this is a fascinating problem and we don't know fully what the answer is.

VDT: And now it's gone to the other extreme, possibly 22 percent childless, as you say, and continuing very low fertility.

CAMPBELL: That's a little easier to understand, because we were headed in that direction before. I think these trends have something to do, though perhaps not entirely, with the changing role of women in society. The cohorts of the early 1900s had among them a fairly large number of professional women, women who had gone to college and become physicians or had other professional occupations. Irene Taeuber was a perfect example of that cohort.

VDT: She was rather special, but okay.

CAMPBELL: Yes, she was. But there was Helen Walker, who was president of the American Statistical Association, and there was . . . who was the lady from North Carolina who wrote Statistics for Sociologists?

VDT: Margaret Hagood.

CAMPBELL: Exactly. And Louise Kiser. There were a number of women from the cohorts of the early 1900s who were professionally very successful and this trend, I think, was interrupted in part by World War II and in part the advantages during the postwar period that were given to men who had served in the armed forces and who were released in great numbers and took over the labor market essentially and were able to gain some economic security because of the GI bill, loans for houses, and other benefits that made it possible for them to marry early and begin families early. At the same time, I think women suffered because of this, that is, the advancement of women into professional careers.

But now that's changing and I think it's bound to have an effect on the fertility rate and the effect is predominantly negative. It makes it possible for fertility to remain fairly low, as happened for the cohorts of the early 1900s.

VDT: Well, then of course it was the Depression.

CAMPBELL: No, it wasn't the Depression. The fertility was maintained at a low level during the Depression, but fertility came down during the 1920s. And it was among those cohorts that it came down. So it wasn't just the Depression that caused the cohorts of 1906-10 to have low numbers of births. The earlier cohorts of 1900-1905 also had fairly low fertility. There was something going on that really reduced fertility during the 1920s. You could almost say the Depression stopped the decline of fertility.

VDT: That's an interesting point.

In your book on the 1960 GAF, Fertility and Family Planning in the United States, you made projections at what was then the peak of the baby boom of what fertility could be in the early 1980s. I was interested that your low series projected the net reproduction rate as still above one and a crude birth rate of about 18. However, in numbers--the actual number in the U.S. in 1980 was 226 million, which fell between your low and medium series of population projections, so that wasn't bad.

Of course, there have been a lot of notoriously incorrect projections in the past, starting with Whelpton, who made projections in the 1930s of the U.S. population in 1975 which were far below what it actually was. However, projections got a bit more accurate as they got closer to the period. Of course, the birth expectations data were first collected with the idea of feeding into projections.

CAMPBELL: That's right, it was. It hasn't proved to be as useful as we thought it might be because women do tend to expect more children than they eventually have, particularly women who are better educated. Less educated women expect fewer children than they actually have because they have so many unwanted children. The birth expectations data have not been too useful during this period of change. Particularly with the cohorts of the 1940s, you can see how they kept revising their birth expectations downward as they became older. You can follow this through the series of surveys that we've had.

VDT: The ones collected in June by the Current Population Survey?

CAMPBELL: That's right, those--and the National Survey of Family Growth also has a comparable series. So what it's going to be in the future is very difficult to tell. We made those 1960 projections on the basis of the birth expectations, but obviously they weren't too accurate.

VDT: Let's go back to your career. In 1964 you went to the National Center for Health Statistics. What did you do there?

CAMPBELL: I was chief of the Natality Statistics Branch. We published the annual data on births in the United States and we got the cohort fertility tables started. That was another project that Whelpton and I did at the Scripps Foundation--publish the early cohort fertility tables. These have since been updated by the National Center for Health Statistics. Now we have every year a set of five cohort fertility measures, that is, the central rates, the cumulative rates, the parity distributions, the age-parity-specific birth probabilities, and the parity progression ratios.

VDT: That's been very important ever since Whelpton developed his cohort fertility measures in the 1930s, pointing out that period rates can be misleading.

What led you to go from there to . . . Was the Center for Population Research of NICHD first formed about 1968 when you came here?

CAMPBELL: Yes, it was. Phil Corfman and I started it in August 1968, almost 20 years ago. The purpose of the Center for Population Research was to support population research in the U.S. and we had not only the behavioral component, which would include demography, but also the biomedical component. So we eventually organized it in four branches: the contraceptive development branch; contraceptive evaluation branch, which studies the safety of methods of contraception; reproductive sciences branch, which supports basic reproductive biology; and the demographic and behavioral sciences branch, which includes demography and also psychology, sociology, and economics. It studies not only fertility but also migration and mortality.

VDT: You should write about that sometime. Notestein in that article I mentioned said that he was writing about the part of the development of demography that he knew about, which was the private organizations and universities and so on. He said that if the story had been told by you--and he named you in particular along with Con Taeuber and Phil Hauser--you could say more about ["do more justice to"] the innovative role of government organizations in the development of demography in the U.S.

You've described the four branches of the Center for Population Research. Could you say that it is probably the leading supporter of research in those fields?

CAMPBELL: We are now. In the United States, it used to be that there was very little support for population research in the social sciences. There was Milbank Fund, the Office of Population Research at Princeton, Rockefeller Foundation, and eventually the Population Council came in and then the Ford Foundation. So the private agencies did begin to support population research to a greater extent. But they were never able to come up to the level of funding that we've been able to provide.

VDT: Of course, all those private agencies you've mentioned, except the Milbank Fund, were supporting research on Third World demography.

CAMPBELL: That's right.

VDT: How did it happen that in the late 1960s the time was ripe for setting up the Center for Population Research? Was that part of the Great Society era, the Johnson era?

CAMPBELL: It was part of that and it arose also out of the concern for the population problem--population outrunning food supply and resources in general. You had the Club of Rome, Paul Ehrlich [The Population Bomb, 1968], and a lot of other people giving publicity to population.

VDT: And the Commission on Population Growth and the American Future in the early 1970s.

CAMPBELL: Yes.

VDT: And it was easy enough to get money then from Congress?

CAMPBELL: The amount of money was really not very much. The Center started in 1968 with only about \$2 million. Our initial amount was spent on studying the safety of the pill. That was a big project we had at the Kaiser-Permanente Hospital at Walnut Creek, California. This was a big cohort study of women who took the pill; they did a lot of medical tests on these women to see what the effects of the pill were.

But since that time our budget has grown to well over \$100 million. So the Congress has continued to appropriate fairly large amounts of money for us. Not that we can fund all of our approved grant applications.

VDT: That's one of the biggest hassles in the population research world. Everybody wishes they could get more funding from NICHD. You approve many proposals in a year which you can't fund, is that it?

CAMPBELL: Oh, yes. I don't know what we're expecting to do this year, but if we can fund as high a proportion as 30 percent of approved grants we're doing well.

VDT: Only 30 percent of approved grants? Phew!

CAMPBELL: Yes, but you also must remember that most of our money goes to biomedical research. I say we have well over \$100 million, but most of that is spent on basic reproductive biology. The amount spent on socio-behavioral aspects of population, including demography, would be around \$20 million. That's still a pretty good chunk of money, but research becomes more and more expensive every year. There are computers to be bought, salaries to be paid, and so forth.

VDT: But you can say that the Center for Population Research has been the most important source. Has it influenced policy in the U.S.?

CAMPBELL: Yes, it has--in two ways. Certainly our biomedical research has influenced policies concerning, let's say, the safety of the pill and the IUD. In the social-behavioral areas, it's a little difficult to see what the direct influence on policy has been. But certainly since 1975 we have made a concerted effort to study the problems associated with teenage pregnancy and childbearing and I think this had a big impact on efforts to reduce fertility at such an early age. Just how our studies have affected policy in a direct way is difficult to say. But certainly by making known what the consequences are and what some of the contributing factors may be, I think we've increased the awareness of this problem and society's attention to it. So, yes, I think we have affected policy, but it's just difficult to say how much.

VDT: Now I'd like to turn to your connections with the Population Association of America, of which you were president in 1973-74. Do you recall when you first attended a meeting, when you first joined PAA?

CAMPBELL: I remember attending meetings at Princeton. The first meeting I attended was in the spring of 1947, when I remember T.J. Woofter making a spontaneous intervention about the usefulness of looking at fertility on a cohort basis rather than a period basis. He made quite a point of that. I don't think it was that particular remark that started Whelpton thinking about cohort fertility tables, but certainly that was the first I ever heard of it.

VDT: I was looking back at Clyde Kiser's 40 years of reminiscences on the Milbank Memorial Fund, a series of papers given at the time Kiser retired from the Fund in 1971 [Clyde V. Kiser, ed., Forty Years of Research in Human Fertility, Milbank Memorial Fund, 1971]. Someone [Wilson Grabill] remarked that Woofter inspired Whelpton in cohort fertility analysis ["A few might regard Woofter's paper on generation reproduction rates as a forerunner of Whelpton's work."]. I thought Whelpton had been doing it in the 1930s. No?

CAMPBELL: He had developed the component method of population projections in the 1930s, meaning that you make separate estimates for the different components of population change. But to my knowledge he hadn't started working on cohort fertility.

VDT: So maybe it really was Woofter that put Whelpton onto it.

CAMPBELL: Could be.

VDT: Tell me about Woofter [PAA president in 1940-41]. Nobody seems to recall much about him.

CAMPBELL: I don't either. He was from the South. I can't remember if he was associated with the University of North Carolina or not [yes, 1926-35]. I don't know what his career was. I do know that he worked for the government, but whether it was the Department of Agriculture or some other agency, I'm not sure [Works Progress Administration, Federal Emergency Relief Administration, Federal Security Agency, CIA].

VDT: Do you remember anyone else that struck you at those early meetings?

CAMPBELL: I'm afraid not.

VDT: The first meeting you attended, in 1947, was indeed at Princeton, where so many meetings were then held. Do you recall some issues or events of note in PAA over the years--any outstanding meetings that you can think of?

CAMPBELL: There was a Catholic priest who used to come to the meetings--Father Collins or something like that [Father Gibbons]. He gave a paper on Catholic fertility values ["The Catholic Value System in Relation to Human Fertility"] at one of the Princeton meetings [1949] and I remember Kingsley Davis making a very heated intervention, saying that such papers had no business at the Population Association. [See Philip Hauser's description of this incident in his interview, above.]

I don't remember any PAA meeting that stands out in my mind. I do remember Irene Taeuber's pleasure when President Johnson said in his State of the Union address that problems of population required attention. That opened the possibility for the State Department to start giving population aid.

VDT: That was the State of the Union address of 1965, when he said that five dollars put into family planning was worth \$100 of economic development aid. That was a famous speech and it did indeed precipitate the U.S.'s getting into population aid, through AID's Office of Population, which started in 1968. It took a while for things to rev up. It must have also spilled over into the domestic climate.

Can you recall what offices you've held in PAA?

CAMPBELL: I was a director, on the Board, for a number of years. I recall counting the ballots one year.

VDT: What do you think of how PAA has changed over the years? Many oldtimers miss the Princeton meetings, when you all fitted into one room and there were no overlapping sessions. Now, of course, we have six or seven overlapping sessions, attended by over a thousand people. How do you view the changes over the years and have they been for the better or worse?

CAMPBELL: It was fun having smaller meetings. But I think it's a natural thing to happen, that the Population Association has become larger, and it's encouraging to see that it has.

One of the things that happened while I was president was the revision of the constitution, which put the offices on a calendar basis rather than a fiscal year basis. There were some other good changes, such as the possibility for people to petition to have candidates on the ballot, rather than just to let the nominations committee choose who the candidates would be.

VDT: That was important later on. There was one year when not a single woman was candidate for the Board and, of course, the women rose up in wrath and proposed three or four. That might have been one year that Wendy Baldwin was proposed as a candidate.

CAMPBELL: Then for a while there we had the Concerned Demographers. They used to have a

publication in the early 1970s.

VDT: Things were still heated up in the early 1970s, with the student unrest and Vietnam war protests that began in the 1960s.

I recall your meeting when you were president, in 1974; it was in New York. I was not impressed with New York. Do you think it's better to have meetings in smaller cities? Although Chicago last year [1987] was a good venue.

CAMPBELL: I liked Chicago; I thought that was a good meeting. I didn't go to the San Francisco meeting [1986], but I went to the American Statistical Association meeting in San Francisco last year and I think that's an ideal city to have meetings in. They have such a nice conference center there; it's not far from downtown. You can walk to the conference center. Of course, New Orleans ought to be a good place. That's where we're meeting this year [1988].

VDT: Have you attended most of the PAA meetings over the years?

CAMPBELL: Yes, I'd say most of them.

VDT: Do you come away refreshed, meeting new people, new ideas?

CAMPBELL: Oh, yes.

VDT: Who would you say was the most important influence on your career?

CAMPBELL: I would say Kingsley Davis. He certainly got me started in demography.

But it would be difficult to choose between him and Jay Siegel, because I learned an awful lot from Jay Siegel. I worked with him at the Census Bureau and he was a very good person to work with, because he taught me so much. For one thing, he taught me how to write. He was a stickler for good grammar, and he was a heavy editor. He could instruct you and still be pleasant about it; he wasn't overbearing at all.

VDT: He's still teaching, as you know. I was supposed to interview him last month for this series, but he was leaving for Cornell, for this semester, till June. He was trying to finish his 1980 census monograph on aging and that was behind schedule, and he was winding up at Georgetown, where he has been teaching for the last few years. You didn't have him as a teacher, but as a colleague.

CAMPBELL: He's a perfectionist. That's why it takes him so long to do things, but when it's done it is perfect. That makes it worthwhile.

VDT: And Kingsley Davis. It's interesting that you name two men who have had such long careers in U.S. demography. Kingsley, of course, is still very active. He's chair of a session at the next PAA meeting on the history of demography, "Two Centuries After Malthus: The History of Demography." What was he like? He must have been just a young man when you studied with him at Columbia.

CAMPBELL: He was very helpful, a very dynamic person. And had a sense of the importance of social structure and the influence of social structure on population that I think is unparalleled. So many demographers today are primarily interested in the mathematical techniques of studying population and I think to a certain extent we may have lost this social structure orientation, this view of

society and population that Kingsley represented so strongly. I think the methodologies that have been developed are certainly very interesting and helpful, but often they are not used to do anything but demonstrate a particular methodological technique, rather than to say something about the relationship between population and society. And this was what his whole career was about--population and society. I think we need more Kingsley Davises.

VDT: That's an interesting point. I've been reading about Frank Notestein. People said that he was a marvelous, well, theorist, of course--there was his demographic transition theory, which has since been much criticized--and seemed to have an overarching view of the importance of society, but was perhaps sloppy with some of the details. But don't you feel there's room for both?

CAMPBELL: Oh, sure.

VDT: You pointed out back in the 1966 volume, Fertility and Family Planning, and again in your 1974 PAA presidential address that fertility decisions are made in a socioeconomic context, so obviously you've thought about this for a long time, that one should look at fertility decisions, migration decisions, whatever, in the context of the social climate. Do you find that missing in, say, current PAA meetings where so many of the papers are very methodological?

CAMPBELL: Yes, I do. I would like to see more emphasis on social aspects. I think a lot of the work on teenage pregnancy and childbearing has this element to it, that is, the connection between society and population. But so many of the other things don't.

VDT: What has given you the most satisfaction in your career?

CAMPBELL: That would be difficult to say.

VDT: Perhaps the two books from the GAF, or your part in setting up the Center for Population Research, which has become so enormously important in the field in the U.S.?

CAMPBELL: I don't know what has given me the most satisfaction. I think one of the things I do find satisfying is the opportunity to continue to learn, to study some of the new techniques. Although I say people may give them a little too much emphasis, nevertheless, they are interesting. Methods of log linear analysis, for instance, which I've been studying recently. I can see how they can be helpful. New techniques of categorical analysis could be very helpful in analyzing fertility surveys.

I think what gives me the most satisfaction is to analyze the results of surveys, or to analyze cohort fertility rates. To do things like that. The administrative things that I do are not really very satisfying. They are really kind of humdrum; I don't find them something I look forward to. I think this is probably true of a lot of people in this field.

VDT: Not just in this field, I would say. I've peripherally seen what NICHD requires for proposals and it's pretty overwhelming. Then when you get the money, the reports that are required.

CAMPBELL: Yes. So the things I find satisfying are really sitting down with computers and analyzing numbers, playing with them, trying to see what's there. For instance, analyzing things like unwanted childbearing to see how this relates to socioeconomic status, education, religion, etc. I like to do things like that. That gives me most satisfaction.

VDT: I think you're fortunate that you've been able to continue to do that. How are you connected with the National Survey of Family Growth? You mentioned that Cycle IV, which I had thought was in the field last year, is actually only just happening now, because questions on AIDS were added to the schedule.

CAMPBELL: I'm project officer on that survey, so I have to stay in close touch with it. Cycle IV is going to be a landmark survey.

VDT: Because of the AIDS questions?

CAMPBELL: Well, that, and they hope to be able to do some longitudinal studies, that is, reinterview a sample of women 17 months after the main survey is done.

VDT: Like the National Fertility Study of 1975, that reinterviewed women from 1970?

CAMPBELL: Exactly. So that should be quite interesting. I'm pleased about that.

VDT: How big a sample is it?

CAMPBELL: They expect to have between nine and ten thousand interviews this time. The sample size is something like 10,600, but then you don't get everybody. And they plan to reinterview half of those people.

VDT: That's a lot!

CAMPBELL: By telephone, so it shouldn't be too expensive.

VDT: That's great. You should be very satisfied with your part in the National Survey of Family Growth.

CAMPBELL: Well, as I say, the satisfying thing for me about that is to play with the numbers, once I get them.

VDT: Do you do more analysis than the actual staff--Bill Pratt, Bill Mosher, etc.--do?

CAMPBELL: No, I don't think so.

VDT: That's always been a very interesting survey. I was in on the planning meetings back in the early 1970s.

CAMPBELL: I remember. Georgetown had the contract to set up those planning meetings.

VDT: Right. Then it was thought they were going to have them every other year. Do you regret the fact that they've been spaced out as they have been [1973, 1976, 1982, 1988]?

CAMPBELL: No, I think that's okay. We can just make estimates those other years. It takes time to analyze these big surveys. I would rather have one big survey every five years than little surveys every two years, because you can go into more detail, more depth, in the larger size.

VDT: That's right--with the sample size now up to 10,000; that's marvelous. It was 2700 [2,713] in 1955?

CAMPBELL: Yes.

VDT: Well, of course, the U.S. population has increased enormously too.

And these U.S. fertility surveys have led to the World Fertility Survey, the Contraceptive Prevalence Surveys, and Demographic and Health Surveys all over the world. That's a tremendous contribution, I would say, of U.S. demography.

CAMPBELL: Yes. I just wish more countries would have cohort fertility tables. And they could too, especially the developed countries. It would make it a lot easier.

VDT: Could they get cohort fertility tables just from their birth registration?

CAMPBELL: No. You have to start somewhere and you have to start with something like a survey of children ever born.

VDT: Actually, not many developed countries have had good surveys, compared to the U.S. Jerzy Berent put together those they had around the early 1970s.

CAMPBELL: There have been quite a few, but they haven't been on a regular basis. Of course, the U.S. surveys have not been on a regular basis but, still, they have been quite useful.

VDT: I think it's a tremendous achievement, and people thought at the beginning that women would be loathe to talk about . . .

CAMPBELL: Yes, and now we're asking all kinds of embarrassing questions.

VDT: And this next time AIDS is going to be asked of the women?

CAMPBELL: Well, it's really pretty much a matter of asking what they know about it to see what the level of information is about AIDS and how they think it might be prevented and how they think it is transmitted. It's mostly questions about the information they have about AIDS.

VDT: The NSFG has never attempted to interview husbands. Some of the early surveys--was it the Princeton survey that interviewed some husbands?

CAMPBELL: Indianapolis interviewed husbands, in 1941. Nowadays you have to say "partners." It's like "householder."

VDT: Right. You don't dare have "head of household."

CAMPBELL: I don't think the Census Bureau ever asked husbands about birth expectations. They may have; I can't remember now.

VDT: Do you think that would be a good idea?

CAMPBELL: Sure. Very often there are differences between husbands and wives in the number of children wanted. You pick that up just by asking the wives how many children they think their husbands want. But it would be also useful in terms of, let's say, methods of contraception being used currently, whether they are being used, in the first place, and whether they agree with the wife. It would be a good way of seeing how consistent the information is.

VDT: What do you see as the outlook for demography in the United States--more and more people in it? You've said that it's becoming very methodological. Obviously, fertility continues to be an area that fascinates a lot of people. How do you think the future looks for the discipline of demography in the U.S.?

CAMPBELL: I think it has gained importance and recognition. I think that it's going to be very important in the future, especially as the baby boom ages. When they reach 65, there is going to be an explosion. The kids who are being born now are going to have to support these old people. I think that the social issues this raises are going to be tremendous. There may even be pronatalist efforts in the future. Who knows?

VDT: Do you think when you come out with your studies on fertility, showing it so low, you might precipitate pronatalist policies? You have said that your research on adolescent pregnancy and childbearing has had some impact on policy.

CAMPBELL: Well, it's pretty obvious to the people concerned about Social Security, for instance, that adjustments are going to have to be made in the future. Whether these will be pronatalist or not, I don't know. But certainly there will be concerns and debate about this, and I think population research will contribute to this.

CHARLES F. WESTOFF

PAA President in 1974-75 (No. 38). Interview with Jean van der Tak at the Office of Population Research, Princeton University, May 10, 1988.

CAREER HIGHLIGHTS: Charles Westoff was born in New York City. He obtained a B.A. in international relations in 1949 and an M.A. in sociology in 1950 from Syracuse University and the Ph.D. in sociology in 1953 from the University of Pennsylvania. He was a research associate with the Milbank Memorial Fund in New York for two years before going in 1955 to Princeton, where he has been Professor of Sociology (since 1962) and of Demographic Studies (since 1972), Chairman of the Department of Sociology (1965-70), and Associate Director (from 1962) and then Director (since 1974) of the Office of Population Research. In 1958-62 he was also Associate Professor of Sociology at New York University. He was Executive Director (with Robert Parke, Jr.) of the Commission on Population Growth and the American Future (1970-72) and has been an adviser to the Census Bureau and the Demographic and Health Surveys, a director of the Alan Guttmacher Institute and the Population Resource Center, and a member of the Committee on Population of the National Academy of Sciences.

He is famous in the field of demography for his influential work and publications on fertility in the U.S. and developed and developing countries. His Ph.D. dissertation analyzed data from the 1941 Indianapolis Fertility Study. He was director (1955-70) of the Princetown Fertility Study and codirector, with Norman Ryder, of the National Fertility Studies of 1965, 1970, and 1975. He designed the core questionnaire for the World Fertility Survey, with Norman Ryder, and designed the basic questionnaire for the Demographic and Health Surveys. He is author or coauthor of more than 100 journal articles and book chapters and at least dozen books.

VDT: What led to your interest in demography and particularly in fertility?

WESTOFF: When I finished graduate school [at Pennsylvania], I worked for two years at the Milbank Memorial Fund in New York, before I came to Princeton. It was Clyde Kiser who was responsible for getting me interested in fertility research.

VDT: How did Clyde Kiser know about and find you?

WESTOFF: It was through Dorothy Thomas. Dorothy Thomas had been my major professor at Penn and I guess Clyde had been in contact with Dorothy, looking for some graduate students who would be interested in developing a thesis around the as-yet-not-completed Indianapolis Study. That was the first large-scale survey of fertility ever done.

VDT: And the data sat there unanalyzed.

WESTOFF: The war interrupted it. There were lots of loose ends to that project. He was looking for graduate students, and graduate students are always looking for a thesis topic. So it was a nice marriage of common interests.

[Adding to the interviewer's sketchy biographical introduction]: I was an associate professor of sociology at New York University for four years, 1958-62. I was at the Milbank Memorial Fund from 1952 or 53 to 1955. I came to Princeton in 1955. Then while still on the research staff here, I went on a part-time basis here and took the faculty position at NYU in Greenwich Village. I taught there for

four years and came back to Princeton full-time in 1962. With the exception of that stint in Washington with the Commission on Population Growth and the American Future, I have been here uninterruptedly.

VDT: How did you get into demography?

WESTOFF: I guess it was when I was an undergraduate at Syracuse. I read some of these scare books about how population was beginning to explode. There was a book by Guy Irving Burch and Elmer Pendell [Population Roads to Peace or War, 1945]. I became fascinated with the power of exponential growth. I think that's what first got me started, first turned me on intellectually to the issue of population.

Then I took a couple of undergraduate courses and then did a master's thesis on social mobility and fertility. Then I got an offer of a teaching assistantship at the University of Pennsylvania, in which I could earn some money and have free tuition and go to graduate school at the same time. It was a department of sociology that had a strong population contingent.

VDT: Dorothy Thomas was your professor there. Were you doing migration?

WESTOFF: I was sort of the maverick of the crowd. Everybody else was. Not everybody, but I don't remember anyone else being interested in fertility. Two of my colleagues at the time were Sid Goldstein and Dick Easterlin, though I didn't see much of Dick; he was in the economics department. I think that was before he was interested in fertility. It was a department that concentrated a lot on migration. And it was only, I think, because of the chance contact with Clyde Kiser that I was sort of rescued from the department specialty. I think fertility is a lot more interesting than migration.

VDT: What was Dorothy Thomas like?

WESTOFF: She was--one of the adjectives that quickly comes to mind--a tremendous bundle of enthusiastic energy. Enthusiasm is a big key to her personality. And she would kind of adopt graduate students and push them hard. I was one of her favorites at the time, as were Everett Lee and Sid and others who were closer to her own interests. She was an extremely supportive person, who was always motivated to get the students finished with all their requirements and get down to the serious business of doing their research.

VDT: She must have felt you deserted her if you went off to the Milbank Memorial Fund.

WESTOFF: She may have.

VDT: You did the requirements for the Ph.D. with her?

WESTOFF: Not only with her; she was the main person. Ed Hutchinson was also on the faculty and I studied with him. His interests were quite different too; he was interested in immigration. He had a very keen mind and I learnt a good deal of what might be called--or I got a good taste of the logic of scientific inquiry, methodology in its logical sense, working with Ed.

VDT: What was Richard Easterlin like?

WESTOFF: I don't remember. Actually, my close friends there were not people in population.

Marvin Bressler, for example, who was a student there and has been for the past 15 years chairman of the sociology department at Princeton. He and I go back a long way together. And several other people who are not demographers.

I think all careers have this kind of curious history of part accident--of what determines your interest in a career. You bump into it at a particular time--serendipity. If you look for any great plan for my life, I think you'll be disappointed.

VDT: Has there ever been anyone who started off saying, "I'm going to be a demographer"?

WESTOFF: I don't think so.

VDT: What was the topic of your dissertation? What data did you use from the Indianapolis Survey?

WESTOFF: I first got interested in the social psychology of fertility and did some work in that area. They had asked a lot of questions designed to measure women's self-confidence and personal adequacy or inadequacy and how that related to fertility. Which it did not.

Subsequently, when I came to Princeton after Milbank, we put a heavy emphasis in that so-called Princeton Fertility Study on trying to tap various personality dimensions to hook them into planning--the propensity to plan effectively--how that translates into contraceptive practice; how it might be responsible for contraceptive failure and so forth. But none of that ever turned out. I sort of gave that up many years ago.

VDT: Why did you go on with that in the Princeton study when it really hadn't yielded much in the Indianapolis study? You felt you had to find something there?

WESTOFF: Yes--despite all the evidence. Well, there's always the possibility that what you are really after hasn't been conceptualized clearly, hasn't been measured accurately. The last thing you do is give up the theory. You always think you can improve the measurement first. Well, I had enough of that and gave up after the first wave of the Princeton Fertility Study was over. Haven't gone back to it since.

VDT: The psychologists got into it [fertility research] in the 1970s. I worked with Henry David and have been in touch with the social-psychological angle. They're still trying.

WESTOFF: I think it's a waste of time.

A lot of my thinking, I think, about the nature of fertility has been shaped by my close work, contacts, with Norman Ryder. We collaborated on several books in subsequent years. I think I have moved much more in the direction of thinking of fertility as the property of an aggregate, as a population aggregate. I don't think it's useful, for demographic purposes, to think of it in individual psychological terms.

VDT: That sounds plausible. You were director of the Princeton Fertility Study during all its 16 years. You started in 1955, when you came from the Milbank Memorial Fund?

WESTOFF: Actually, that research was begun [in 1954] at the Milbank Fund by a large steering committee that was set up, where Frank Notestein played a major role--and Clyde Kiser. I got moved down to Princeton on July 1st, 1955.

VDT: That was an interesting survey; one of the first in this country, with that select sample of women who'd had a second birth in September 1956 in metropolitan areas. I had here that it, like Indianapolis, found little in the way of influential social-psychological variables. You've just confirmed that. You said then--I glanced through The Later Years of Childbearing [1970]--that a longitudinal survey was the best way to follow U.S. fertility. Do you still believe that?

WESTOFF: It's only through a longitudinal survey that you can answer certain kinds of questions. I don't want to open this whole line of discussion, because it's endless, but I think they're greatly overrated. The longitudinal design is greatly overrated for purposes of studying fertility. And it's an extraordinarily expensive and time-consuming way of doing research.

Now, it is one way of evaluating--and we mined the data quite extensively for this purpose--one way of assessing the reliability of such information that you collect. And we learned a lot in that effort by reinterviewing the same women about the same events at two different points in time and measuring the consistency of response. You can learn a great deal about what sort of biases occur--recall of critical pieces of information in the study of contraceptive failure rates, for example. We learned that there was some tendency for women who initially reported a pregnancy to be the result of a failure with the rhythm method to subsequently report about the same event that they had not used contraception. That's a plausible kind of thing, but it does have an effect on calculating failure rates that's not trivial. We learned, I think, a lot of things of that kind, but those are methodological results.

We did a longitudinal study also later on in the National Fertility Study [reinterviewing in 1975 respondents from the 1970 survey] and repeated a lot of these same kinds of analysis. There were a lot of substantive things that came out of these, but the major finding was that women are not very successful in forecasting their own fertility--individually. In the aggregate, it's not bad.

Once again, I think there's another lesson that the appropriate level of demographic analysis is in the aggregate. Let the compensating error work in the aggregate. Some women say they're going to have a child and don't, and others say they're not going to and do. A lot of this tends to balance out, so you can get fairly accurate predictions from the percent of women who say they're going to do something, rather than relying on the stability of individual behavior.

VDT: That was one of the great findings between the 1965 and 1970 National Fertility Studies, that they did balance out in the aggregate. Then you had the longitudinal study following the actual individuals [1970 to 1975] and that was pretty much what you also found.

What led you to suggest the National Fertility Study of 1965 and then 1970 and 1975, following the Growth of American Families studies of 1955 and 1960?

WESTOFF: I can remember that quite vividly. Norm Ryder and I were attending a conference on population genetics over here in the Princeton Inn, before it became a coeducational dormitory. One of our great losses, the loss of that facility. It was a hotel and a lovely restaurant.

VDT: I know. My husband and I came here on our honeymoon. I had a brother at Princeton and his wedding gift to us was a night at the Inn. Cost \$12 then [1952] and it was a big gift.

WESTOFF: We learned at the meeting that the Growth of American Families study, which was at Michigan--the Scripps Foundation [at Miami University, Oxford, Ohio] developed it and Freedman and Whelpton and later Arthur Campbell collaborated, and Norman Ryder was part of it in the early days; he was out at Miami University. We learned at the meeting--this would have been about 1964 [Christmastime 1964, according to Ryder]--that this was not going to be repeated; people were doing other things. Ryder and I, with the arrogance of youth, said, "What the hell, we can do this out of our

hip pocket with all of our survey experience"--him with the original [1955] GAF study and me with the Princeton Fertility Study.

I've never learned that lesson yet in my life, that you can't just take a questionnaire and cross out a few things, add a few things, and go with it. A year later you're still fooling around with that questionnaire. I've had that experience over and over again, with the World Fertility Survey and more recently with the Demographic and Health Surveys. We spent a year devising a [DHS] questionnaire I thought we could knock off in a couple of weeks.

So we said, "Let's do it." And there was some funding available. I don't remember the details of this too well, but I think originally it was the National Cancer Institute. This was the time the pill was getting some use. I think it had been licensed in 1963 [1960] and was beginning to get used by increasing fractions of American women. And there was some concern about getting some baseline data, demographic characteristics, on who was using it. Although the Cancer Institute was not interested in fertility studies, they were interested in the data on contraception, because it is a national sample. So that gave us an additional impetus. It also coincided with the creation of the National Institute of Child Health and Human Development [which actually funded it. See Ryder interview, above.]. I remember that Eunice Kennedy Shriver was involved in that.

So that's how it happened. A chance discussion at the Princeton Inn started the whole thing.

VDT: It certainly led to a magnificent series. It wound its way through the World Fertility Survey, the Contraceptive Prevalence Surveys. Were you involved in that too?

WESTOFF: No, the Contraceptive Prevalence Survey I was not involved in.

VDT: But you say you've been involved in devising the questionnaire for the Demographic and Health Surveys?

WESTOFF: Yes. And Norman Ryder and I designed the World Fertility Survey questionnaire. Then I took responsibility for this DHS questionnaire design. As I said, it was a year's work. These things are not done quickly. And now, working with the results of it, I'm already making a mental checklist of things that have to be changed. But it's the nature of the whole enterprise--continuous improvement in measurements.

VDT: You have a big niche in the history of these surveys. It sounds like you and Norman Ryder almost singlehandedly carried them through. Well, you had a few predecessors, like Clyde Kiser.

WESTOFF: Ron Freedman.

VDT: Of course, and Whelpton. You made a tremendous contribution to a tremendous achievement, despite Kingsley Davis's very critical review of the World Fertility Survey. Have you seen that, in the Sociological Forum ["The World's Most Expensive Survey," Special Issue: Demography as an Interdiscipline, Fall 1987]?

WESTOFF: No, I saw a reference to it.

VDT: In his usual way, he says that it lacks socioeconomic background, the social and cultural setting in which these parameters unfold.

WESTOFF: Well, that's true. I think the criticism can be made, which I make quite frequently, that

they have not made any great contributions to understanding of the social and economic determinants of fertility. Maybe surveys are not appropriate instruments for that kind of research. But they have led to an enormous expansion of our knowledge of the proximate determinants of fertility. I don't know whether Kingsley said that or not, but he should if he doesn't.

VDT: Well, he does say they led to large masses of data.

WESTOFF: I saw the title of it, "The World's Most Expensive Survey."

VDT: That's right. He wasn't sure it was worth the \$40 million spent on it. That's peanuts compared to many other things.

What do you consider the most important findings to come out of the National Fertility Study? Of course, much of this was covered in that series, initiated by Barbara Wilson, of videotaped interviews with directors of fertility surveys. [Six interviews conducted in summer 1985, shown at the 1986 PAA meeting. See PAA history "vignette" by Barbara Wilson, "Videotaped Interviews About American Fertility Surveys," PAA Affairs, Winter 1985.] But off the top of your head, what do you think were the most important findings to come out of all three?

WESTOFF: I think the single most important finding of that research, although that represents my own kind of pet interest, was the demonstration of the demographic significance of the level of unwanted fertility in this country.

VDT: That was the big finding of the 1965 survey already and everybody was waiting to see what happened in 1970.

WESTOFF: It came out in the 1970 survey as well. It also explained the differences between black and white fertility.

I remember getting into a television debate with Jesse Jackson, in fact, in 1973, I think it was, following the Commission report [Population and the American Future, report of the Commission on Population Growth and the American Future, 1972]. He was one of the critics on a WGBH national television program, which also included Ben Wattenburg and Marjorie Mecklenburg. Jackson has changed his tune completely on this subject now, but at that time he was arguing the genocide line, that we were pushing or interested in birth control because it would reduce the political power of blacks. He said that if you looked at the black mayors in a lot of Midwestern cities, this was what we were afraid of. It occurred to me, fresh from these fertility surveys, and I then said that I'd done a great deal of research on black fertility in this country and it was quite clear that a lot of black women were having babies that they didn't want to have. It was not exactly in their best interests, especially teenagers. And I said, "Are you arguing that it's in your political interest that they should continue to do this?" That sort of stopped the argument; it was an effective response. Now, I should say to his credit, one of his most effective speeches is on teenage fertility in the black community and all the liabilities that go with it.

That information--provoked as a matter of fact by Fred Jaffe [of the Alan Guttmacher Institute], who pushed me into writing this paper on unwanted fertility which we did with Larry Bumpass. It was published in Science magazine many years ago ["The Perfect Contraceptive Population: The Extent and Implications of Unwanted Fertility in the U.S., 1960-65," Science, Vol. 169, pp. 1172-1182, 1970]. Had a lot of impact and it continues to have impact now in developing countries.

VDT: That's a very famous paper, indeed. Let's talk about the Commission on Population Growth and

the American Future. That was a tremendous task. The implementing legislation for the Commission was passed in March of 1970 and in March 1972 you produced a report. As staff director, you had to collect over a hundred research reports; there were all those meetings. How did you manage all that in that short time?

WESTOFF: And I wasn't even on leave.

VDT: You were on two-thirds time.

WESTOFF: That's right.

VDT: You were still lecturing at Princeton; you went back and forth?

WESTOFF: Yes. That was a fascinating experience, in retrospect. I'm sure I must have complained continuously during it. It was an awful challenge.

VDT: How come you were chosen to be head of that staff? The leading demographer, known to be a good administrator?

WESTOFF: I don't know why I was chosen. You've got to be lucky, I guess. I guess it was at the suggestion made by Barney [Bernard] Berelson to John Rockefeller [3rd] and many very interesting and sort of humorous conversations I had with Rockefeller when he was trying to recruit me for this task. I would relay these conversations to Berelson. One of the central themes of it was that I was going to have to commute back and forth because I wasn't going to leave Princeton and that was going to be pretty damn expensive--maintaining two households, an apartment in Washington, and transportation back and forth. I kept having these conversations with Mr. Rockefeller on how are we going to do this? Barney finally got an idea. "Next time he asks you that question," he said, "say, 'Mr. Rockefeller, if you know a rich man by any chance' . . .". And, indeed, that turned out to be the solution. Because I ended up working for Mr. Rockefeller rather than for the federal government. That's a lot nicer.

VDT: Why did you do that? More money?

WESTOFF: Yes. He was paying all these additional expenses, whereas if I was working for the government, he could not have supplemented my income, because it would have been contrary to government regulations.

VDT: Why did you commute? I phoned up to ask about trains the other day and there's only one a day to Princeton Junction.

WESTOFF: There used to be a lot more. They didn't go to Princeton, they went to Trenton, and the Metroliner used to stop at Trenton all the time.

VDT: It still does.

WESTOFF: Only once or twice a day, but it was regular then, so I took the Metroliner.

VDT: And you really kept up classes, students?

WESTOFF: I don't remember what I did, but I did it. It was pretty tiring. But it was a stimulating time of my life, which I remember fondly. Beginning with Rockefeller, it was a great experience. Indeed, some of the people who were on the Commission became close friends. George Woods became a very close friend of mine, Missy Chandler of Los Angeles.

And I think the report had some impact. It fell, of course, on the somewhat unresponsive ears of Mr. Nixon.

VDT: Maybe not unresponding, but responding in a way that one didn't want at that time--the reactions to the proposals for teenage contraceptive services and legalized abortion.

WESTOFF: Yes, that's right. The report happened to coincide with the beginning of the 1972 presidential campaign and Nixon just couldn't resist using it to pick up points with the Right-to-Life people and the right-wing political groups.

VDT: Already then. But other than that, do you think it has had some long-lasting effect? I picked it up the other day; there was an incredible range of people who contributed to the research reports and on the Commission itself.

WESTOFF: Oh, I think a lot of those papers continue to be referred to. The work that Ansley Coale did, for example, which was a little technical appendix and which turned out to be quite important, on the implication of immigration for achieving zero population growth in this country. He worked on the formal demography of that, a statistical appendix paper. The whole volume on the economic and environmental consequences of population growth. That was the first time that that whole subject had ever really been frontally approached. A lot of research has been done on the Third World--what are the economic implications of population growth--but not for developed countries. So that, I think, was breaking new ground.

VDT: You showed that through the various impacts of the two-child or the three-child family?

WESTOFF: That's right--the environmental and economic consequences.

Then I think the report itself, which went to the White House and Congress, was--if I may be immodest--very well done. I think a lot of the intellectual quality of that has to be credited to Berelson, because he and I worked very closely.

VDT: He was sort of an adviser to you?

WESTOFF: No, he was on the Commission, but he played a very active role. And as anybody who worked with him closely, or knew him well, would quickly recognize, in his ability to hammer away at certain points, making sure the argument held water, trying to politically bring in some of the minority opinion on the Commission, trying to get a consensus, there was something of a good politician as well as an intellect. Dudley Duncan was on that Commission too and he was one of those who had problems with that.

VDT: In what way?

WESTOFF: He wanted to sort of hit everybody on the head with things, like the birth rate in this country is too damn high, ruining the country, and abortion is a good thing because it will bring down the birth rate. Stuff that would get you politically cut off at the knees.

So we had a lot of fun, and a lot of frustration working with people who were politically oriented. But that's natural, I suppose.

VDT: You mean those who were on the Commission?

WESTOFF: Some outside people, outside the Commission.

VDT: There were four Congressmen on it.

WESTOFF: Senator Packwood was very, very interested and went to the meetings and participated in it. There was a congressman.

VDT: Scheuer?

WESTOFF: He came into it at the end, made a lot of noise. He's been very supportive of population activities, but Packwood made more contribution. There was a congressman from Illinois, John Erlenborn, who was really superb. He was very faithful in coming to meetings and had to be convinced, was quite concerned about things, but ended up developing a real interest. I later encountered him at an international conference on population. Good guy.

VDT: Of course, the Commission report came out just the year U.S. fertility dropped to replacement level. Like the committee that we had in the 1930s [Committee on Population Problems of the National Resources Committee], set up because of concern about population decline, that came out with its report [1938] just as the baby boom was about to kick off.

WESTOFF: Yes, that's right. And abortion was legalized at the same time [Roe v. Wade, January 1973]. That cut off a lot of things.

VDT: Both reports [1938 and 1972] have been highly praised, that you were balanced and you came out in favor of the welfare of youth and you didn't use scare tactics.

WESTOFF: I think the essential theme of the recommendations of the report was--there was some sort of fancy political footwork on this--but what we did was that we did not argue for population stabilization in a direct sense, which Dudley Duncan, for example, would have preferred--that zero population growth would be good for the nation and therefore we ought to try for it. But rather that the control of fertility, the avoidance of unwanted pregnancies, was good for individuals and individual families and for the wanted children. And this is sort of equivalent to, as it turned out, ultimate replacement fertility and zero population growth. That would be a consequence of doing what's good for the individual and for the family, which has this additional salutary consequence of leading to zero population growth, but emphasizes the immediate micro objective, rather than the long-range macro objective. The long-range objective is a consequence of this micro-level consideration or concerns. I think that was a nice bit of footwork that we did in there that made it much more politically palatable. So what you do is that you arrange the environment in such a way that people can maximize their self-interest. The consequence of this self-interest is a macro result, zpg, which for other reasons we think is also advantageous.

VDT: That's a marvelously psychological way to handle people.

WESTOFF: It dissolved a lot of opposition, I think. Disarmed a lot of opposition to what otherwise might be seen as a sort of Machiavellian policy.

VDT: Why don't they emphasize more reducing unwanted fertility in developing countries? Of course they do, but then there are also those who say you've got to reduce wanted fertility too.

WESTOFF: I'm working on that, as a matter of fact. I'm writing a paper for a National Academy of Sciences meeting in October and I was working this morning on the subject of unwanted fertility, using some data from Peru.

VDT: From their Demographic and Health Survey?

WESTOFF: Yes--from Peru, Colombia, Dominican Republic, and Sri Lanka. All DHS surveys.

VDT: Isn't it marvelous how fast their data are available?

WESTOFF: That's the beauty of the use of micro-computers in the input of the data in the countries. The interviewing stops on Tuesday and by Friday you've got a data bank. It used to take a year.

What I was writing about this morning and you can see very clearly from the data is that it's true that even if you eliminated all unwanted fertility in several of these countries, you would still be a distance from replacement fertility. However, if you look at the wanted fertility of the urban populations and of the more educated, secondary-school educated, the wanted fertility is down at replacement. So you can see a future coming with more education and improvements in contraception, which will affect the unwanted fertility of rural populations and the less educated, and also the process of modernization and urbanization will also reduce the levels of wanted fertility.

VDT: As demographic transition theory has already said.

WESTOFF: Yes.

VDT: Despite all the exceptions to that, as found in the European Fertility Study. That's what Kingsley Davis has always hammered at, you have to bring down the wanted fertility. But it's nice to know that you've cleverly found that low wanted fertility exists in one segment of society.

WESTOFF: Yes, but in the segment that the rest of the population is moving toward. It's like looking at Sweden if you want to find out what's going to happen in the United States.

VDT: Yes--living together. Although sometimes you look at Sweden two centuries ago and say it's going to happen. What people are concerned about is whether it's going to happen fast enough, bring fertility down soon enough.

WESTOFF: Well, it is. Look at Brazil; the birth rate has just collapsed there. In their DHS, the fertility rate is down to 2.8.

VDT: National--2.8, incredible! Even including the Northeast?

WESTOFF: Yes, including the Northeast.

VDT: That's what makes this whole series of fertility surveys that you've been involved with so exciting, that you can find these things, and reliably. Of course, they can be found retrospectively in censuses--whenever they're taken.

WESTOFF: All I can think of is what's going to happen in Africa. Because looking at Africa now is like looking at Latin America 30 years ago. We would be very pessimistic 30 years ago, take the line that you're saying Kingsley Davis did, that you'd need all kinds of expensive modernization to increase the per capita income in order to reduce fertility. Thailand is another example of this [fertility decline before extensive modernization].

VDT: Were people so pessimistic as they are now about Africa? Their fertility seems to be so stubborn, with 8.5 in Yemen and Sudan.

WESTOFF: Well, the Moslem countries are another issue.

VDT: Ansley Coale has said if you take the marriage patterns of Africa and you impose on them the Hutterite pattern of childbearing, you get a lifetime fertility rate of 10 and no human population has ever approached that, but they're approaching it in Africa now. It seems very discouraging.

INTERRUPTION

VDT: We're talking about the Office of Population Research. The university spent a million dollars rebuilding the current building after the eating club left?

WESTOFF: Yes. We came here in 1974.

VDT: It's a beautiful place, a lovely part of the world. Princeton doesn't seem to have changed much; it must have been great to live here all these years.

WESTOFF: Yes, it has been, but it's changed enormously. The traffic is terrible; population has just zoomed.

VDT: From 12,000 to 13,000?

WESTOFF: Oh no, you're talking about the metropolitan area of the country here. You're talking about probably a million people within five miles of here. Housing has gone out of sight. You have houses in this town of \$330,000 average.

VDT: What does a young faculty person do?

WESTOFF: Fortunately, the university has a very extensive setup of houses that they rent at subsidized prices to young faculty, and for permanent faculty they have a mortgage system which is heavily subsidized as well.

VDT: You've chosen to live your life in academia. It sounds like an ideal situation. Every once in a while you would migrate to the outer world and do exciting projects like the Commission and the surveys.

WESTOFF: Well, it is. I think that to do nothing but the university would be pretty dull. You've got

to mix it up. I could probably use a bit more right now. I have a leave of absence coming now in which I'd like to do something new. This is now the third year it's been postponed because of other demands on running the office here and recruiting and miscellaneous fundraising and so on. But I think I'm beginning to see a proverbial light at the next spring semester.

VDT: Where are you going? Do you have plans for that?

WESTOFF: No plans at all, but I'm going to go somewhere. I've got an invitation to go to the Center for Advanced Study in the Behavioral Sciences out in Palo Alto, so I could spend a semester there, but I don't know. I want to get some rest, a change, so I may not do that. I may go to Paris.

VDT: Did you ever live in Paris?

WESTOFF: No. I spent one leave in Mexico City, and I've spent a lot of time in Honolulu at the East-West Center [visiting senior fellow, East-West Population Institute, 1979 and 1981], although not in recent years. And time in Sweden.

VDT: Usually attached to some institution, doing some work?

WESTOFF: Doing some work. I've done a lot of work with the Alan Guttmacher Institute. I got started a study in Europe . . .

VDT: The Alan Guttmacher teenage study.

WESTOFF: That came out of some research that I'd done with Gerard Calot, the director of INED [Institut National d'Etudes Demographiques] in Paris. He and I published a paper that showed that the United States was in a class by itself with respect to the teenage birth rate [Westoff, Calot, and Andrew Foster, "Teenage Pregnancy in Developed Nations: 1971-80," International Family Planning Perspectives, June 1983]. That raised the obvious question of why is it so high in this country and so low in European countries. And that in turn gave rise to the research that we did under the auspices of the Alan Guttmacher Institute, which resulted in publication of that book, Teenage Pregnancy in Industrialized Countries [by Elise F. Jones et al., 1986]. We took a hard look at some countries that we took as case studies, such as Sweden, Netherlands, Britain, France, and Canada. We came up with some, I think, reasonable speculations.

I'm also now returning to that subject. Dick Lincoln has persuaded me to write a paper for the 20th anniversary issue of Family Planning Perspectives. He wants me to write a sort of think piece on why the rate of unintentional pregnancy is so high among young people in America compared to Europe--not just teenagers but up to age 25; we've demonstrated that's the case. He wants me to become a sort of armchair sociologist. It's not a research paper, but just some speculations about what I can give the explanations for. [Published as "Unintended Pregnancy in America and Abroad," Family Planning Perspectives, 20th anniversary issue, November/December 1988.] I don't have a hell of a lot of ideas.

VDT: Well, it's obvious. In Holland teenagers can get contraceptives easily; nobody makes such a fuss.

WESTOFF: It's not making a fuss; that's what makes availability possible to begin with. I think it's a more basic explanation than that.

VDT: The Virginia state legislature has now legislated that sex education must be available in schools and there's tremendous grassroots opposition to that. I had that on my list: Is U.S. teenage fertility and contraceptive practice still a problem? You've more or less answered that, yes--and on to age 25.

Could you say a bit about the influence of the Office of Population Research, the first and still the leading academic center of demography in the U.S.? It's been a great place to work, I'm sure.

WESTOFF: It's been a fascinating place to work. It's in a great university and the Office has enjoyed the full and enthusiastic support of the university, which has been translated into this lovely building that they give to us at no direct charge and a great deal of financial support. And they have recently, this past year, enabled us in effect to have a number of "treaties" to connect with the associated departments, like economics and sociology and the Woodrow Wilson School, which guarantee that we can have a certain number of demographers on the faculty.

VDT: You mean you always had to negotiate with them?

WESTOFF: A lot of it happened accidentally and was through research money. Like, for example, Jane Menken is leaving to go to Penn. Nobody says that that slot has to be occupied by a demographer; there are lots of other constituencies that would like to have that position.

VDT: Wasn't she on the faculty?

WESTOFF: She was on the faculty of the Woodrow Wilson School. We are not a department. We do not make faculty appointments and we have to depend on the interests and resources of the regular departments.

VDT: Is it still true at Princeton that you never get a Ph.D. in demography--you get it in sociology or economics or whatever?

WESTOFF: Until this year, yes. Not in demography; it's called population studies. There's a sort of umbrella now, that there are various routes to a Ph.D. You still have all the traditional routes through the various departments. Now we have a new one, which is straight through a program that we designed which comes close to being a Ph.D. in demography, although we don't call it that.

VDT: Still not. It will be a Ph.D. in population studies?

WESTOFF: That's right.

VDT: Princeton degrees, of course, have been very outstanding in the field--economics with specialty in population, or sociology with population specialty. Will this be equally prestigious? Anything from Princeton is prestigious.

WESTOFF: Well, it's not just that. It's an attempt to satisfy what we think is a demand, particularly from the Third World applicants who are not interested mainly in economic or sociological theory, or somebody like Jane Menken who came out of a biostatistics background, or like German Rodriguez, the statistician. These people were not interested in taking lots of courses in these other fields; they were more interested in the internal discipline of demography. So it gives them an opportunity.

VDT: You said in an interview you had in 1977 that appeared in the Princeton Alumni Weekly ["Can Population Research Change History? Charles Westoff explains how Princeton demographers have influenced population policy," Princeton Alumni Weekly, March 7, 1977] that the Office of Population Research, having been so long in the field, was perhaps having a problem in funding because much of the funding in population is now coming from government and it goes to projects or someone who's working on family planning programs in developing countries. Is that still the case?

WESTOFF: We have been successful in lots of ways and not in one other way. Princeton as a university depends a great deal on the contributions of alumni, annual giving and special campaigns. They raised \$35 million in a special campaign five years ago. We have never, with one recent exception, been able to get the interest of individual alumni in making contributions to the Office.

There are two reasons for this, I think. One is that what we do here is pretty academic and esoteric and theoretical as far as the average person who is interested in supporting his local planned parenthood can see. The second reason, more importantly, is that this is not an undergraduate program. The average person can come to Princeton for four years and hardly be aware of the existence of this place. It's a graduate program, so if they don't make any connection to the Office while they're here as an undergraduate, they don't think of us.

Now, to answer your larger question and to respond to my own statement of 1977, we rely extensively on grants, as do other university population centers, on NIH center grants or individual research grants. I think more than other centers, we have also been successful in the fact that the university has given us quite a bit of money. They allocated one and a quarter million dollars just last year from that campaign.

VDT: For just general support?

WESTOFF: It goes into our endowment so interest accrues, but it's ours. And then we have gotten, as have some other places, foundation grants from Hewlett, Mellon, Rockefeller Foundation supported us for 30 years, Ford Foundation.

VDT: The Ford money has dried up now.

WESTOFF: Yes, but they gave us a big terminal ten-year grant. So we have not been impoverished. As somebody said to me recently, any organization that's doing its job right always needs more money. So we always need more money, but . . .

VDT: You also have several graduate students who are supported by fellowships. Any from the Population Council now? The Frank Notestein fellowship, for instance, where does that come from?

WESTOFF: That comes from the Population Council and that's for a person to come here with that title for one year.

VDT: One of your graduates won the Dorothy Thomas Award at PAA last year, wasn't it?

WESTOFF: We've had somebody winning one of those every year; that or the other one [Mindel Sheps or Irene B. Taeuber award?]. We had one--I can't remember her name, but she had been an undergraduate here and recently got her Ph.D. at Harvard and won that award. The Office has turned out a lot of very successful people in this field. Of course, it has the cumulative product by virtue of having been the first one to get going in this business and just having been around for now 52 years.

VDT: And a continually distinguished reputation.

WESTOFF: Well, it sort of always depends on who you get here. And there's Ansley Coale.

VDT: That leads into my next question. I'm going to be interviewing him tomorrow, but I wanted to ask you who have been some of the leading influences in your career. I presume Clyde Kiser; you mentioned Dorothy Thomas.

WESTOFF: Norman Ryder has had an impact, and Ansley Coale.

VDT: The three of you are a triumvirate. You and Norman Ryder and Ansley Coale have been here longest of those who are here now?

WESTOFF: Yes, I guess. A lot of these younger people have been here for a long time, like Jim Trussell has been here for over a decade, Noreen Goldman eight years probably, Anne Pebley has been here ten years.

VDT: How do you and Norman Ryder work? Does one write something out, then the other?

WESTOFF: We haven't done very much together in the past five years. During the National Fertility Study days, I think that our collaboration was quite successful because we wrote on separate topics. We'd just carve up the subject matter; you do this and I'll do that; you handle fecundability and I'll take contraceptive failure rates. So that rather than trying to work on exactly the same topic and having to argue about emphasis or interpretation, we divided the chapters up. And that worked very well.

VDT: I think you bat things off each other, because your PAA presidential address ["The Yield of the Imperfect: The 1970 National Fertility Study," 1975] was a defense of the National Fertility Study because two years earlier, in 1973, Norman Ryder had criticized the study in his address ["A Critique of the National Fertility Study"].

WESTOFF: Exactly, yes.

VDT: What's it like to work with Ansley Coale? He has had almost his entire career here too--even more so than you, I believe.

WESTOFF: Yes, Ansley's been at Princeton since he started as a freshman. Well, Ansley is a--I described Dorothy Thomas this way; there's a great similarity--is a man who exudes an enormous, almost infectious, enthusiasm for ideas and generates quantities of creative ideas each year. He's really devoted to his work and he gets an enormous intellectual pleasure, kick, out of working on various demographic models. He's in the office all day Saturday; something of a workaholic. He maintains this kind of youthful exuberance for ideas which is, as I say, very contagious. The only problem is we have to listen to his bad jokes.

VDT: What's Jane Menken been like to have around, to work with, although you haven't worked on specific projects together?

WESTOFF: I don't think she and I ever coauthored anything.

VDT: She's more quantitative.

WESTOFF: She's done a lot more work on biostatistical subjects, mathematical models of human reproduction. Although both she and Trussell, who's done similar kinds of things, also have a great receptivity to policy-relevant subjects.

VDT: Could some of that have rubbed off from you? After all, most of your topics have been policy-relevant too.

WESTOFF: No, I think they came to it through their own devices. I think with Trussell it came through--he had done a lot of work at CDC [Centers for Disease Control] as a graduate student. He continues with Bob Hatcher down there to revise that book on contraceptive technology. His academic pedigree is in economics. He's moved out of economics the same way that Ansley has moved out of it, and what remains is the set of quantitative skills that is common to those disciplines. Jane is the same way.

One of the great things about working here is that the place, aside from its physical attractiveness, has always had a very high morale.

VDT: It is a beautiful place. You don't feel yourself too removed from the world's problems? Coming up here I passed through the grimness of Philadelphia, a huge metropolitan area that's trying to rehabilitate and has not yet succeeded. How can you be concerned about the world's problems in this beautiful Princeton?

WESTOFF: Yes, I know. It seems almost like the stereotype of the ivory tower.

VDT: What accomplishments in your career have given you the most satisfaction? You have many; can you choose from them?

WESTOFF: Not really. I guess the Commission was a very satisfying experience because, as I say, we turned out a report that was both well done and I think a useful--not necessarily politically useful--but a useful intellectual contribution to population policymaking. I think that was certainly one of the highlights; I don't know whether the highlight or not.

And I got a great deal of pleasure out of working on the World Fertility Survey, especially in the days when we used to commute to London about every eight weeks, working on that questionnaire.

VDT: Did you go to some of the countries, like Fiji, for instance?

WESTOFF: I guess Panama was the only country I went to in connection with WFS, although we went to meetings in different places. I don't remember now exactly where, nothing exotic--Trinidad a couple of times. Recently with the Demographic and Health Surveys, I've been to both Peru and the Dominican Republic in connection with some experimental work that we were doing.

VDT: The Demographic and Health Surveys seem to be doing extremely well, although there's the tragedy of the death of Bob Lapham [DHS director] halfway through. Did you work with him quite a bit?

WESTOFF: In the beginning of this project I did. I didn't know Bob other than to say hello before we got hooked together in this project and I played a very similar role to that which I had done with

Maurice Kendall and WFS, in helping to formulate the thing and get it off the ground, pointing it in directions that I think have paid off, working intensively and in great detail on that questionnaire. Now I continue as--what's my title?--senior technical adviser to DHS and go down there about once every two to three weeks, spend a couple of days with people, sort of trouble-shooting. Then I've done some writing of papers, helping them pull together a proposal for analysis of a lot of these data that goes into the proposal for the next five years of this project, which goes out next month. That's another thing I'm doing now.

VDT: So the data will continue to be exploited, like the WFS data have been, of which you wrote so much. And you also write some for the National Survey of Family Growth. Do you still do that?

WESTOFF: That's not so organized; that's just using the data set. They put that in the public domain and you get ideas.

VDT: Now let's leap into the PAA.

WESTOFF: Was all this by way of preface?

VDT: No. Besides PAA itself, the purpose of this series of interviews is to learn something of the careers of this very select, non-random sample of people who have floated to the top of PAA. You have to be very special. How does all that come about? Is there some networking that goes on? Those of you who first knew each other at the Princeton Inn, and was that where your first meeting was? Can you remember where your first one was?

WESTOFF: Yes, it wasn't at Princeton. I think I probably hold the world's modern record of attendance at these things, because I think--I'm not quite sure--I've not missed a single PAA meeting since 1949.

VDT: Wow! And the 1949 meeting was indeed at Princeton. That was one of the last ones at Princeton. By then they were beginning to meet elsewhere--Washington . . .

WESTOFF: Chapel Hill; they met there in the early 1950s.

VDT: Chapel Hill was 1951. Of course, you can tell me, like everybody else, that the meetings then were small and intimate; everybody knew everybody else. Who stands out in your recollections of your first meetings?

WESTOFF: Frank Lorimer, Frank Notestein [pointing to old photos on the wall].

VDT: These are wonderful photos.

WESTOFF: Kingsley Davis up there; he was at Princeton too. This one--I think the PAA used to be small enough that the Notesteins had them out to a party in their backyard.

VDT: I hadn't heard about that. You all met and stayed at the Princeton Inn. Of course, you lived here.

WESTOFF: I didn't live here then [early 1950s]. I came from Philadelphia where I was a graduate

student.

VDT: And you have not missed one since 1949? Fantastic! You must hold the record. You should get a prize.

WESTOFF: Maybe for lack of imagination.

VDT: How have the meetings changed? Do you regret the growth?

WESTOFF: No, no, not at all. Of course, you reach a certain age. There was a point--I suppose the optimal point would have been 20 years ago or so, where you knew nearly everybody. Now you get to the point where they keep bringing up reserves, these young people and a lot of students, and you couldn't possibly know them. You're looking at them, trying to distinguish . . .

VDT: In 1969--that's nearly 20 years ago--in Atlantic City, there were 486 people registered at the meeting. You probably knew them all.

WESTOFF: As a matter of fact, I was chairman of the local arrangements committee that year. I arranged for the hotel in Atlantic City. I remember going down there in advance to get that hotel.

VDT: By then it was a three-day meeting. Can you remember that particular one? Why were you chairman of the local arrangements? Well, it was because you were relatively nearby, in Princeton.

WESTOFF: They couldn't do it in Princeton anymore because it was too small and Atlantic City was the--Princeton was still the closest to Atlantic City.

VDT: The 1969 meeting must have been the last one in Atlantic City [second and last]. There was a famous one there in 1942, the last one till 1946, because of the war. What did you do for entertainment in Atlantic City?

WESTOFF: There were no casinos then.

VDT: So it was quite pure; you could walk intellectually on the beach.

WESTOFF: On the boardwalk. I don't remember.

VDT: At the 1975 meeting in Seattle when you were president, I think that was one of the last times we combined the presidential speech with the banquet. That year we went out to that Indian island.

WESTOFF: That was my great contribution to the Population Association; I was responsible for that. I made two changes. The first was to get rid of the banquet.

VDT: We went on a boat trip. You didn't consider that a banquet? When we got there we ate well, as I recall [salmon bake]. Did you give your address at that time? No.

WESTOFF: That was the first time the presidential address was given at 5 o'clock, followed by a cocktail party, and not at a banquet where only half the people show up because the price is too high or the food too awful and waiters are running around tuning glasses, making noise, everybody is sleepy, a

lot of alcohol. That was a bad idea. So that, I think, was a major innovation.

The other change I introduced was a rule that a person could appear only once on the program. Damn good idea. But that has been so compromised now that it's no longer recognized. There are coauthorships and you can only be in one role [present one paper?], but you can be a discussant, you can be a chairman, you can be six other things, and I think it's a mistake. [1991 program also allows at least one person to present two papers, as senior author of one, sole author of the other.]

VDT: Why a mistake?

WESTOFF: Because it blocks opportunity for other people to be on the program. The counter-argument is that why should you penalize somebody who's just a junior coauthor? What difference does it make that their name is on that program or not? It's only for publication that it counts for their career. Anyway, those are the two things I did.

We also had some issues which are not worth trying to reconstruct or recollect now, having to do with the business office and where it was going to be located, whether we would have our own or continue with the American Statistical Association.

VDT: Right. The American Statistical Association took over PAA's business affairs about 1967, when they were getting too much for the secretary-treasurer. There was some talk about your time about reconsidering whether they should continue. Their price went up about that time, was that it?

WESTOFF: I don't remember.

VDT: So you don't regret that the meeting attendance has exploded to about 1,100 and you can't possibly know everybody and we have seven or eight overlapping sessions?

WESTOFF: Pretty soon we'll start splintering up into sub-sections and they'll begin to have their own national meetings. On the other hand, I don't think the Association is growing very rapidly now, is it?

VDT: No, it seems to be steady at about 2,600; we lose some, we gain some [2,679 at the end of 1989; 2,752 at the end of 1990]. That's small compared to other professional organizations. It's still a small discipline.

WESTOFF: It's always demography, I think. I hope it does stay sort of small. I suppose growth is a sign of health. It is still expanding in all kinds of directions; now we have a market version they call "demographics." That shows some signs of taking off.

VDT: Indeed, it does. But you think there's still room for the academics?

WESTOFF: I don't like to see any major changes in that respect, no. The population centers are by and large pretty healthy places, intellectually, a lot of good people, a lot of good young people coming up. I think the academic group is in a very healthy state.

VDT: What are you doing now? You mentioned that you're writing a paper on unwanted fertility.

WESTOFF: You don't have enough tape for me to tell you what I'm doing. I've got this National Academy paper I'm doing on the demographic potential of improvements in contraceptive technology in the Third World. I've got the paper I'm doing for Dick Lincoln [[Family Planning Perspectives](#)] on

high levels of unintentional pregnancy among American youth and why they're different from European. I'm writing a monograph on the experimental work that Noreen Goldman and a colleague at Westinghouse--a fellow that was here today, Moreno [Lorenzo Moreno-Navarro], a Mexican who works at DHS--we have been collaborating on an experimental study of fertility in Peru and the Dominican Republic and I have to write up a report for that.

I've got an NIH center grant proposal for renewal for the next five years. That's due on October 1 and that takes several months' preparation and I dread the prospect of having to do that this summer. Then we have another NIH proposal that we had to put in for a continuation grant on the Dominican Republic part of this study. Then I'm giving a paper at an IIASA [International Institute for Applied Systems Analysis] conference in Hungary this October on the future of reproduction in Europe. I've started drafting that. And I'm just finishing a paper on "Is the KAP-gap real?", based on DHS data. And I just finished another paper, which has to be edited yet, on unmet needs in contraception in Latin America. So that's it.

VDT: And you supervise students too?

WESTOFF: Students and all the rest of the staff. Spend much of my time recruiting faculty.

VDT: How do you get it all done?

WESTOFF: You don't sleep much. You learn to do things efficiently and you learn to get help from other people. If you can't delegate some of this stuff, you're dead.

VDT: And you manage to generate all these ideas decade after decade. What about the future, are you going to keep on at this pace? You mentioned taking a semester off.

WESTOFF: Well, that's one of the things that keeps you going. Ansley Coale would certainly subscribe to this tennis; that is sort of sacred time. You don't have to lunch with people; you go out and play tennis. I played today. Sometimes it means a long lunch hour. I got in at 7:30 this morning. And you work late.

VDT: You got in at 7:30; is that normal?

WESTOFF: No, it's not normal, but because of all these things I've got to do the anxiety has apparently picked up, so I wake up early and can't get back to sleep. So I get up and try to use the time productively.

VDT: But you're not riding a bicycle to work like Ansley Coale. Do you walk?

WESTOFF: No, I drive.

VDT: Well, my last question--I promise. Have you thought of writing your autobiography someday?

WESTOFF: No.

VDT: Can you at least write a long paper? Like Frank Notestein's last paper ["Demography in the United States: A Partial Account of the Development of the Field"], which appeared in Population and Development Review in the December 1982 issue, and he died the following February. It covered the

field and is a wonderful history.

WESTOFF: He had written that for the Woodrow Wilson School, actually, at the dean's request, as a sort of life history of the Office and its contributions.

VDT: It covered more than that. It pretty well covered the history of demography in the U.S. too. But there are other stories to be told, and I think you have been sitting in the middle of them and can tell them too--about some of the people.

WESTOFF: Ansley's closer to that point than I am, I think. You can get him to do that.

VDT: One last thing. Are you depressed by the fact that growth rates are not coming down faster in the developing world? The news release with the latest [1988] Population Reference Bureau World Population Data Sheet featured the stall in the world's population growth rate decline. That was picked up by the media. World population growth is trending closer to the UN high variant than the medium variant at the moment.

WESTOFF: Well, you can look at it two ways. I look at what has been an incredibly rapid decline of fertility in Latin America, although it has stalled in a few places--Costa Rica is one of the most widely cited examples of the stall. And you look at some of the Asian countries with rapid declines even before then; you're now even beginning to get some concerns about too low fertility, in places like Singapore. And then, of course, there's the China story. So where is one depressed?

One can be depressed about some of the classic cases, India, Pakistan, Bangladesh--although things are happening in Bangladesh. And then mainly the Arab countries and sub-Saharan Africa--the Middle East and Africa. There the problems may be more intractable.

But I'm optimistic about it, just on the general principle--which, I suppose, is not a particularly sensible basis--which is that we were all pretty pessimistic 20 years ago, 30 years ago, about this stuff, and look what's happened. Africa is going to be tougher, I think. But once it gets going, I think it's going to go like wildfire.

VDT: That's a good note to end on. Thank you.

SIDNEY GOLDSTEIN

PAA President in 1975-76 (No. 39). Interview with Jean van der Tak at the Population Studies and Training Center, Brown University, Providence, Rhode Island, December 14, 1989.

CAREER HIGHLIGHTS: Sidney Goldstein was born and grew up in New London, Connecticut, and attended the University of Connecticut, where he obtained his B.A. in 1949 and M.A. in 1951, both in sociology. He received his Ph.D. in sociology from the University of Pennsylvania in June 1953 at the same time that Alice Goldstein received her B.A. (from Connecticut College) and they were married. He was an instructor in sociology at the University of Pennsylvania from 1953 to 1955. Since 1955 he has been at Brown University, where he has been, variously, Professor of Sociology (since 1960), Chair of the Department of Sociology (1963-70), George Hazard Crooker University Professor (since 1977), and Director (1966-89) of the Population Studies and Training Center, which he founded. He has been demographic adviser to or a fellow at many institutions, including Chulalongkorn University in Bangkok, Thailand, the Hebrew University, Jerusalem, the Council of Jewish Federations, Australian National University, the East-West Population Institute, the UN Economic and Social Commission for Asia and the Far East, the IUSSP, U.S. Bureau of the Census, the National Center for Health Statistics, the National Research Council, the Smithsonian, and the Rand Corporation.

Sidney Goldstein is famous in the field of demography for his research and publications on internal migration and urbanization in Rhode Island, the U.S., and internationally, particularly in Thailand, Southeast Asia, and China. He has also published extensively on the American Jewish population. He is author or coauthor (increasingly with Alice Goldstein) of close to 40 books or monographs and some 170 journal articles. He is also particularly well known in the demographic world for his teaching and what has been called his "pastoral care" of his students throughout their careers (John and Pat Caldwell, Limiting Population Growth and the Ford Foundation Contribution, 1986, p. 124).

VDT: Dr. Goldstein returned just three days ago from China and at the end of next week he is beginning a sabbatical leave. So I thank you very much, Sid, for making time for this interview during this particularly busy week. Today is also an exam day and he must check periodically on his students. How many people are writing the exam?

GOLDSTEIN: This is a graduate exam in population techniques; only nine students, our entering group in demography.

VDT: Later I will also be interviewing Alice Goldstein, because she has shared much of Dr. Goldstein's career and has her own career in demography too. That makes them a rare, distinguished couple in U.S. demography.

How and when did you first become interested in demography, Sid?

GOLDSTEIN: Actually, it was as an undergraduate at the University of Connecticut. I went there intending initially to major in foreign languages. I spent a considerable time studying French and German and then taking social science courses, both out of interest and distribution requirements. I found sociology interesting, but was turned off a little by the general courses that dealt more with theoretical issues, had a lot of jargon in them. When I came across the course in population and

enrolled in it, it attracted me very much, both because of its more empirical character and because it seemed to confront more directly many of the concerns of society. In fact, I was enticed so much that I decided to major in sociology rather than foreign languages.

VDT: The population course led you into sociology?

GOLDSTEIN: Right, because that's where population was housed. This was fairly late in my undergraduate program of studies. I realized I couldn't do much with it as an undergraduate but I was determined already to go on to graduate school.

VDT: Who taught that first course?

GOLDSTEIN: The course was not taught by a demographer but by a sociologist named Otto Dahke. But it was taught well and, as I said, the subject attracted me greatly.

Just about that time, Bob Burnright had come to the University of Connecticut to do work in rural sociology and be responsible for future teaching in population. He had come from the University of Pennsylvania, where he had studied under Dorothy Thomas. This was a particularly welcome development for me because it meant I could think of going on in graduate studies, working with someone who was well qualified in the field of demography. Since I had had only one undergraduate course in population, I decided at that point to stay on at the University of Connecticut for my master's degree, working with Bob Burnright.

VDT: Was that particularly in migration already, or just general demography?

GOLDSTEIN: I guess you could say I caught Burnright's interest in migration, because when it came to writing a master's thesis I was encouraged to write it in the field of migration. This was in the early 1950s, a time when the first sets of special tabulations on migration from the 1940 census became available. That was the first census that had the question on "Where were you living five years ago?" Those data sets were being made available particularly, if I remember correctly, as the result of some work that Don Bogue had done to arrange for special tabulations for a number of socioeconomic regions. And Bob, in his role as a member of rural sociology, had taken on responsibility for working on the New England region. He made available to me the material on Maine. He himself was working on Connecticut; other New England institutions were working on the other states.

I wrote my master's thesis on internal migration in Maine, a detailed analysis of interregional movement. It was always a joke as to whether there were more people or cows in Maine.

VDT: There can't be many people who migrate within Maine; it could well be more cows. That 1940 census was famous, and it was always felt that it wasn't exploited as much as it should have been, because of the war.

GOLDSTEIN: Right. And this was unique in that it was the first time the census had really asked a direct question on migration, so it was an opportunity to explore how useful that kind of question was as well as to get insights into migration patterns.

VDT: Was that thesis published--an article or something?

GOLDSTEIN: No. I wrote it as a kind of useful exercise. At that time I didn't have time to exploit it further because I was too busy preparing for getting into graduate school for Ph.D. work.

Bob steered me in the direction of Pennsylvania. He was a wonderful teacher and a very close friend. His death, a little over a year ago, was a great loss. I applied to Penn along with some other schools, but my heart was set on Pennsylvania, because I'd heard so much about his work with Dorothy Thomas and also knowing Penn's concentration on the field of migration.

I applied for a Harrison fellowship at Pennsylvania. One of the requirements for that was that one passed the language requirements before one enrolled at the university. As I recall, the motive was to make sure the individual was free to concentrate on your major field of study and not diverted into meeting the language requirement. So it meant on very short notice that I had to take time off to refresh myself in foreign languages. It was a real stroke of luck that having studied French and German the first few years at Connecticut, I didn't have to start from scratch. But it did mean taking time out and that's one reason I didn't do much more with getting publications out of my thesis. Fortunately, I did pass those foreign language exams. I received the Harrison fellowship and was able to go to Penn in 1951.

VDT: And you went through your Ph.D. program in just two years. You got the degree in 1953.

GOLDSTEIN: Was it that soon?

VDT: Yes. And Charlie Westoff and Richard Easterlin got Ph.D.s at Penn the same year.

GOLDSTEIN: Right. I still have movies of that commencement, in which the three of us are marching together down the line.

VDT: You marched together a long way in the same field.

GOLDSTEIN: I always thought that was symbolic. I've often thought back to that commencement, the three of us being together. And a number of years later, the three of us were presidents of PAA almost consecutively [Westoff, 1974-75; Goldstein, 1976-77; Easterlin, 1978].

VDT: Only Evelyn Kitagawa came in between [1977].

GOLDSTEIN: Right. You did your homework.

VDT: Well, I have interviewed everybody but Evelyn in this distinguished sample of [living] PAA presidents.

GOLDSTEIN: Charlie was first; I followed Charlie; and then Dick followed Evelyn.

VDT: That was really interesting.

GOLDSTEIN: I think it's strong testimony to the quality of the program at Penn.

VDT: Tell me a bit about it. I've heard a lot about Dorothy Thomas in particular. Did you work directly with her, or who was your professor-mentor?

GOLDSTEIN: I worked most closely with Dorothy. Pennsylvania was a wonderful experience again. I don't know "who" was looking after me, but I was very fortunate in arriving at Penn just as they had received a grant from the Ford Foundation under a program which--again by coincidence--was

initiated by Barney [Bernard] Berelson when he was at the Ford Foundation, not in his role as a population expert but rather as a social science expert. He had developed the idea that what was needed in graduate training was more interdisciplinary interaction. So grants were given to Pennsylvania, Michigan, and a third institution, which slips my mind for the moment, to encourage graduate programs to develop interdisciplinary training.

VDT: Not necessarily in population?

GOLDSTEIN: No, it had nothing to do with population--in the behavioral sciences. At Penn, it involved sociology, in which demography was housed, and anthropology, economics, and history were also brought in.

Penn, as its effort in using the Ford grant, had developed an interdisciplinary seminar, which brought faculty members together from these different fields. And the idea was also to select graduate students from these different fields and have them concentrate a good part of their studies in this seminar, which was called the Interdisciplinary Seminar on Technological Change and Social Adjustment. When I arrived, I was invited to be one of the sociologists that participated in that seminar. What it meant was that one forewent a number of the traditional courses in one's field. One still took a limited number of them, such as sociological methods and theory.

VDT: Did everybody participate--people like Easterlin, who was actually in economics?

GOLDSTEIN: Neither Westoff nor Easterlin was involved in this. Easterlin was in economics and worked most closely with Simon Kuznets and some with Dorothy Thomas. Westoff went the traditional route in sociology, because he had arrived there earlier than I had.

VDT: Did you sit around daily?

GOLDSTEIN: The students and faculty met regularly as a weekly seminar, but we interacted more often in planning the study and analysis and by sharing the same facility.

The unique thing about the seminar was that it was organized very heavily around research projects, which was something new in graduate training, at least at Pennsylvania. And they had selected Norristown, Pennsylvania, which was a relatively small community outside Philadelphia, as the study community. All students and faculty in the seminar were supposed to develop their research projects based on Norristown, some of them being historical, some sociological, anthropological--demographic in my case. Then in the seminar we not only dealt with the more theoretical and methodological issues, but each of the disciplines had the opportunity to present their own ideas, initially their research designs, and as the data came in, discussing the actual results and trying to look at them from the perspectives of the different disciplines. So in that way, I learned not only demography but also how an economist or an historian would look at my set of data. It was a fantastic experience.

VDT: And it became your dissertation.

GOLDSTEIN: Not only the dissertation, but it influenced my whole outlook on demography and social sciences up until this day.

VDT: In what way?

GOLDSTEIN: In the sense that I came to appreciate the need for a multidisciplinary approach to

understanding demographic problems and that it was quite artificial to even house demography in a single disciplinary department, such as sociology or economics, and to restrict oneself to approaching the field from the perspectives of just that single discipline. And I think this evidenced in the fact that so many population programs at American universities now, while they may be housed for degree purposes in departments, are housed for research purposes in centers that involve a full range of social sciences and even experts outside of social science departments, in public health, medicine.

VDT: Has that been a conscious effort of yours in the center here at Brown, to draw in people from different disciplines?

GOLDSTEIN: Yes, and I think the cause of it goes back to this experience I had as part of the Norristown study.

VDT: Is that approach still carried on at Penn? Of course, I've interviewed Sam Preston.

GOLDSTEIN: These are people who were not trained at Penn and come from a different professional origin. The Norristown seminar unfortunately came to an end. The reason was, as is all too often the case, that the initial funding given by Ford ran out and the university was not able to support it itself. It was an expensive endeavor in that it involved a number of high-powered faculty members in a single seminar.

As you noted, I was able to complete my degree at Penn in just two years, and the speed with which I was able to do it I attribute in part to the success of the seminar, because I was able to benefit so much from the close work with a number of faculty. Dorothy Thomas was one; Tony Wallace, a distinguished anthropologist, was another; and Tom Cochran, a very distinguished historian. I benefited very much from them and from others through contacts, even though they weren't directly involved in this seminar. Simon Kuznets was a close friend of Dorothy's. He was not working on the Norristown project but Dorothy was working closely with him on other migration projects. So I benefited indirectly from that kind of interaction too. Dorothy was always very generous in giving not only time to students but also sharing all her professional friends with students. There was very little status distinction.

VDT: I've heard often of the famous coffee klatches.

GOLDSTEIN: Right. Students were very often attending klatches and spending evenings, weekends, at her home, discussing research and meeting with friends. The line between classroom time and all the informal mechanisms for learning were very blurred in Dorothy's agenda. That was one of the great things about her.

VDT: She certainly made an impact on you. You dedicated your 1976 PAA presidential address to her ["Facets of Redistribution: Research Challenges and Opportunities," published in Demography, November 1976].

GOLDSTEIN: Right. One of my great regrets was that at that point she was already too ill to attend the Montreal meeting. She died the next year.

VDT: Was your Norristown Ph.D. dissertation topic migration?

GOLDSTEIN: Yes, but migration with an historical tilt. The sort of common theme that ran through

the Norristown seminar was the kind of changes that a community and its population underwent in response to the industrialization that was occurring during the first half of the 20th century, and that was part of my dissertation. What I did--it was both methodological and substantive--was to go back, using city directories, to retrace the movement of population into and out of Norristown over that period of time.

VDT: You traced where people of Norristown went and found them in the directories of other cities?

GOLDSTEIN: Picked samples from city directories, starting in 1910 and all the way through 1950, and then literally followed these people backward and forward through the directories. And if they disappeared going either way, tried to ascertain through linkages with other record systems, such as death or birth or school records, why it was they had disappeared. Through this record linkage, one could re-create the demographic experiences, particularly the mobility experience, of this population, both when they came into and when they left the community, what years, and also what mobility went on while they were within the community.

What that led to was the recognition--and this was the unique contribution of my thesis--that a high percentage of the people who moved into the community in any one period of time constituted a higher percentage of those individuals who moved out of the community at a later point in time, and that a core group remained in the community over prolonged periods. So we jokingly referred--I forget now if I used the terms in my thesis or not--to "homads" and "nomads."

VDT: That's marvelous!

GOLDSTEIN: Maybe it was Tom Cochran who suggested those terms initially. What it was pointing to was the recognition that there was a highly stable segment of the population, the homads, and a highly mobile segment, the nomads, or what later more technically I came to refer to as "repeat migrants." This was a somewhat unique contribution in the sense that the census, to the extent it's based on a question on a single point in time, is not able to re-create the migration history. We do a slightly better job now by linking place-of-birth data with the five-year question and so on, but for the most part we don't get a complete migration history. But by being able to trace these people over this longer period of time, we were able to get a much better insight into the dynamics of the migration process.

VDT: Has a multidisciplinary project of the Norristown sort been used elsewhere?

GOLDSTEIN: Well, the program funded by Ford took a slightly different form and, in fact, has been longer-lasting at Michigan. I think I'm correct in saying that Michigan used its Ford resources to establish the Detroit Area Study.

VDT: That's a famous project; I didn't realize it came from the same source.

GOLDSTEIN: You can see some similarities. It's unfortunate that the University of Pennsylvania didn't continue it the same way as Michigan.

VDT: That was an apprenticeship, hands-on, way of learning your discipline.

What about your other colleagues at Penn at the time? It sounds like you were a little removed from the other people in demography.

GOLDSTEIN: Right, to some extent it did have that kind of isolating effect. I was not as much involved in the department of sociology, where population was concentrated, and more of my learning of population was by working in Norristown and with Dorothy Thomas. But I certainly had close relations with Ed Hutchinson--the other Penn faculty member in population--and other people at Penn. Everett and Anne Lee were there, as was Ann Miller, working closely with Dorothy on the large census analysis of migration. So there were many opportunities to learn outside of the immediate classroom.

VDT: And you stayed on for two years teaching there.

GOLDSTEIN: I stayed on for two reasons. One was a strong desire to write up the results of the Norristown experience. So I not only published my dissertation, which came out as Patterns of Mobility, 1910-1950 [University of Pennsylvania Press, 1958], but I was also responsible for editing a volume called The Norristown Study: An Experiment in Interdisciplinary Research Training [1961]. Part of my responsibilities during the two years I spent at Penn after my degree was pulling together the materials for that particular volume, which consisted of contributions by a number of students who had been a part of the seminar, most of whom based their dissertations on the Norristown data. At the same time, I was teaching in the department of sociology, and in those years teaching was a heavy load.

VDT: How many hours?

GOLDSTEIN: Full-time teachers, instructors at least, at that time taught six courses, each consisting of two hours per week, so that's 12 hours.

VDT: Phew!

GOLDSTEIN: Fortunately, I didn't teach the full six, since part of my time was devoted to the Norristown study. But I keep reminding our young people today that they have it comparatively easy!

VDT: Indeed. And you were married then, you and Alice.

GOLDSTEIN: By 1953, yes, we were already married and our first child was born in January 1955, our daughter Beth, who is now an assistant professor herself. She's at the University of Kentucky.

VDT: My mother graduated from the University of Kentucky.

GOLDSTEIN: I guess she's an example of how our own professional careers, mine and Alice's, very much influenced the interests of at least two of our three children.

VDT: Is she in sociology?

GOLDSTEIN: No. She did her undergraduate work in anthropology at Yale and then became what Yale refers to as a "bachelor in China." She was stationed in Hong Kong because that was just before the United States renewed diplomatic relations with China. She spent three years teaching at the Chinese University in Hong Kong and in the process learned Chinese--Guangdonese, actually. Then she did her graduate studies at the University of Wisconsin, where she combined her interest in anthropology with interests in education and Southeast Asia and wrote her dissertation on the integration of the Hmong refugees into the Madison community, particularly into the school system of

Madison.

VDT: Interesting! Well, I'll learn more about these brilliant children. It's obviously a very influential family.

GOLDSTEIN: One thing leads to another.

VDT: Through Bob Burnright and Dorothy Thomas to you, on to your wife, your children.

GOLDSTEIN: During the time I was teaching at Penn, there was a thorough reevaluation of many of the programs at Penn. One of the great concerns was that some of the departments had become quite incestuous, in that a high proportion of their faculty were made up of their own alumni. That was certainly true of sociology at Pennsylvania at that time. I had already made up my mind that after some reasonable number of years I should look elsewhere. And Dorothy, knowing of my interests, called my attention to the opportunity that had opened at Brown. It was early 1955 when she received a letter from the chair of the department announcing a vacancy in this new program that was being developed in population studies, within their department of sociology.

VDT: Which had been founded by Vincent Whitney in 1949.

GOLDSTEIN: Well, sociology has a strange history at Brown. It began early in the 20th century and was revived in the early 1950s, after Vince came to Brown. I applied for the position. Fortunately, I had good recommendations from people at Penn and from Henry Shryock, who had liked my dissertation.

VDT: Henry had been on your dissertation committee? He was at the Census Bureau then.

GOLDSTEIN: No, Dorothy had sent him a copy of my dissertation.

VDT: I saw him yesterday. He was so interested to know I was coming up here today.

GOLDSTEIN: He's a very old friend. One of the first talks I gave after getting my Ph.D. was in Washington at the invitation of Henry. It was before the Washington branch of the American Statistical Association, reporting on the methodology in my Ph.D. research. Another early talk--which again illustrates how Dorothy was very supportive--while I was still a graduate student, was a presentation at the PAA meetings, which in those days were largely at Princeton. I think that was 1952.

VDT: 1952 was in Princeton; that was almost the last Princeton meeting.

GOLDSTEIN: Yes, but up until that time I think the practice was to alternate Princeton with another city. Then they started drifting away and didn't get back there till 1955. I guess they haven't been back there since.

VDT: No. They outgrew it. 1952 was your first paper at PAA? ["Problems in the Migration History and Social Demography of a Moderate-Size City"]

GOLDSTEIN: Right. Obviously, as a graduate student that was both a thrilling and a terrifying experience, because in those years the whole PAA membership literally met in one room; there were

no multiple sessions. So I knew I had the most distinguished American demographers in the audience. I still remember, and I've always been very grateful to Ron Freedman, who was the discussant of my paper, that he didn't tear it apart, because that would have been devastating for a graduate student. So that was a wonderful opportunity and one of my first professional contacts with PAA, in the sense of actual interaction, aside from being on the membership rolls.

VDT: So then you went to Brown, where Kurt Mayer was chair of the department?

GOLDSTEIN: Kurt was acting chair when I was interviewed for the position. Vince Whitney was the actual chair, but he was on leave when I was interviewed.

Brown, as I said, has a somewhat strange history as far as sociology goes. It had one of the early departments in the country. Lester Ward, one of the founding fathers in American sociology, came to Brown in the early 1900s. And the first two presidents of the American Sociological Association were here at Brown--Lester Ward and Dealey. And the first meetings of the American Sociological Association were held here in Providence. So there's a long history of sociology at Brown.

But in the 1930s, there was something of a catastrophe. Dealey, who was a follower of the French sociologist, Auguste Comte, believed that sociology was the queen of the sciences and he argued that because that was the case, the sociology department should have a kind of review function of decisions being made at the university.

VDT: In any field?

GOLDSTEIN: Right--at least in the social sciences. That recommendation didn't go over big with the university and one result was that sociology as a separate program was done away with and although individual sociologists remained on the faculty, they came to be housed in other departments.

Then after World War II, in the late 1940s, the decision was made to reactivate sociology. And at that time the decision was evidently also made that they would concentrate it heavily on population studies. It's not clear exactly why. One explanation is that population studies, being much more empirical, would be less likely to come up with the kind of suggestions Dealey made back in the 1930s. The other, which is probably closer to the truth, is that after looking at the programs of other universities with which Brown saw itself competing, they realized that population was one area that wasn't covered very thoroughly and that this was a field in which Brown could therefore make an important contribution. There weren't programs at Yale or Harvard, for example, or Dartmouth--the other Ivy Leagues in the New England area.

So that was when Vince Whitney was brought here. He came in 1949, as I remember, as a young assistant professor, but with the responsibility for developing the new department of sociology with this focus on population and urban studies. Then in the next few years, he brought in several people--Kurt Mayer, Leo Schnore, and one or two sociologists. In 1955 a new position was created; that was the one I was recruited for. There were the four of us [in population] in the late 1950s.

We brought in our first graduate students; there were a few already when I arrived. The first Ph.D. was awarded in 1958, to a Japanese student, Shigemi Kono, who has had a very distinguished career. He's now director of the Population Institute [Institute of Population Problems] at the Ministry of Health in Tokyo. Two years ago Brown brought him back to campus and gave him its Distinguished Graduate Alumni Award. It was the same year in which our program awarded its 100th Ph.D. with specialization in demography. Between 1958 and 1987, we actually awarded 100 Ph.D.s.

VDT: And the focus was mainly migration? You were brought in because you were a migration,

urbanization person? Was that one reason you got the job?

GOLDSTEIN: Actually, I know some of the other candidates--they'll remain nameless--with whom I was competing and they were not migration experts, so I don't think that was an actual requirement of the job, although it was an area, direction, in which they were interested. Whitney obviously was interested in that. Leo Schnore also, at least in the urban side of the coin, not so much in migration itself.

VDT: Your first studies with Kurt Mayer were Rhode Island?

GOLDSTEIN: Right. When I arrived there was some interest in doing a series of studies on Rhode Island, partly as a kind of service to the larger community.

VDT: Did you get funding as a result of its being that service?

GOLDSTEIN: Well, the state had been undergoing some serious economic problems. There had been quite dramatic changes with the decline in the textile industry and its movement to the South. So there was considerable interest in how these impacted on the population. A development planning office had been set up in the state and they asked the university to undertake some of these studies. Kurt and I worked together for several years doing a series of studies on migration.

VDT: May I ask about Kurt Mayer. He is one of only three people I may not get to interview. Is he still alive?

GOLDSTEIN: Oh, yes. I've maintained close ties with Kurt because we developed a very close friendship while we were together here at Brown. I succeeded him as chairman of the department. But before that we had collaborated on a number of these studies, including one that was somewhat outside the area of demography, on small business [The First Two Years: Problems of Small Business Growth and Survival, 1961]. Even there, however, we applied a number of demographic concepts and tried to show the linkages between small business development and population redistribution.

Kurt left Brown, I think the year would be 1963 or 64 [1966]. Before that, as you know, he served as secretary-treasurer of PAA.

VDT: He did--from 1959 to 1962. And he should be interviewed for this series, but I didn't quite know how to get to him in Switzerland. I have to admit that Nathan Keyfitz was in Jakarta and self-recorded an interview from a questionnaire I sent him. Perhaps I could do the same with him.

GOLDSTEIN: The PAA meetings were held in Providence in 1959, weren't they?

VDT: Yes, you're right.

GOLDSTEIN: I think part of the reason for that was Kurt's strong involvement, also Vince Whitney's, in PAA.

VDT: Did he go back to Switzerland when he left Brown?

GOLDSTEIN: Yes. He was a native of Switzerland and in that period in the 1960s he was offered an endowed chair at Berne University. Unfortunately, he held the position only a short number of years.

In a nutshell what happened was a very considerable student unrest at the university, partly as a byproduct of the unrest on American campuses.

VDT: It happened in Switzerland too?

GOLDSTEIN: Right. And it was concentrated in sociology. And Kurt, having come just a few years before from the United States, students were disrupting his classes, making it very difficult for him to teach. He tried, one year later, to pick up and it didn't work; there was the same unrest. So as a result of that he took early retirement.

VDT: I never heard of anyone being hounded out. I know the department of demography at Berkeley had a lot of problems in that period, but . . . Isn't that sad!

GOLDSTEIN: He left Berne and went to Ascona, Switzerland, where his family had a home and he's lived there ever since. He's built himself a lovely home on a lake. He's not withdrawn completely from the field of population. He doesn't teach, but he has done research and writing from time to time at a leisurely pace. I've visited him there a couple of times and we look forward to seeing him again next spring when we spend time at the Rockefeller study center in Bellagio, Italy. It's not too far from where he lives.

VDT: Well, you'll tell him about this interview and that he should have been interviewed and that I'll somehow get hold of him for that someday.

GOLDSTEIN: I'm sure if you send him a number of questions.

VDT: I'd like to do that if you'll give me his address. [See Mayer's "self-interview" of April 1990, above.]

In the early 1960s, you took a year [1961-62] in Denmark, as a Fulbright scholar.

GOLDSTEIN: Yes, that was my first sabbatical at Brown and first time overseas.

It was a very exciting experience, coming to Brown and being involved in the development of the program. The first six years went by very rapidly; they were very satisfying years. As I said, our first Ph.D. came in 1958 and the momentum picked up.

VDT: Did most students go into migration?

GOLDSTEIN: No. People have this illusion that at Brown everyone does their work on migration. A large number do, but a number of our graduate students work on fertility and mortality and all the traditional areas of population. Ed Stockwell, for example, who reworked that Shryock and Siegel textbook, Methods and Materials of Demography.

VDT: What do you mean "reworked" it?

GOLDSTEIN: He converted the two volumes into one. He did that while he was at the Bureau of the Census. He wrote his dissertation on infant mortality in Rhode Island. That's an example of someone outside the field of migration, and there have been many such individuals among the 100 Ph.D.s and those who terminated at the M.A.

During that period I worked with Kurt on the Rhode Island migration studies. But I'd always had this frustration, growing out of my Ph.D. dissertation, that in order to further explore this whole

question of repeat migration, there was no good set of statistics available on the American scene. As I told you, to do it in Norristown, I had to re-create these life histories from city directories and that was a long, laborious process. So I was always on the lookout for a data set with which I could test some of the ideas I had developed, based on the Norristown research, to ascertain whether in fact this pattern I had found in Norristown--a very high percentage of the out-migrants being former in-migrants--held in other places. And probably again stimulated by contacts with Dorothy, who had worked for a number of years on Swedish material, I was aware of the fact that the Scandinavian countries had population registers. So, as I approached the time for my first sabbatical, I decided I would like to take advantage of some of those rich materials to explore these ideas that had been lingering for several years.

So I went to Denmark for my first sabbatical, supported by a combination of a Fulbright award, a Guggenheim fellowship, and a Social Science Research Council fellowship. Putting the three together, I was able to manage a year of support away from Brown.

VDT: And you had the article on that in the American Statistical Association journal in 1964 ["The Extent of Repeated Migration: An Analysis Based on the Danish Population Register," Journal of the American Statistical Association, December 1964].

GOLDSTEIN: I was affiliated in Copenhagen with the Danish National Institute of Social Research, which was very supportive of my research. I didn't do any teaching. I did give a number of seminars at the Institute and a number of lectures at various institutes around Europe. That's one of the requirements of the Fulbright, but something I very much wanted to do in any case.

VDT: Where did you go, for instance--INED?

GOLDSTEIN: Sweden, where Hyrenius was teaching at Goteborg; I went to Stockholm where the Myrdals were. The Myrdals were old friends of Dorothy Thomas. I'd gotten to know them in Philadelphia before, so I was renewing that friendship. I went to INED; Sauvy had invited me there. I went to Kiel University, where Hilde Wander was professor, working on migration.

VDT: I noticed that you became an IUSSP member about this time, perhaps even before [1960], and obviously you were making early foreign contacts.

GOLDSTEIN: Yes. One other place I went was the London School of Economics, where David Glass and John Hajnal were working. All these contacts and my work in Denmark enhanced my international interests and that's reflected in part in becoming a member of IUSSP.

VDT: Did you set up the Population Studies and Training Center, which was established in 1966, a few years after you came back from Denmark?

GOLDSTEIN: The answer to that is yes and no. The population program was actually initiated by Vince Whitney, back in the early 1950s, and also by Kurt Mayer; they played very important roles in developing the program. What happened in 1965 was that by that time a number of faculty at Brown were interested in population, both in sociology and in a growing number of departments outside of sociology. And having by that time become chair of the joint sociology and anthropology department, I was much more into administration. In my role as chair of the department, I was more or less also responsible for the population program, because that was such an important component of sociology at Brown. Recognizing that there were these interests outside of sociology, I decided the time had come

to establish some more formal organization; we already had informal ties with people outside the department interested in population. So we did get permission from the university to establish the Population Studies and Training Center. This was, in fact, one of the first centers established at Brown. Since then, a large number of other centers have been created.

VDT: In what, for instance?

GOLDSTEIN: Environmental studies is one; another is the program in World Hunger.

VDT: Did you consciously emulate one of the already existing population centers--Chicago, Michigan? Well, Michigan had only just started [1962].

GOLDSTEIN: No. Each place, I think, is unique and we let ours evolve based on the interests at Brown and the nature of our training program and the research interests of the faculty.

VDT: What is unique about Brown?

GOLDSTEIN: For one thing, I think, it's the nature of the university itself. It's a relatively small university, to judge by the number of students. There are only about 6,000 students; that includes both undergraduates and graduate students.

VDT: No wonder it's so exclusive! You have 11,000 applicants for freshman year.

GOLDSTEIN: The undergraduate body runs about 4,500 to 5,000 and the graduate students run somewhere around 1,200 to 1,500. It varies from year to year.

It's been a very intentional decision on the part of the Corporation, which is the name we give to our Board of Trustees, to keep Brown small in order to maintain the character of the university and the quality of the program. Brown does not try to cover the waterfront in terms of all fields or all topics, but rather tries to develop excellence in limited areas. It wasn't until they felt that population had achieved a level of excellence that let it stand on its own two feet that they agreed to incorporate other fields of interest into sociology, such as comparative development. That didn't happen until sometime in the 1960s, so it took almost a decade before we felt we were ready for that.

Given the character of the university, there's very close interaction among faculty in different departments. There's also been close interaction between the faculty and the administration, particularly in earlier years when there was less bureaucracy. Now, given government requirements and all the paperwork in research grants and affirmative action, there's much more bureaucracy. But, even today, it's very simple for me to go directly to the provost or even the president of the university with some problem I want to discuss. And they've been very supportive of the program in population. They see it as one of the jewels in Brown's crown. They're quite determined to maintain it as a strong program. Even in the years when we've had difficulty because of cutbacks in government funding, they've managed to come up with the funds to carry the program through the critical period.

VDT: The Center?

GOLDSTEIN: Right--until we could pick up again on the outside funding.

VDT: You had some Ford money and then you got NIH money.

GOLDSTEIN: Right.

VDT: And then you established the Population Research Lab, in 1966, and that was concentrated on Rhode Island.

GOLDSTEIN: Well, we've always felt that Rhode Island was a somewhat unique location. It's unique in the sense that it's the smallest state in area in the union. It has a population just under one million persons, but it's a very heterogeneous population because of the nature of the state's economy. It's had a series of immigrant groups, starting with the Irish and then the French Canadians, followed by the Italians and East Europeans and, more recently, a number of Asian groups. So it's very heterogeneous. As a result of that, we feel it lends itself well to demographic research, because one can do quite a bit of comparative analysis among these various ethnic groups and different migration cohorts who settled in the state in different periods.

VDT: The surveys that led to the book you did with Al Speare and Bill Frey in 1975, Residential Mobility, Migration, and Metropolitan Change, when you had three samples interviewed in 1967, 68, 69--you followed them annually until 1971--were they unique?

GOLDSTEIN: I would say they were somewhat unique. We referred to them as a population laboratory because what we did was pick a statewide sample of the population in the initial year of the survey and then, in the succeeding rounds of the survey, as far as possible the same individuals were reinterviewed. So it was a longitudinal survey and in that way we were able to measure change over time. At the same time, the survey design was structured in such a way that the representativeness of the sample could be maintained by replacing cases that were lost between the time of the previous survey and the most current survey. In this way, we were getting cross-sectional and longitudinal data at the same time.

It was kind of an omnibus survey--again, I can clearly trace some of the origins to the sort of philosophy of the Norristown experience--in that we had a number of faculty involved, a number of perspectives, not only demography but health and ecological concerns and some historical and economic concerns. And it was used for both research and training purposes. We tried to involve graduate students as much as possible and some undergraduates. A fairly substantial number of Ph.D. dissertations were written based on the data collected in those surveys.

VDT: Did it carry on past 1971?

GOLDSTEIN: Not the full-scale survey, because there comes a point when you lose so much of the original sample that it doesn't make sense to keep the same structure. We've had new surveys, such as the one done by Basil Zimmer, and from time to time, even up to the present, we go back as best we can to the individuals in that original survey. We've sent interviewers scooting across the country to find respondents in the original survey who have left the state since then. There's a possibility of using that sample even for studying problems of the aged, for example. That possibility is being considered at the present time, because we've had fantastic luck in locating the original sample, or at least accounting for it, if they've died, and then locating those who have moved away and reinterviewing them, either by mail or by sending interviewers around.

VDT: And in 1968-69, you turned your attention to something I would think quite different--Thailand. What brought that on? Who approached whom? You were Population Council-sponsored there.

GOLDSTEIN: Up to that point, as you can see from my c.v., I concentrated heavily on the U.S. and

to some extent on Denmark, with a strong interest in migration. But already early in the 1960s, I had been approached by Dudley Kirk and Frank Notestein about an LDC overseas assignment. Vince Whitney, in the meantime, had served on the Population Council when he was on leave from Brown, so there was that channel of communication too. The Population Council was engaged by that time in developing a number of training and research programs in developing countries and concentrating heavily on Asia. So they had asked me on several occasions if I'd be interested in going overseas.

In the interval, given my growing international interests, starting in Denmark, I realized that the main focus of demographic concern overseas was going to be developing countries. So I was very enticed by the possibility of going overseas. I didn't go as early as I had been invited to go, because there was some concern on the part of Alice and myself about taking very young children to places like India.

VDT: Let me tell you that we lived in Thailand from 1958 to 1960--we beat you--with a small child. My husband was with ECAFE [Economic Commission for Asia and the Far East], as it was called then.

GOLDSTEIN: You were braver than I was. So we put it off, but I was determined to go overseas. I had taken some overseas trips in the interval. In 1967 I went to Australia for an IUSSP meeting and for some UN activities in which I had become engaged by that time. This involved working on problems of urbanization, particularly problems of clarifying the definitions of urban-rural populations.

But finally in 1968, my second sabbatical at Brown, we decided to take the plunge and I accepted the invitation of the Council to become the adviser at the newly developing program of population studies at Chulalongkorn University in Bangkok.

VDT: You have said in your works on migration in Thailand that Thailand was unusual among less developed countries because it had focused on migration. Which came first--their interest in migration or did they approach you because of your migration interest or what?

GOLDSTEIN: Migration itself was not a major variable in why I was invited to go to Thailand. They had already been doing some work in that area and, of course, that made it more attractive to me. But it was mainly just a coincidence. The Council needed an adviser at that time, that was my sabbatical year, and we were eager by that point to work in a developing country. It made it easier for me in the sense that there had already been some interest in migration in Thailand. So when I was involved in this work as adviser--and, again, you can see the influence of the Norristown experience--one of the first things my predecessor, Dov Friedlander, and I decided to do at the program at Chulalongkorn was to develop a research project which would serve both as a training mechanism for the students . . .

VDT: The Longitudinal Study of Social, Economic, and Demographic Change in Thailand?

GOLDSTEIN: Right. And also serve as a way of gaining data that could be used by the Institute of Population Studies to provide insights to government policymakers on matters related both to fertility and migration.

VDT: Did you think it must be deliberately related to policy?

GOLDSTEIN: Yes. I felt strongly that in a country like Thailand there should be strong linkages between training and research on the one hand and between those two activities and policymaking on the other. I'm firmly convinced that one of the reasons that Thailand has been so successful in its efforts to control fertility is that there has been this close cooperation between research scholars and government policymakers. A number of committees, in fact, in Thailand are composed of both

researchers and policymakers.

VDT: You mean back and forth, researchers at the universities like Chulalongkorn? Like Visid Prachuabmoh?

GOLDSTEIN: He's a beautiful example. He was director of the Institute, but was also active on a number of government committees. He even was elected a member of the Thai parliament. And growing out of all this, Thailand was one of the first, perhaps even the first, country that had a provision about population control in its constitution.

VDT: I thought Yugoslavia was the first; I know China has one.

GOLDSTEIN: That's why I said it may have been among the first.

VDT: Do you think you had a hand in encouraging that?

GOLDSTEIN: Only indirectly. There were other advisers to the Institute. One who preceded me was Dov Friedlander from Israel, and Ralph Thomlinson and then John Knodel succeeded me. So I certainly can't take sole credit for everything. There were also other Population Council representatives in Thailand in other important roles, at the Ministry of Health, and Jim Fawcett played a very important role as the main representative of the Population Council and coordinator of all the other activities. Allan Rosenfield, now Dean of Public Health at Columbia, was very active in Thailand. So there were a number of very good people. We worked closely together and--I'm immodest--I think we had a good influence on the development of training and research in demography in Thailand and on government policy.

VDT: I always like to say that the Thais, never having been colonized, have no chips on their shoulders about "farangs"--foreigners. Perhaps there could be an openness in the relationships which was almost not possible in other developing countries.

GOLDSTEIN: Amos Hawley, as you may know, was instrumental in getting the Population Council to come into Thailand and in developing the training program at Chulalongkorn University. I consulted quite a bit with him before I made the decision to go to Thailand.

VDT: The Potharam study--his time in Thailand [1964-65] came before yours.

GOLDSTEIN: Right. He brought Jim Fawcett to Thailand to work on the Potharam study.

VDT: You were in Thailand just that one year. How often have you been back? The longitudinal survey went into the field in 1969 and 70. You had left by that time?

GOLDSTEIN: No, I was there for the first round of the longitudinal study and spent quite a bit of time in the field, in fact, on the study. That was the rural phase of it.

VDT: The 1,500 rural households were interviewed in 1969?

GOLDSTEIN: Right. The urban phase was a year later; by that time I had left Thailand. But I have remained involved in research in Thailand up to the present time and returned there a number of times after 1969. There was a period when I was going back at least once or twice a year. I've lost track of

how many times I've been to Thailand. Now a disproportionate amount of my research efforts are devoted to China.

VDT: Did you ever learn Thai?

GOLDSTEIN: Broken Thai. Thai is a very difficult language.

VDT: It is, indeed--tones; too many tones.

GOLDSTEIN: My wife Alice became quite fluent and my daughter became very fluent in Thai.

VDT: She was probably there at a susceptible age. My oldest son's first language was Thai. At age three, he couldn't speak anything but Thai.

GOLDSTEIN: At the Institute they pretty much insist on speaking English with foreigners, so visiting scholars don't get much practice. The same thing happened in Denmark. They use English quite extensively in these institutes.

VDT: Are you the only U.S. demographer or sociologist who has studied internal migration in the Third World? That's still fairly rare. I'm going to get on to how rare migration research is in demography generally, though less so now. Most Americans who have worked with less developed countries have been focused on fertility.

GOLDSTEIN: Certainly I do a disproportionate amount of my work on migration. I certainly can't claim that I'm the only one. There have been a number of other studies; even Amos Hawley has worked on migration.

VDT: He was there only the one year. His interest in the Third World was fairly brief; he said so in his interview.

GOLDSTEIN: But there have been a number of people since then; some are students of our own program.

VDT: There are still Thai and other Third World students at Brown--now, there are Chinese. Through the years, you've had about what--half and half, U.S./Third World?

GOLDSTEIN: It varies from year to year. At the present time in the demography program, it's closer to 60 percent overseas and 40 percent American.

VDT: Third World people still need you for training in demography?

GOLDSTEIN: The ideal situation, of course, is that you train enough of them so that they can go home and establish their own programs and there's less need to come to the U.S. for degree work. I think that's been one of the satisfying experiences about Thailand. They have been turning out a considerable number of master's degree people on their own. Actually, one can now get the Ph.D. in demography in Thailand itself.

VDT: Have they had people complete it?

GOLDSTEIN: I can't honestly answer that question. The program is at an institution called, NIDA, National Institute of Development Administration, and most of the people who get trained there are government people who do it on a part-time basis, so it's been a much slower process. I'm not aware of anyone who has yet received the Ph.D.

VDT: That institute is staffed by some of your graduates?

GOLDSTEIN: Yes, the first director of the institute was a Ph.D. of ours, Suchart Prasithrathsin. The interesting thing is that at the moment the directors of the three major demography training programs in Bangkok are Brown alumni. Suchart is at NIDA. The director of the program at Chulalongkorn is Bhassorn Limanonda. And the director of the program at Mahidol University is Aphichat Chamrathirong. They're all Brown Ph.D.s. There are other Brown Ph.D.s, some on the staff of these institutions, some in government service, and some in other institutions. There may be around ten, but I wouldn't vouchsafe for the exactness of that figure.

VDT: Brown has such a reputation for its strong tie with Thailand, as Michigan does with Taiwan.

In your 1976 presidential address ["Facets of Redistribution: Research Challenges and Opportunities"], which was a summary of your many interests and one of the few migration addresses among those given by PAA presidents, you quoted Dudley Kirk, who said in his 1960 presidential address ["Some Reflections on American Demography in the Nineteen Sixties," Population Studies, October 1960] that internal migration had been the "stepchild" of demography. Speaking in 1976, you said that although attention had been focused on fertility in the last decades, migration might well become the most important branch of demography in the last quarter of the century, because of the tremendous pressure building up in LDCs with rapid urbanization. Of course, in the U.S. there was what looked like a rural renaissance in the 1980s, which changed in the 1980s. Has migration indeed gained in importance in demographic research and teaching?

GOLDSTEIN: I certainly think there's been much more recognition of the importance of problems related to migration and urbanization. It hasn't displaced fertility as the major area of concern. But judging by number of publications, we've made great progress.

BREAK

VDT: We're back again after lunch and after I've interviewed Alice. We were talking about migration. In the U.S., has there been enough interest shown in internal migration? My impression is that the migration turnaround of the 1970s aroused great interest, the fact that rural areas were growing faster than metropolitan areas for probably the first time in U.S. history, but it turned around again in the 1980s, so interest has been lost in internal migration.

GOLDSTEIN: No, I don't think that's the case. If anything, there's probably more interest. I think I was right when I predicted that as fertility largely came under control, migration would take on more importance. It means that, particularly with respect to local areas, much more of the growth--whether a place will grow or decline--is influenced by migration. Given the up and down swings in the economy, localities can be affected at a relatively fast pace.

Take the kind of situation in Texas--oil boom and rapid movement, more recently decline, and now it seems to be rebounding--all this is mirrored very much in the amount of movement in and out of these places. That in turn has tremendous impact on housing markets, the whole local economy. So

it becomes a kind of chain reaction. And as you just mentioned, first the movement out from the cities to the suburbs and nonmetropolitan areas and even from the suburbs to nonmetropolitan areas, and now some evidence that that's turned around again. All these dynamics of change are very much associated with the migration process.

My own recent interest, of course, has focused much more heavily on the developing world and particularly on the interrelations between migration and rapid rates of urban growth in developing countries. The problems associated with rapid urban growth and rising levels of urbanization have moved very much up in the priorities of policymakers and those concerned with the welfare of populations. A number of countries have introduced programs that directly or indirectly are designed to control the pace of urban growth, which means that they're really trying to control the pace of population movement from rural to urban places. These take many forms, from outright control to encouraging people to go elsewhere by developing satellite cities, growth centers, and small towns. This is actually what I'm working on in China.

Another major area is the whole question of the relation between migration and fertility. As countries try to control their fertility, they realize that as people move from rural to urban areas, they may affect the fertility of cities if they bring with them the high fertility that characterizes the population in rural areas. On the other hand, if when people move they reduce their fertility, this may be one more factor that contributes to overall fertility decline in the country, especially if a high percentage of the population is urbanizing. Right now in China, with the one-child-per-family policy, there's great concern about whether individuals who are what they refer to as the "floating population," or what I call "temporary migrants"--that is, persons who are moving around the country--are using migration to escape the controls of the system, by living in places where they aren't officially registered and, while there, having more children than the government policy allows.

Still another major problem, which again I can trace back directly to my work in Norristown, is the whole question of what we refer to as "circular mobility." In a number of developing countries, there's strong evidence of a fairly substantial movement back and forth between rural and urban places. This is a form of internal migration--I actually alluded to it in my presidential address--that has not been adequately covered in our statistics or even in our migration theory. Yet, the more research that I and others have done on this topic, we find that numerically in many places--right now in China, again--this kind of movement probably exceeds the more standard forms of mobility that show up in response to standard migration questions in the census and surveys. But these people are very different in terms of their characteristics. They're very different in terms of the kind of impact they have on both their origins and destinations, because they're in both these locations only on a part-time basis, because they are making frequent moves back and forth. It has both positive and negative aspects. It may not place as much strain on the facilities of the cities. On the other hand, it may be very disruptive of family life if they leave spouses and children behind. But it can also have a positive impact on rural development if, while working in the city, they send substantial remittances or bring money back with them that is then used in the development process. So, again growing out of that interest that first arose in Norristown as a result of my identification of the extensive repeat migration, reinforced later by the work I did in Denmark, a good part of my work now in developing countries is on this process of circulation.

VDT: Let's skip on to China. Alice said you first went in 1979. You first went alone?

GOLDSTEIN: Right, 1979 was my first trip to China. That trip arose as a result of my service as chair of the IUSSP Committee on Urbanization. We were doing a series of comparative studies of a number of developed and developing countries, both capitalist and socialist countries, and we wanted very much to include China in the comparison. That was before the United States had re-established

diplomatic relations with China, so I'd had great trouble establishing contact with any scholars in China. I did so finally, but I couldn't elicit any cooperation from them, probably for political reasons. The period in which I was trying to establish contacts was the tail end of the Cultural Revolution, so things were very unstable in China.

But no sooner had diplomatic relations been re-established between the U.S. and China than I began getting very favorable reactions in reply to my earlier inquiries, saying they wanted to cooperate. They knew from my letters that I had been working in Southeast Asia, so they asked me to come to visit China the next time I was in Asia. And I, of course, grabbed the opportunity. I think--I can't claim this absolutely but I'm told, by the Chinese--that I was either the first or among the very first American demographers to have come in on an official visit and actually dealt professionally with Chinese demographers. Others had been there before, but they came more as tourists. In fact, a group from the PAA that I helped arrange during my presidency went there, but they went as tourists, not as professionals.

So I went in the fall of 1979. It was mainly to meet with small groups of sociologists and so-called demographers--so-called, because these were persons not trained as demographers, but assigned to help develop the field; they really hadn't re-established themselves yet. They were mainly interested in what had been going on in the outside world during the whole time that China was so isolated.

We hit it off quite well, and before I left China, they invited me to come back for a more leisurely visit to give lectures on urbanization and migration in Southeast Asia. They were interested in learning what was happening in the profession with respect to methodological concerns, as well as in substantive areas and policy issues.

So in 1981 I obtained a fellowship from the Committee on Scholarly Communication with the People's Republic of China, the CSC-PRC, sponsored by our National Research Council, and Alice and I went back for a month of lectures under the local sponsorship of the Chinese Academy of Social Sciences. That was very educational for me and, I hope, helpful to the Chinese. We traveled across a good bit of China giving lectures in various centers, academies of social science and universities. I gave them a choice of six topics and they selected the ones that they thought would be of most interest to them.

The interesting thing was that I was mainly interested in questions of urbanization and migration in China. They indicated to me that they didn't have any problems in those areas because they had everything under control through their registration system, which requires that people get official permission to move and settle in particular locations. They said, "We're controlling our urban growth; we're controlling our migration; there's no problem in these areas in China." As I traveled around China, I noticed that there was considerable mobility of people that wasn't covered by the system. I incorporated these observations into my lectures and by the time I finished the lecture series, the sponsors said to me, "Maybe you saw things that we didn't realize ourselves. Why don't you come back to China and do some research on this topic?"

So I went back to the CSC-PRC with a research proposal, in conjunction with my next sabbatical, which was coming up in 1982-83. I received a grant to go back to China for three months to do research. I almost lost the opportunity because the award came just after the scandal in China in which Stephen Mosher, the Stanford anthropologist, got himself into trouble with China and, as a result, China decided they wouldn't allow any more social science researchers in the next year. But after quite a bit of negotiation, they agreed that I could come. And Alice and I spent three months in China in early 1983.

VDT: Was that perhaps because you were going to look at migration and not the sensitive topic of fertility--and abortion?

GOLDSTEIN: That may have been relevant, but it was also the nature of the research I was doing. It wasn't at that point going to involve doing sample surveys of any kind, but rather interviewing government officials and community leaders, contacts of that kind.

So 1983-83 was a very busy, interesting, successful sabbatical year. We started it at the East-West Center and from there traveled through parts of Southeast Asia, including Indonesia, Malaysia, and Thailand, focusing particularly on the volume, nature, and impact of circular migration. In all the countries, we collected information both in the cities and in the rural places of origin.

VDT: Just by talking to officials?

GOLDSTEIN: This was not by doing field surveys, but by talking to statisticians, policymakers, community leaders, and making observations.

VDT: You are well known for that kind of material that you use, which is not what some demographers would consider hard, solid data. What do you think about that?

GOLDSTEIN: Well, again, that grows out of my Norristown experience. While I have the highest regard for hard data, I feel they need to be supplemented by qualitative material in order to gain insight into what the data mean. I have my personal biases and one is that too heavy reliance on just quantitative statistics is dangerous. Again, you can see Dorothy Thomas's influence operating here. Although Dorothy was known by many as a staunch empiricist, the fact is that she relied not just on quantitative statistics but on a vast array of qualitative work, both in the early work she did on children and in the extensive work she did on Japanese-Americans who were displaced during the war. Her books on the Salvage and the Spoilage [Japaneses American Evacuation and Resettlement: The Spoilage, 1946, and The Salvage, 1952] and a third volume that was put together on cast histories bring together, I think, in a very effective way both quantitative and qualitative data. So I was very much influenced by that.

VDT: Do you think demography in particular has gotten away from that kind of study?

GOLDSTEIN: I think we've gotten away from it too much. And for that reason, I'm very pleased that Jack Caldwell, among others, has been trying to restore the balance. I'm very sympathetic to what he has been doing by stressing the qualitative aspect of demographic research.

VDT: His village observation?

GOLDSTEIN: Right. That's exactly what we were doing when we were traveling in Southeast Asia, and we repeated the process in China. In three months, we traveled somewhere between 12 and 15,000 miles around China; we were never in one location more than two weeks and usually less than that. Most of the time was spent interviewing local officials and residents in cities, towns, and villages. It was a wonderful experience.

VDT: You had it all set up in advance? It would have to be.

GOLDSTEIN: Yes, we arranged it before we went with the Chinese sponsor, which was the Chinese Academy of Social Science. They knew what we wanted to do; they had approved our research proposal. And they arranged for the itinerary, which involved making a grand circle of China, starting in Guangzhou, which was formerly Canton, and going through southwest China to Chengdu in Sichuan

and working our way through the northwest all the way up to Lanzhou, Gansu province, and then over to Shanghai and up to Beijing, and many, many places in between.

At every major city we went to, we tried as much as possible to get out into the countryside as well, to small towns and villages. So we were doing rural-urban site visits, with the opportunity to study situations both at migration destination and at origin and in the process get a feel for China itself, the kind of demographic problems it's facing, and how in particular migration fits into the picture and how it was being influenced.

And this was important. This was just the period when China had begun to introduce the economic reforms, basically what is referred to as the household responsibility system, which meant moving away from the commune system to a contract system, where individual households were allowed to produce as much as they could, meet their obligations to the government, and sell the rest on the market. This marked the beginning of the vast changes in rural areas, which eventually spilled over into urban areas. One of the impacts it has is that it freed up literally millions of rural Chinese laborers, because the system became much more effective, eliminating the inefficiencies of the earlier decades, when people were paid automatically on the basis of work points. Many people in the process became surplus labor, had nothing to do in agriculture, and were turning to either rural industry or opportunities that they saw in the towns and cities. Yet they couldn't move to the city officially because the migration control system was still operating. So in order to take advantage of urban opportunities, they had to move as temporary migrants. This meant they could stay in cities as long as they didn't become a burden on the urban structure. That basically meant if they were able to feed and house and work for themselves.

So they were channeled in certain directions. For example, many went into domestic work, because there they were fed and housed by their host families, as well as earning their living.

VDT: Alice said that you, as a result of your work, probably inspired more migration questions in the upcoming survey. Is there a survey of 1989, or was it the census that was going to be in 1989?

GOLDSTEIN: That was an interesting by-product. For reasons that I mentioned earlier, the Chinese claimed they knew everything they needed to know about migration. So during my lecture visit--I think it was 1981--I had contacts with the State Statistical Bureau. They were planning their 1982 census, and I was horrified to hear that they weren't including any question on migration. I urged them to do so but, again, they told me it wasn't necessary; they knew everything they needed to know from the registration.

Then as a result of this field research in 1983 and in subsequent years as well, the Chinese said, "You really have convinced us we have a problem and what we want you to do now is to come back and teach us how to do research on migration." They also began to realize they needed data. So what that 1983 research eventually led to was a national survey--it's called a national survey but it actually took place in 16 of the 29 Chinese provinces--in which they tried to collect basic migration data. The whole focus of the survey was on migration.

The survey was conducted in 1986, but the preparation took several years. It was, as I recall, 1984 that we went back and held the first workshop for them, under UN sponsorship, taught them how to do migration research, how to construct questionnaires, to sample, and tabulate data from the sampled populations. Then for a year, they did pretests and learned how to conduct the survey. And a year later, I went back and we had another workshop, along with Walter Mertens, who's now at Harvard, and we reviewed the results of that experience. That must have been early 1986. And during 1986, they actually initiated this national survey. And the trip I just took to China [December 1989] was for a conference at which the results of that research were reported. The whole conference was devoted to migration and urbanization. A good proportion of the papers drew their material from the

results of the survey. So that was a very satisfying experience.

What happened in addition was that in 1987, when the State Statistical Bureau undertook the equivalent of a mid-decade census in China--it wasn't a full census; it was a sample survey--they too incorporated questions on migration.

VDT: That was separate from the migration survey?

GOLDSTEIN: Yes, this was the official government equivalent of--a sort of grand Current Population Survey. They had questions on migration. Those data have been published. And on the basis of those results, they've decided now to include questions in the 1990 census. Just a moment ago, I took from my desk and put up there the draft questionnaire for the 1990 census. It now includes questions on migration.

VDT: Thanks to you!

GOLDSTEIN: Well, I won't take all the credit, but I won't deny that I had an influence.

VDT: Much more beyond questions of just lifetime migration and where were you living five years ago?

GOLDSTEIN: Well, census questions have to be restricted in number, so they're of that kind, and those are the kind of questions the UN recommends too. They go beyond it by asking for reasons for migration.

But in the 1986 migration survey, we had very detailed information on all kinds of mobility, sort of life histories of migration, so it's a very comprehensive set of data that will allow us to get at the traditional kinds of movement as well as temporary migration and commuting. And as I just reported at the conference in Beijing, I think the survey provides a solid model that a number of other developing countries could benefit from using.

VDT: It's the first time it's been used, this particular kind of survey that the Chinese had in 1986?

GOLDSTEIN: There have been forms of it. We haven't yet talked about my consultation work with ESCAP [Economic and Social Commission for Asia and the Pacific], the United Nations agency in Bangkok that covers Asia and the Pacific. I was involved in helping them develop a series of instruments for doing migration surveys in developing countries. All of this illustrates why I think it's true that as we approach the end of the 20th century the interest in migration has risen considerably, probably more in developing countries than even in the United States. In this ESCAP series there are about ten short monographs with questionnaires, tabulations, sampling designs, and so on for use in developing countries. And several countries have already carried out surveys based on those recommendations.

VDT: Did that work also grow out of your IUSSP work in the early 1970s?

GOLDSTEIN: Yes, one thing led into the other--a kind of chain reaction, and some of it goes on.

VDT: Your work in China has not been interrupted since Tiananmen Square? That didn't seem to cause any delay?

GOLDSTEIN: Interestingly, I can't really say that it's interfered in any serious way with our research. Some of our survey material, such as a large migration survey in Hubei province, was collected before that and obviously wasn't affected by the June [1989] events. But one of the surveys we're doing, which is in cooperation with the Chinese Population Information Research Center, was planned before June and I thought would certainly come to a halt, and yet they went ahead and carried it out in July and August as if nothing had happened in between. In fact, just a week before I left for China, in late November, the staff member of that center, who's just come back to Brown to finish his Ph.D., brought all the data with him, so we're now in the process of getting the analysis under way.

There are one or two other projects we're involved in and those have also gone ahead right on schedule. In October, we had a three-person delegation here from Beijing College of Economics for cooperation in analyzing registration statistics and survey data on the floating population in Beijing. And I was very encouraged when I attended the Beijing conference last week that there was a turnout-- I would estimate roughly 50 people--from various universities, research centers, throughout China. They were very enthusiastic about the research.

It doesn't mean there aren't problems. I think social science is facing a very dangerous period in China. There is a possibility the government may put some of the blame for the events of last summer on social scientists. Social science, particularly sociology, didn't fare very well in China for about three decades after 1949, so it's shaky in terms of its history. I'm hoping that the kind of research we and our Chinese colleagues and others in China are doing will demonstrate to the government that this is necessary, valuable information that can be used for very positive purposes in terms of raising the quality of life and that they will separate these activities from any of the political matters. That's a big order; maybe I'm unrealistic.

VDT: Well, it's encouraging so far. You said, though, that you believe most Chinese students now in the U.S. are unlikely to return.

GOLDSTEIN: At least in the immediate future. Judging by people I've talked to at other universities and by our own experience here, most have at least temporarily decided not to return.

VDT: How many do you have now in the program at Brown?

GOLDSTEIN: We have seven graduate students and one post-doctoral. And even those one or two who have completed since June have not returned and don't plan to return in the foreseeable future.

VDT: And you said it's such a waste to their country.

GOLDSTEIN: Well, I can understand their reluctance to return. At the same time I'm frustrated, and the people in China at the universities and institutions to which they are supposed to return are frustrated, because there's so much research to be done in China and they were counting on these individuals for this. Also a considerable amount of effort and money was invested in training them with the specific goal of their going back to use their talents.

As an aside, I think it's also fair to say that if there's any hope for China moving toward a "more democratic" arrangement, then it has to depend very heavily on individuals who have received advanced training to provide some of the leadership and if all those who are sympathetic to these kinds of changes stay out of the country, it's probably going to be that much more difficult to bring about the kinds of changes which they want to see happen in the country.

VDT: Let's now jump into your Jewish population research and publications. How do you get

everything done, because you always have many irons in the fire?

GOLDSTEIN: I often ask myself that too. I guess I learned early in my career to organize myself fairly effectively. It doesn't mean I don't operate under pressure a number of times, but I feel I've managed to have a reasonably good balance between professional activities and my personal life and my family commitments.

VDT: Where do you put your Jewish publications--halfway between your professional and your personal commitments?

GOLDSTEIN: I made a decision fairly early on--I guess it was somewhat different from some of my other classmates in graduate school, and again I often wonder, although I can't blame her for this, whether I didn't model it to some extent on Dorothy Thomas. I made the decision that, even though I'm interested particularly in migration and urbanization, I wasn't going to confine myself strictly to those particular topics or to any one population group. If you look at my vitae, as you noted, I started working on U.S. populations, to some extent concentrating on Rhode Island, then moved to Denmark, then to developing countries with a focus first on Thailand and then on China. I've written on fertility; I've written on illegitimacy . . .

VDT: I hadn't seen that particular topic.

GOLDSTEIN: It wasn't called illegitimacy but premarital conceptions, using Danish data ["Premarital Pregnancies in Denmark, 1950-1965," in Contributed Papers, Sydney Conference, August 21-25, 1967, IUSSP, 1967]. I did the study with Kurt Mayer on small business.

I think maybe again it grows somewhat out of my Norristown experience, where I was exposed at the training stage not only to different disciplines but also to a number of different research topics and learned about them in considerable detail, both with respect to methodology and theoretical and policy issues.

I am personally a committed Jew, and as a result of that I've been interested in what has come to be known as Jewish demography. Within the Jewish population there has been some concern about whether the demographic processes are operating in a negative way, because Jewish fertility has been quite low and intermarriage has been rising, and because of the obviously catastrophic impact of the Holocaust on population size in a number of countries. Migration has always been an important feature of Jewish life from the very beginning of biblical times, with the stories of Abraham and the Exodus from Egypt and refugee populations shifting from one country to another--my own wife Alice is a refugee. So whenever I work on demographic topics, in my mind at least I always have the question: How does this relate to the religious group of which I am a member?

The fact that the U.S. census does not include a question on religion means that in order for any religious group to know anything about itself demographically speaking it has to collect its own data or rely on insights gained from other surveys. Different groups do it in different ways. The Catholic group relies largely on their parish records. The Jewish group doesn't maintain that kind of record. Synagogue membership is not universal, so you can't rely on those kinds of lists. So increasingly since, say, the 1950s, Jewish communities have done their own surveys to try to identify Jewish populations, to understand their demographic behavior and how that impinges on Jewish identity, intermarriage, and a whole host of factors that have implications for survival as a religious group.

When they started working in those kinds of areas, they obviously looked for people who had a little expertise. Right here in Rhode Island, for example, in the early 1960s, the Jewish community wanted to do a survey of the Jewish population and they enlisted my assistance. That in a sense

marked the beginning of my active involvement in Jewish population studies, and that has continued ever since. If anything, it's become much more intense.

I did the 1963 Rhode Island survey. Calvin Goldscheider worked with me on it, for his dissertation. We coauthored a book for Prentice-Hall in their ethnic series [Jewish-Americans: Three Generations in a Jewish Community, 1968]. Following that, I became involved in other community surveys, because they saw the Providence one as a kind of model they should follow. I did another survey in Springfield [A Population Survey of the Greater Springfield Jewish Community, 1968]. We used the data from those surveys as a basis for our regular research and published in professional journals. I should mention that in 1987 we did a second survey in Rhode Island, so now we have two surveys to measure change over roughly a quarter century; the second survey encompassed the whole state [The Jewish Community of Rhode Island: A Social and Demographic Survey, with Calvin Goldscheider, 1987].

Gradually I became involved in national groups, because the same kinds of questions that arose on the local scene were being raised nationally as to what is happening to the Jewish population. I have come to be referred to as the "Dean" of Jewish demography in the U.S.

VDT: Obviously--you have to be. You and Calvin.

GOLDSTEIN: So now I serve as chair of the National Technical Advisory Committee of the Council of Jewish Federations, which is the nationwide organization of all local Jewish community federations. We're right now undertaking a national survey of the Jewish population. We're doing screening fieldwork as a basis for selecting our sample. The actual survey will go into the field roughly at the time of the U.S. census in 1990, so that we'll have the census with which to compare the results from the survey of the Jewish population.

VDT: What will the sample size be?

GOLDSTEIN: Well, it's a complicated sample design. We're planning a national sample of about 2,500 households. But in addition there will be samples in some of the major metropolitan areas that contain large Jewish populations, like New York, Chicago, and Los Angeles, and some in a few smaller communities. Those will be cosponsored by the local organizations.

And the last step, just to bring this to a close, there is a movement initiated by the population program of the Institute of Contemporary Jewry at the Hebrew University in Jerusalem to encourage a number of countries to undertake similar surveys around the world. So I am serving as codirector of the international committee that is trying to launch that endeavor.

VDT: That is another example of your tremendously far-reaching influence. That's great!

GOLDSTEIN: Just one general observation. Some people ask, "Don't you regret not having stuck with a sort of narrow focus on migration?" I've just found it's been terribly exciting to move from one area to another, because it's especially challenging to see whether the methods and ideas that have been developed in one area have relevance for another area. And in a sense, I'm working in two extremes at the same time. The Jewish population is very much concerned with the possibility of decline and with survival. And China, with over a billion people, is so much concerned with its size and rapid growth and trying to bring that growth under control. I jokingly tell my friends sometimes that the easy solution would be for some Chinese to become Jews and that may solve both situations! [Laughter]

VDT: That in a sense answers one of my next questions, which is, What accomplishments in your

career have given you the most satisfaction? That's one very good answer.

GOLDSTEIN: I think that you can see as we talked that I really have enjoyed my work throughout.

VDT: Obviously.

GOLDSTEIN: It's satisfying to see that one's research has some value for the places which one is studying.

But my work, as you know, encompasses not only research but training as well. And one of the most rewarding activities I've engaged in has been training and getting the satisfaction of seeing that training being put to good use by the students who take up posts both here in the United States and in their home countries, and then either seeing or getting letters from their employers or from other people telling about the good job they're doing and contributions they're making.

VDT: In this "pastoral care"--another direct legacy from Dorothy Thomas--how do you keep up with your students? You write back and forth, you visit them when you're there, and you mentioned at lunch some efforts to bring some back for refresher courses?

GOLDSTEIN: I rely very heavily on all these mechanisms. I've developed very close relations with a large percentage of the students who have passed through our program. A large number of them correspond very frequently with me. Certainly a week doesn't go by that I don't receive some letters from former students; sometimes almost every day.

VDT: Do you have regular reunions of some kind?

GOLDSTEIN: We've had one back here at Brown and we do have annual reunions at PAA. But so many of the students are scattered around the world that it's difficult to bring them all together at one time. Someone said jokingly, "Sailors are supposed to have a girl in every port; Sid Goldstein has a student in every port." It's not quite true, but it's true that as I travel around the world in connection with my other work, it's quite rare that I don't meet one or more of my former students.

VDT: You say now there are about 60 percent of students from less developed countries to 40 percent U.S. in the program. What's the ratio among the hundred Ph.D.s you have had up to about a year ago?

GOLDSTEIN: I would say roughly a half were from outside the United States.

VDT: I usually ask people about some of their leading students. Do you dare to list some off the top of your head?

GOLDSTEIN: That's very difficult to do, because each makes his or her own kind of contribution. It's very difficult to put them on a scale. A few examples, but by no means an exhaustive list or even all the top ones.

Ron Lesthaeghe is certainly one of our top graduates. He received his master's degree here, was called back to Belgium to go in the army, so he completed his Ph.D. while in Belgium. I mentioned earlier Dr. Kono in Japan. Apichat Chamratrithirong in Thailand has an international reputation, as do a number of others in Thailand. And in the U.S., we have alumni at the UN, at the Bureau of the Census, the National Center for Health Statistics, obviously a number of universities and research centers. I'll not dare to identify individuals among them.

VDT: Rand.

GOLDSTEIN: At Rand Corporation, or even in the business world. You look and you'll see a Brown alumnus.

VDT: That's great. On influences, well, the leading influences on your career have come out as you've talked along. Dorothy Thomas's name has come up time and again, and the Norristown study. Would you rate her as the leading influence on your career--Dorothy Thomas? Any others?

GOLDSTEIN: As I said, it really started with Bob Burnright, who was himself influenced by Dorothy Thomas. Bob was originally my teacher, but eventually he came here to Brown as a faculty member so we had the opportunity later on to work as colleagues. He didn't compile a very long publications list, so from that point of view his contribution maybe was more limited, but like Dorothy, he was a great person as a teacher and a man with ideas. I learned very much from working with him, both as a student and as a colleague.

I mentioned also in connection with the Norristown seminar not only Dorothy but also Tom Cochran, who as a historian had great impact in making me recognize the importance of seeing things in historical perspective, which has had great use in my work in Thailand and China and on the Jewish population. And here at Brown, I've been fortunate in having good colleagues with whom I've enjoyed working and from whom I learned much.

VDT: And Alice?--her historical perspective?

GOLDSTEIN: Well, working with her--she combines in one person the historical and the demographic perspective.

VDT: And she said she felt she had made you a little more interested in women, as she herself became interested in women's issues that might not have captured your interest as soon.

GOLDSTEIN: That's very true. Just working with her and seeing her develop as a demographer has made me very aware of the whole women's issues question. I've marveled at how she has managed to combine her various roles as a wife, mother, and professional in very successful fashion and made major adjustments in becoming a professional.

Obviously, I was very pleased when she decided not only to go back to graduate school but to go back and to mine her earlier international training in history in terms of historical demography. I've been even more pleased that we work so well together. As you know from both our vitae, there's been an increasing number of publications that we have coauthored. And I guess it's fair to say that almost--I won't even say almost--every publication that I have prepared and I think it's true that those she authored independently has benefited from some input from the other one of us, so that we make a very good team. It's been nice that we've been able to work together not just here at the university. By having these common interests, when we go on sabbaticals we're still working on the same projects. So it's been an ideal arrangement. I recommend this highly.

VDT: Great! What do you see as leading issues in demography over the years you've been involved? Now, obviously migration has loomed very large. Have you really not been too concerned about international migration?

GOLDSTEIN: I have been, in the sense that in my work on the U.S. obviously international migration is a major consideration and particularly so in the Jewish population. So much of the historical growth of the Jewish population of the U.S. is due to international movement, so that has entered into my research in that way.

However, in the countries in which I've worked on abroad, particularly Thailand and China, there's very little movement into those countries, at least in the period in which I've been working. There was earlier movement of Chinese and some Indians into Thailand, but for the most part international migration was controlled in the periods on which my own research focused. And in China, while there was heavier outmigration in earlier decades and there may be some renewal of that now, for the most part it doesn't enter into the research problems on which I now focus.

VDT: What is your feeling about the slowdown in the decline of the world population growth rate? In your 1976 PAA presidential address [delivered April 30, 1976], you pointed out that world population had passed the 4 billion mark since the previous year's PAA meeting. Incidentally, it was I who drew attention to that. The Population Reference Bureau was putting out its 1976 World Population Data Sheet and I was out jogging one morning and it suddenly occurred to me that we were going to pass the 4 billion mark. So I went to Paul Myers, who was doing the calculations for our data sheet that year, and said, "Figure out when." He came up with Sunday, March 28th, 1976. We put out a press release and it hit the whole world. Of course, later the UN decided that it had happened in 1975.

But, okay, we'd passed it, and you said in another 13 years, it would probably pass 5 billion-- well, you did say within another 13 years. That would have been 1989, but, of course, the UN has estimated that it happened last year, 1988. And as you know, the UN has revised their projections upward. Does that discourage you? Or do you not worry about the world population growth rate?

GOLDSTEIN: Well, obviously, it's very worrying. It's not something on which I directly focus my own research, but both as a demographer and as a human being, I'm concerned with what implications that has for the quality of life. I'm very much involved here at Brown, for example, in our hunger program, which is one of the other center activities. A good part of my input to that particular program is from a demographic perspective, what the implications of growing populations are for hunger and poverty, especially now in Africa. And I think the research I'm doing on migration and urbanization relates very closely to, or is at least a by-product of, the rate of growth of the world as a whole and particularly the 90 percent of that growth which is in developing countries. China with its billion people has already added about 100 million people since the 1982 census. So even though it's controlling its fertility much better than it did formerly, it's still making a considerable contribution to world population growth. So while this may not come across as being at the center of my concern, it's certainly enters very much into the picture through the pressures this growth creates for movement from rural to urban places and for rapid urban growth.

VDT: What about the future for U.S. demographers? Do you feel there's still a place for basic research in academia and ties with the Third World, as you have had, or are the jobs now mainly for applied demographers?

GOLDSTEIN: Judging by where we place our graduates, certainly there's a growing demand for applied demographers. More, at least over the more recent period, have been going into government activities and national agencies and some even into the business world. And I suspect the demand will grow, probably more in the business world than in government, particularly if government cuts back in funding. But I don't think that this in any way means that the need for demographers in universities is going to decline. We still have the pressing need for basic research, and I think the best place for basic research is in a university setting.

And obviously there's the need for training future demographers, not only for work in universities but for all these applied activities. My own philosophy has always been that the best training--even in applied demography--is in a solid demography training program, that if one receives a good solid core training then one can go out and do virtually anything. That's been one of the satisfactions I've had, seeing some of our graduates shift from the work in the university to work in the Census Bureau, and sometimes even from there or something like the Census Bureau to private industry. And the fact that they can be successful in all these attests to the quality of their basic training and the flexibility it allows in careers. If they get the solid training, then they can apply it in different ways, whether it's in basic methods, theory, or applied work.

I hold the same philosophy, and we've followed it here at Brown, that we shouldn't train people to work in a particular area of the world. Even though we have had students from China and Thailand in disproportionate numbers over the years, we don't really train them specifically to work in China or Thailand or train American students for particular areas. They can write their dissertations on particular areas, and usually do, but we feel they should get a solid basic training, so that if for five years they're working in Africa and the next five years in the U.S., they are more or less equally qualified to work in both areas. I think that's where the university has the role to play.

VDT: That's a great answer. Now a little on PAA. You remember that you gave your first paper at a PAA meeting at Princeton in 1952. That was on "Patterns of Internal Migration." You were speaking to the bigshots and you were a bit intimidated. Was that also your first meeting? 1951 was at Chapel Hill, did you go to that one?

GOLDSTEIN: No. I started attending meetings back in 1950, at Princeton. That was while I was still working on my master's degree at Connecticut.

VDT: And your memories of the first meetings? You said everyone met in one room, and Alice in particular stressed the give-and-take that was possible; many people have talked about that.

GOLDSTEIN: Particularly as a graduate student, it was a thrill. And I still, as a result, make an effort even today at PAA meetings to introduce graduate students to the leaders of the field. But in those days it was much easier; we were all together.

VDT: You make that effort still? You're not intimidated by the large numbers?

GOLDSTEIN: No. I think it's exciting for graduate students to see and meet in the flesh the people whose works they've been reading and using.

VDT: And they need someone to introduce them. When I interviewed Ron Lee, he said, "At the last meeting I happened to go in the elevator which a guy who had on his tag 'Henry Shryock.'" And I said, "I hope you introduced yourself." No way. Ron Lee was too shy to introduce himself to Henry Shryock! It needs someone to make the introduction, I guess.

GOLDSTEIN: I guess most people feel reluctant to just walk up and say, "Hi, I'm so-and-so." Particularly if there's a big status gap between them.

VDT: There's not that much there.

GOLDSTEIN: No, in that case there wasn't, certainly, but in the case of graduate students . . .

VDT: But you made an effort to do that?

GOLDSTEIN: Again, I credit Dorothy Thomas with that, because, as I said, I had wonderful opportunities at Pennsylvania. There were a number of top people not only of the U.S. but Gunnar Myrdal and others who came from overseas and were her guests and she always made a point of giving students a chance to know and spend time with them. I've learned much and gotten much satisfaction out of that and as much as possible would like to give those same opportunities to my own students.

VDT: You must have been PAA president for a year and a half. Your year was 1975-76 and it switched to the calendar year with Evelyn Kitagawa, who followed you in 1977.

GOLDSTEIN: I don't know if it's true but someone has told me that I may have been the longest-serving PAA president, at least in recent decades. Some early presidents served a couple of terms?

VDT: Yes. Lowell Reed served during the war, 1942 to 1945, and Henry Pratt Fairchild, the very first, was 1931 to 1935. But you served longest in the modern era?

GOLDSTEIN: In the modern era. When they made that transition from beginning the term at the annual meeting, Charlie Westoff [president 1974-75] and I decided the most reasonable way to handle it was to split it up, but I got a little more of it than he did. I took over in September 1975 and went to December 31, 1976.

VDT: That also was the time when the responsibility for the meeting program was switched from the first vice-president to the president; they usually have to start on it the year before. So Evelyn Kitagawa was supposed to do it for 1977, but she had done it as first vice-president two years earlier, so she had Ren Farley do it in her year.

GOLDSTEIN: I had also already arranged the program when I was first vice-president. That was for the New York meeting in 1974. Then I was president in 1976 and at that point the responsibility had switched to the president. So I appointed Charlie Nam to organize the program, because otherwise I would have been doing it two years apart.

VDT: Just as Evelyn had Ren Farley do it for her. Why did you choose Charlie Nam? Nice guy, very efficient, but why did you choose him?

GOLDSTEIN: Well, ruling out all those who had been vice-president, there was a limited pool and among those I thought his interests reflected mine more closely than other people's. Also, I'd had contacts with him before, so it was a matter of having an appointee with whom I felt comfortable working. We had not coauthored anything, but we had come to know each other over the years.

VDT: What do you remember about particular issues about that time, say, from the New York meeting when you were responsible for the program to your presidential meeting in Montreal? The Women's Caucus, for instance, was an issue at that time.

GOLDSTEIN: The women's caucus was being developed through that whole period of time. Eventually--in fact during my term as president--it sort of culminated in the sense that there had been the Committee on Discrimination, which was directed heavily at professional women's issues. They

had completed several surveys to find out whether women and others were being discriminated against; the evidence was there in the report. Then when I became president, one of the new committees I established was the Committee on Participation. The idea behind it was to build on the information collected by the Committee on Discrimination and find ways to enhance the participation of PAA members, both women and minority group members, in the activities of the Association. Jose Hernandez headed that.

VDT: I should tell you I interviewed Mary Grace Kovar a couple of weeks ago. She was secretary-treasurer [1975-78] during your time.

GOLDSTEIN: A great secretary-treasurer.

VDT: And she says that about you--a great president. One incident she particularly remembered happened the year before, in 1975, at the meeting in Seattle. She arrived late and although she had a reservation, there was no room immediately available for her. And you spoke up immediately and said, "This lady is to be secretary-treasurer of this organization next year, so please find her a room." Which they promptly did. She went up to you and said, well, of course she was extremely grateful; she wouldn't have dared pull rank. And you said, "Of course not; that's why I did it." She cited that as an example of your people-thoughtfulness, for which you're so well known.

GOLDSTEIN: Well, during that whole period there were a number of incidents where not so much women but minority group members were denied registration at hotels and so on.

VDT: Even so late as the 1970s?

GOLDSTEIN: Certainly in the 1970s, yes.

VDT: I've heard stories of that happening at Atlantic City in 1942 and Chapel Hill in 1951, but . . .

GOLDSTEIN: I was glancing through PAA Affairs in the 1970s to remind myself of things that went on and there's one mention in there where registration was denied . . .

VDT: Goodness, in one of the places where we met, in the early 1970s?

GOLDSTEIN: So that's why at some point, I don't remember the exact year, we adopted that resolution that we would not hold the meetings in any place that discriminated in any way on the basis of sex, race, or religion.

VDT: What do you remember about Montreal, your year? We've talked somewhat about your address.

GOLDSTEIN: The Montreal meetings, obviously, were very satisfying for me, both in the fact that I was serving as president and giving the presidential address, being able in that way to highlight the importance of migration in the field since I did feel it was still being treated somewhat as a stepchild. The chance to present it and argue for its importance was particularly welcome and I was very satisfied by the favorable reception to the presidential address--the fact that it's been cited so much since that time. Obviously, the ceremony in which the alumni stood up . . .

VDT: The Brown alumni, of course! I had forgotten that.

GOLDSTEIN: That was a particularly satisfying experience too. I guess that was in a way unique, I don't recall another such occasion.

VDT: What did they do? I was there, but refresh my memory.

GOLDSTEIN: Peter Morrison came to the podium--it was a complete surprise; I hadn't the vaguest indication that this was going to happen--he came to the podium and presented me with a Steuben piece from all Brown alumni in honor of my being president.

VDT: Oh, right. Peter Morrison was my author right about then; we were working on his Population Bulletin, "Rural Renaissance in America" [October 1976].

GOLDSTEIN: And then he asked everybody in the audience who had been trained by me to stand up and it looked like half the audience stood up.

VDT: Absolutely!

GOLDSTEIN: So that was satisfying. I was pleased by the meetings, because even though Charlie Nam was the program director, we had worked out . . . One of the goals of my presidency was to try to open up the meetings. I had done this survey after I finished my term as first vice-president of the membership's attitudes toward meetings.

VDT: I never knew about that one.

GOLDSTEIN: It was reported in PAA Affairs; one whole issue is devoted to just that survey. That was probably in 1975 ["Survey on Annual Meeting," PAA Affairs, Spring 1975]. There's also, by the way, a report of my presidency in PAA Affairs. I just found it yesterday. I think I'm the only one that's done it. It's required by the constitution.

VDT: And no one else has done it? What issue is that?

GOLDSTEIN: That would have been after 1976 ["Report of President Sidney Goldstein," PAA Affairs, Spring 1977]. It's about my whole term as president, three and a half columns, so maybe we don't have to give all the details here.

One of the things we did at the Montreal meeting was to try to open up the sessions. That was the time, for example, we introduced the luncheon roundtable discussions and we had poster sessions for the first time. Poster sessions didn't last.

VDT: They came back again.

GOLDSTEIN: But the roundtables really caught on. That was also the time we introduced the idea of publishing the abstracts beforehand.

VDT: That little booklet, you didn't have that before?

GOLDSTEIN: No. We also had a new type of discussion panel, instead of just papers. All of this was intended to broaden the participation in the meetings.

In the same way, back in the 1974 meetings which I organized in New York--1974 was World

Population Year--I think for the first time, we gave a strong international focus to the contents and I had Paul Demeny take charge of that. We actually received a grant to bring people from overseas to participate in the meetings and there was an impressive number of people invited from overseas, including some very distinguished ones. Tinbergen, a Nobel Prize winner, was one. There was Lopez from Latin America, Ado from Africa--there were about ten people, I think, whom we funded to bring to the meetings.

Another innovation during my presidency was a new committee for liaison with other professional organizations [Committee on Relationships with Other Organizations]. Again, I felt that the PAA was too provincial. Parker Mauldin headed that. That committee set up a whole set of guidelines for relating PAA to other professional organizations. It was understood in the beginning that it would go out of business once the guidelines were adopted, as they were.

The other major new opening was that I created--and this was even before I went to China--I created a Committee on China Study and Exchange.

VDT: You started that! I tried to get Susan Greenhalgh [current chair] to write a piece in PAA Affairs about what's going on with that committee now.

GOLDSTEIN: Yuan Tien at Ohio State headed the committee initially. One of the first things he did was to arrange to have copies of Demography sent to China. That was a big breakthrough. He also organized a tour to China. It ended up being just a tour and not a full set of professional contacts; the Chinese weren't ready for that yet.

VDT: What happened to that committee? I understand it sort of faded away.

GOLDSTEIN: It faded away for a while and then it was reactivated, as you know.

VDT: I didn't realize you were responsible for so many innovations. Some have faded away, such as the president's report of what he did during his term.

GOLDSTEIN: I don't know if they did or not; I haven't checked.

VDT: No, never have.

GOLDSTEIN: Either that, or people just ignore it. I guess I was more conscious of it because I was the first president under the new constitution [July 1974] and I assumed it would set a kind of model, so I took it seriously. I don't know if it was even done by Evelyn.

VDT: No, it wasn't.

GOLDSTEIN: But I started out by saying, "The new Constitution of the PAA requires the outgoing President to report to the membership on the activities of the Association [during the preceding year]."

VDT: What do you think of PAA now? You say the meetings have grown very large, but you still make an effort to get around and meet everybody. You attend them all--when you're in the country.

GOLDSTEIN: Whenever I'm in the country.

VDT: Do you still enjoy them?

GOLDSTEIN: I enjoy them, but I guess I would be dishonest if I didn't say that I don't enjoy them as much as I used to, because they're so large and, obviously, as I age, I know fewer and fewer of the people there. And it's sad, of course, to come and find that some of the oldtimers are no longer around. But professionally I think the meetings are as stimulating as ever. It's frustrating now because there are just so many sessions going on concurrently. But on the other hand, that's testimony to the way in which the profession has grown, so we have to look at it positively and not just in terms of frustration.

VDT: And last year [1989 meeting in Baltimore] there were so many--there have been many workshop spinoffs--but particularly the Chinese were very much in evidence. Did you have a lot to do with that?

GOLDSTEIN: Not that much. I think Susan Greenhalgh through her committee arranged that. I was involved tangentially with a few people. I was particularly pleased with that, both in terms of my work in China and, as I said, I thought the PAA was somewhat provincial and made a concerted effort to internationalize it more back in 1974. I think that's caught on--obviously in people's interests and work--and I think we should do more of that kind of thing.

VDT: Do you think perhaps we're still a bit too provincial within the discipline? There are only about 2,600 or 2,700 members [2,679, end 1989; 2,752, end 1990]. It's fluctuated there since the mid-1970s, which is small by comparison with most professional organizations.

GOLDSTEIN: Well, during my various responsibilities in PAA from vice-president to president-elect and president, one movement was this greater international focus. The other one--again reflecting my Norristown influence and I tried this in 1974 and also in cooperation with Charlie Nam in 1976--was to get more involvement of people working on population from various disciplinary perspectives. If you look at the programs, you'll see there were sessions in terms of psychology, geography, anthropology, and so on. I thought of "provincial" as being isolated in that sense too, to the extent that so much of population is concentrated in sociology. There are many people working out there who were not members of PAA at the time and I thought we ought to try to get them into the mainstream.

VDT: You did that consciously for the New York meeting?

GOLDSTEIN: Right, so that we could benefit by their participation and also they would benefit by more contact with mainstream demographers. It doesn't always succeed; it depends on continuing reinforcement and encouragement. One leading scholar comes to mind who, I remember, was invited to participate in the 1974 meeting, had a very positive reaction to it, and that experience led him to more involvement in population research in later years.

VDT: This opening up--that has or has not carried on in PAA meeting programs? The geographers are there now.

GOLDSTEIN: I think it's had an impact. I think there's room for more concerted effort in that direction, because I still have the feeling there are many people out there who are working on demographic problems who are not PAA members. It's in the interest of the profession and the discipline to get them into PAA, get them exposed to other demographers, get more interaction going.

During my term there were the usual concerns about funding, because government funding was tightening up. Up to that time we had a number of surplus years, but it was expensive to fund

Demography and Population Index. It wasn't that funds were short immediately, but the prospect of their becoming short was beginning to get serious, especially the possibilities of outside funding for PAA.

VDT: You know that right now PAA is having to switch from the American Statistical Association.

GOLDSTEIN: That was another thing we did during Charlie Westoff's and my terms. We took a very hard look at the whole business office relation, and that's when we made the firm decision to continue with the ASA. So I was pleased to note that that's lasted a good 13 years; it seemed to work. But now I gather they're going to have to review the situation.

VDT: Not just review; ASA has asked them to leave. Barbara Bailer has said that they can't carry it on.

GOLDSTEIN: I see. Well, we had a very thorough review at that time. Jim Brackett [secretary-treasurer 1971-75] initiated it, and the end result was a strong recommendation to stick with ASA.

Another innovation during my term was the initiation of the Irene Taeuber Award [for Excellence in Demographic Research].

And even though it had started earlier--it was one of those activities that had faded out--Charlie and I reactivated the Committee on Population Statistics, COPS, which has come to play a very crucial role, I think, in the profession. I appointed Jeanne Ridley as chair--actually, Charlie officially appointed her, but it was in anticipation of my taking over as president. That became a very active committee and has been so ever since then. It's very much helped to enhance the quality of demographic statistics. Some of its responsibilities have since been taken over by other groups, such as the Political Action Committee, but COPS at that time was also our liaison to Congress and they testified before congressional committees. That marked the beginning of that kind of interaction with Congress with respect to the census.

During my term we also joined the Committee on Government Statistics, COGS, which was a joint committee with a number of professional organizations, such as the American Statistical Association, American Sociological Association, and several others, to represent the professional groups in relations with government agencies responsible for statistics, particularly to try to keep politics out of statistics. Con Taeuber was our representative on that. This was not the Federal Statistics Users' Conference. This was more a pressure group. Beyond the activities of COPS and COGS, the PAA's interests in government statistics were also furthered by the arrangement developed [in the late 1960s] for PAA appointments to the Technical Advisory Committee of the Bureau of the Census.

So it was a really active period. I saw my presidency as a transition period in which major new changes were introduced in response, shall we say, to the sense that the profession had come of age and there was a need for us to get much more involved both internationally and nationally outside of annual meetings and publication. We came to recognize that there were many things going on outside which related to the work we were doing and to which, therefore, we needed to relate.

VDT: I think you've left a wonderful legacy. That's a wonderful place to end. There are many demands on your time. You think you've managed to get through all the notes you've made there on your presidency?

GOLDSTEIN: I hope so. I looked through some PAA Affairs just to remind myself. It's amazing what one forgets.

VDT: But you've had so many other things to keep in your mind. You must have enjoyed your career, and it's far from over.

GOLDSTEIN: Well, I don't know how close it is to the end. People keep asking me when I'm going to retire and . . .

VDT: They keep asking you to do more things.

GOLDSTEIN: Right. I really haven't made up my mind yet and I enjoy it. So as long as I keep enjoying it and feel I can continue making contributions, I'll continue in one way or another.